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TRANSACTIONS

OF THE

North-East Coast Institution

OF

Engineers and Shipbuilders.

EDITED BY

E. W. FRASER-SMITH, M.A., A.M.INST.C.E., *Secretary*.

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OF

ENGINEERS AND SHIPBUILDERS

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TRANSACTIONS.

VOL. XXXIX.

THIRTY-NINTH SESSION, 1922-1923.

EDITED BY

E. W. FRASER SMITH, M.A., A.M.INST.C.E., SECRETARY.

NEWCASTLE-UPON-TYNE:
PUBLISHED BY THE INSTITUTION, BOLBEC HALL.
1923.

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ERRATUM.

Page 218. Third paragraph, line 2:—

For "Company" *read* "oil,"

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THE NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS.

FOUNDED 1884. INCORPORATED 1914.

Patron:

HIS MAJESTY THE KING.

Past-Presidents:

1884-86 *William Boyd, J.P.*

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1888-90 *F. C. Marshall.*

1890-92 *John Wigham Richardson, J.P.*

1892-94 *Robert Thompson, J.P.*

1894-96 *Sir Thomas Richardson, B.A., J.P*

1896-98 *Col. Henry F. Swan, C.B., J.P.*

1898-00 *Sir Benjamin Chapman Browne, Kt., D.C.L., J.P., M.Inst.C.E.*

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1902-04 *John Tweedy, J.P.*

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1908-10 SUMMERS HUNTER, C.B.E., M.Inst.C.E.

1910-12 *Colonel Richard Saxton White, V.D., J.P., M.Inst.C.E.*

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1915-17 *Sir Herbert Babington Rowell, K.B.E., M.Inst.C.E.*

1917-19 *Edwin Lancelot Orde.*

1919-21 A. ERNEST DOXFORD, M.A.

1921-22 SIR WILLIAM JOSEPH NOBLE, BART.

The names of those deceased are printed in italics

COUNCIL AND OFFICERS.

THIRTY-NINTH SESSION, 1922-1923.

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Sir ARCHIBALD C. ROSS, K.B.E.

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The Past-Presidents.

Vice-Presidents:

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C. WALDIE CAIRNS, M.Sc.
R. S. DALGLIESH.
Major-Gen. Sir PERCY GIRCUARD, K.C.M.G., D.S.O.
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Prof. J. J. WELCH, D.Sc., M.Inst.C.E.

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G. TRISTRAM EDWARDS.
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ROBERT HOPE WINSTANLEY.

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1922-1923.

Advisory Committee for the William Froude National Tank:
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THE PRESIDENT.

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Col. D. R. MACDONALD. D. MYLES.

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G. M. HARROWAY. F. T. DICKINSON.

Council of Armstrong College:
THE PRESIDENT.

Governing Body of The Marine School, South Shields:
D. W. FITZGERALD.

Selective Committee of the Ministry of Labour Appointments Department:
D. MYLES. SIR ARCHIBALD C. ROSS, K.B.E.

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THE SECRETARY.

Electrical Engineering Department Committee, Armstrong College:
R. P. SLOAN, C.B.E.

Gateshead Education Committee, Joint Advisory Committee:
A. P. PYNE.

British Engineering Standards Association Committees:
Committee on Ships' Details, North East Coast Institution Panels:
COL. D. R. MACDONALD, Chairman.

Sectional Committee on Pipe Threads:
ANDREW LAING, C.B.E.

Committee on Standardization of Gears:
STANLEY S. COOK.

Sub-Committee on Steel Flanges for Hydraulic Pressure:
C. O. RIDLEY.

Sectional Committee on Petroleum for Engineering Purposes:
TOM WESTGARTH.

Committee on Ships Machinery Details:
F. T. DICKINSON. SUMMERS HUNTER, C.B.E.
ANDREW LAING, C.B.E. D. B. MORISON
(*Panel 12, Advisory Committee*: C. WALDIE CAIRNS, M.Sc.)

Committee on Tabulating the results of Steam Engine and Boiler Trials (Institution of Civil Engineers):
THE PRESIDENT. S. S. COOK, M.A.

Northern Counties Technical Examinations Board:
THE PRESIDENT. THE SECRETARY.

NORTH-EAST COAST INSTITUTION

OF

ENGINEERS AND SHIPBUILDERS.

THIRTY-NINTH SESSION, 1922-1923.

PROCEEDINGS.

The Thirty-ninth Annual General Meeting of the Institution was held in the Lecture Theatre of the Literary and Philosophical Society, Newcastle-up-Tyne, on Friday, the 20th October, 1922, at 7.30 p.m.

SIR WILLIAM J. NOBLE, BART., Retiring President, in the Chair.

The notice convening the Meeting was taken as read, as well as the Minutes of the last General Meeting, which the Chairman was authorized to sign.

The following resolution was adopted :—

> "That the Institution place on record its deep sorrow over the loss of Col. R. Saxton White, V.D., J.P., M.Inst.C.E., Past-President and Fellow, and their high appreciation of the unsurpassed interest that he manifested in the work of the Institution."

The following candidates for membership were unanimously elected :—

Members.

ROBERT CHARLTON WHELDON BELL.
AMOS HAYTON COWAP.
HERBERT ALFRED HUMPHREY.
WILLIAM STANLEY SMITH.
THOMAS DALRYMPLE STRAKER-SMITH.
OSWALD WANS.

Transfers to Member.

ERNEST CAMERON BRUNTON.
ERROL HAY THOMPSON.

Associate Member.

JOHN CAMPION GUNN.

Transfers from Graduate to Associate Member.

SIDNEY JAMES ADAMS.
ALFRED WILLIAM EMMERSON.
DAVID MYLES.
JAMES STUART OLIVER.
THOMAS SPENCE.

Associate.

THOMAS EDWARD THIRLAWAY.

Graduates.

RICHARD NICHOLSON ANDERSON.
DONALD ARMSTRONG.
ROBERT FENWICK BATTEY.
PHILIP GORDON HUDSON.
WILLIAM GORDON HUNTER.
ALFRED OXLEY.
ALFRED PEARSON.
CLAUS THOMAS PETERSEN.
JOHN MIDDLETON THOMPSON.
JOHN EDWARD RUDOLPH WILLIAMSON.

The Honorary Treasurer, Mr. R. H. Winstanley, submitted and explained the Income and Expenditure Account, Balance Sheet and other accounts.

Mr. Harold Thomson reviewed the leading features of the Council Report for 1921-2 (see page 13), copies of which had been supplied to the audience, and on his motion, seconded by Mr. B. C. Browne, it was

Resolved.—That the Report of the Council, Balance Sheet and Accounts for Session 1921-2, be adopted, and that they be printed in the *Transactions*.

On the motion of Mr. W. J. Paulin, seconded by Mr. H. A. J. Rang, it was

Resolved.—That the cordial thanks of the Institution be given to Mr. Winstanley, the Honorary Treasurer, for his conduct of the financial business of the Institution during the 1921-2 Session.

Sir William Noble installed Sir Archibald C. Ross, K.B.E., as President of the Institution for Session 1922-3.

From this point onwards the proceedings were conducted under the Chairmanship of

Sir ARCHIBALD C. ROSS, K.B.E., M.Inst. C.E.

Mr. Summers Hunter, *Past-President*, said they had that night witnessed the inauguration of a new session and at the same time the somewhat pathetic scene of the closing of the past session. And yet that past session need not be pathetic but rather a cause for congratulation, and those who had not read the Past-President's Address since he delivered it a year ago would be well advised to read it again. He gave that advice because Sir William's Address had indicated what they might expect of him and they had not been disappointed. His year of office had been an outstanding success: such an Address made it a foregone conclusion. Representing as he did the whole shipping industry, he was in a position to deal with it very fully at the time when the industry was under a cloud of depression such as they had never experienced before. At that time the future might be said to have been full of black uncertainty. To-day the position was certainly more hopeful. Many of the difficulties with which they were then confronted—economic and financial—if not actually overcome, had very nearly disappeared. The Presidential Address of a year ago indicated the desire of Sir William Noble not only to give the Institution the benefit of his long experience and very active life in the shipping industry, but also to advise and suggest as far as he could remedies for the maladies from which the industry was suffering. Sir William had analysed closely the causes of the depression; he had arrived at conclusions and had pointed out the remedies. His diagnoses had been absolutely true and many of the remedies which he indicated had actually been applied, and they were in a better position that day than they had been a year previously. It was difficult to estimate the extent to which the shipping industry, both locally and nationally, was really indebted to Sir William for his wise guidance on many occasions, not only in the Institution but in other organizations where his opinions were always appreciated and his advice or suggestions acted upon.

They could not measure what Sir William had done for the Institution simply by his work—the fact of presiding at general meetings was no indication of the work of the President. There were the Council meetings and Committee meetings and the many details which had to be carried out by the President and the Secretary and Treasurer; and on no occasion had their late President failed to give his best assistance and help to those engaged in the Institution's affairs. He did not think the Institution had ever had a President who took a closer and

more thoughtful interest in all that came before him than Sir William Noble had done during his year of office. They were only sorry that the many claims upon his time had prevented him from continuing in office for another year; but he had indicated a strong desire to assist in the Council and in other ways in which he might be useful. He was sure they would all agree that they had been honoured by having had such a President, and in future he hoped they might find others identified with the shipping industry who would follow his example and take a leading part in the work of the Institution.

Mr. Hunter then moved:—

"That the members present at this meeting desire, on behalf of themselves and others, to record their high appreciation of the services rendered to the Institution by Sir William J. Noble during his term of office as President."

Mr. John T. Batey, *Vice-President*, said the duty which he had to perform was a very pleasant one—to second the vote of thanks which Mr. Summers Hunter had just proposed and which he was sure the meeting would receive with great pleasure. Mr. Summers Hunter had pretty well covered all the duties which their President had performed, and as far as he (Mr. Batey) was concerned, he could say that he never sat under a President who had conducted that and other meetings with greater ability. Sir William had had an awkward team to drive; he had begun by telling them that he was not technical and would not be able to drive them properly, but the fact of his not being so technical as some of them had proved to be his strength and he had driven the team with marked ability; in fact, if any members of the team were inclined to go off at a tangent it was delightful to see him pull that person up and keep him to the point. If their President had remained in the back seat to which he said he had retired now, the Institution would certainly have been the loser, and the members of the Council and the members of the Institution would also certainly have been the losers. They had had the benefit of his experience as a shipowner, a name to which Mr. Batey believed he objected. He had brought into their counsels ideas which would certainly assist them as shipbuilders and engineers in the future. When the time came for vessels to be ordered, and they approached Sir William and put their proposals before him, they had not the slightest doubt that they would go to one who was in deep sympathy with them in their troubles and in deep sympathy with them in their endeavours to produce an article which would enable him and his fellow shipowners to carry on the industry and to maintain the prestige of their country. He had the greatest possible pleasure in seconding the vote so ably proposed by Mr. Summers Hunter.

The resolution was carried unanimously and with acclamation.

Sir William J. Noble, who was received with applause, in acknowledging the vote of thanks, said that the Institution was not a mutual admiration society though he could well imagine those members present thinking so. They started by acknowledging the good qualities of their President-elect who had replied by placing upon him the responsibility of standing before them again. It was somewhat embarrassing to sit and listen to such remarks as he had had to listen to because one was so fully conscious of the shortcomings in the performance of one's duties in a position of that kind. He had regarded it at the time as a very great honour—possibly the greatest honour that had been conferred upon him in the north of England—when he was asked to be their President. He had undertaken the duties with fear and trepidation for the reason which Mr. Batey had referred to, that this was a technical Institution, of which the ordinary mere manager of ships knew nothing; but the ordinary manager of ships perhaps learned one thing, and that was that when he knew nothing to say nothing, and in that respect he had been successful in driving that very "awkward team" which they had appointed as their Council. He had the great wisdom to hold his tongue when he could not say anything useful.

It had been a very real pleasure to have the distinguished position of President thrust upon him. He had made many friends, he had had the opportunity of becoming better acquainted with others, and his only regret was that the state of the industry was such that he could not place with each one of their members an order for ships.

One of the gratifying features of the Council Report was that twenty-five per cent. of their activities of the past session had been devoted to the consideration of the internal-combustion engine. Times of depression were never very pleasant, as he had already told the members of the Council in another place. Yet sweet were the uses of adversity which, like the toad, vile and venomous, had yet a precious jewel in its head. So with depression; it had always been through times of depression that progress had been made in the shipbuilding industry, and as the result of times of depression be believed an internal-combustion engine was going to be produced that would make it possible for the poor manager of ships to make some modest return to those who had invested their money in the industry. At the present time it was impossible for any ship be knew to make ends meet, and it was only by two methods that that position could be remedied, first, by reducing costs and, secondly, by improving the scientific machine placed in their hands. That was for members of the Institution to do, and before many months were over he hoped they would be able to find in the internal-combustion engine the real solution of their troubles.

In conclusion he thanked the members very sincerely for their kind vote of thanks, but wished to add that rather were his thanks due to them for the great privilege they had accorded him of occupying the distinguished position of their President for one year, and to the Secretary of the Institution whose help had reduced the President's tasks to the minimum.

He would not attempt in an extempore speech to say all he felt with regard to the extraordinary help he had received from the members of the Council still less could he adequately express his indebtedness to Mr. Frazer Smith: he would endeavour to do so in a letter which he hoped might find a permanent place in the records of the *Transactions* of the Institution.

On the motion of the President, it was unanimously

RESOLVED.—That Messrs. Sisson & Allden, the retiring Auditors, be re-appointed the Institution's Auditors for the current session.

Presentation of Engineering and Shipbuilding Gold Medals:—

The Engineering Gold Medal for 1921-2 was presented to Mr. Paul Belyavin for his Paper entitled "Some Problems of Marine Diesel Engine Design."

It was intimated that the Shipbuilding Gold Medal had been awarded to Mr. John McGovern for his Paper entitled "Some Notes on Shipbuilding Methods."

Mr. McGovern was unavoidably prevented from attending the meeting.

Presentation of Institute Scholarship for 1922, *and Prizes awarded upon upon the results of the Scholarship Examination:—*

The Institution Scholarship for 1922 was presented to Mr. F. H. Todd.

The following prizes were presented:—

Mr. William Learmonth, £5.
Mr. L. P. Tappin, £3.

It was intimated that a prize of £5 had also been awarded to Mr. Bertram Lillie, who was unable to be present.

Presentation of Awards for Bibliographical Papers on "Types of Ships and Reasons for their suitability for Particular Trades":—

It was announced that the first prize (value £5) had been awarded to Mr. Henry K. Rodgers, a student at the Royal Naval College, Greenwich, who was unable to be present at the meeting.

A second prize (value £3) was awarded to Mr. F. McAlister.

Presentation of Awards for Papers read before the Graduate Section during Session 1921-2:—

The following prizes for Papers read before the Graduate Section were presented:—

		£	s.	d.
Mr. J. Calderwood	value	3	0	0
Mr. F. Mills	,,	2	10	0
Mr. J. C. Arthur	,,	2	10	0
Mr. J. Tutin	,,	1	10	0
Mr. T. A. Crowe	,,	1	10	0

It was announced that Mr. A. C. Hardy had been awarded a prize (value £1), but that he was unable to attend owing to his being employed in London.

PRELIMINARY NOTICE of the proposed alterations to the Articles of Association was announced by the President.

Sir Archibald C. Ross delivered his Presidential Address (see page 7).

Sir William J. Noble said it was almost impossible to express one's feelings on the magnificent Address to which they had just listened. To him it had been peculiarly impressive. He felt that the spirit running through the whole of it was something that ought to be known and felt by employer and employee alike. To have a man occupying the position of Sir Archibald, in touch with the workers of this country, preaching that spirit of mutual sympathy and confidence was just the sort of thing that was wanted to remove the industrial unrest that they were suffering from to-day. If it were not for the fact that he believed he might be able to do something to remove the suspicion that was the cause of their troubles he would be only too glad to retire from the turmoil of commercial life; but so long as they had men with the spirit of Sir Archibald Ross at the head of great industries there was hope that that suspicion might be removed. To him it had been a great pleasure to listen to the Address not only because of its economic truths, but even more because of the spirit that pervaded it, and he thought he was only voicing their views when be moved:

> "That the grateful thanks of the Institution be accorded to the President for his Address and that he be requested to permit it to be printed in the *Transactions*."

Personally he would like to go a good deal further than that. The *Transactions* only reached a limited number of people, and he felt that the great mass of the workers of this country should have the opportunity of reading such a human document as that produced before them that night, and if the Council could see their way he would like to see the Address produced in pamphlet form and distributed broadcast amongst the workers of this country. It was that spirit which he was quite sure was going to break down the troubles from which they were suffering at the present time. If they could only get the men to realize that those who were called the employers were working for—that they were

all working for the same end and that they were one body of workers—he was sure that only good could result.

He begged to move the resolution.

Prof. R. L. Weighton seconded the resolution moved by Sir William J. Noble. He said that their ex-President had so completely justified the motion that he had left little or nothing for him to say. He had to confess that he was there that night with the greatest possible pleasure, for it gave him great satisfaction to see his old friend, Sir Archibald Ross, in the Presidential Chair. Sir Archibald was a very modest man, and he could tell from his eye that he was not altogether comfortable at that moment. Sir Archibald did not readily suffer praise. However he (Prof. Weighton) was temperamentally not given to "laying it on too thickly" and if he did not do it now he was sure Sir Archibald would be all the better pleased.

Like their past-President, he had been struck by the general tone of moderation of the Address just delivered. It said nothing to exasperate, but everything to conciliate, and that was not what every employer of labour did, he was sorry to say. Sir Archibald struck a note which was exceedingly moderate and just as between employer and employed. The Address was of an economic character, such as one would have expected; it did not touch on technical matters at all, and it was very gratifying to one who, like himself, considered there was far too much said about labour trying to get more than it was entitled to. All labour, whether it were physical or mental, was justified in getting the best terms it legitimately could, and he was very pleased to realize that their President had shown evidence of realizing this. He heartily endorsed everything that had fallen from Sir William Noble, and he asked the meeting to accord Sir Archibald Ross a very hearty vote of thanks for his Address and to wish him a successful, prosperous and happy year of office. He felt sure he would enjoy himself in the Presidential chair, and that with his facility of utterance he would adequately rise to every occasion.

The vote of thanks was carried by acclamation.

The President thanked Sir William Noble for his kind vote of thanks: he would not say it had been overdone, because he knew Sir William meant what he had said and said it from his heart. He looked upon Sir William as one of his best friends, and he would like to add with regard to the seconder, who so kindly supported the vote of thanks, that it was an especial pleasure that Professor Weighton should have undertaken this because the Professor had once been his honoured chief. Nothing he could think of could have been further from his mind in 1889, when he worked under the Professor at Hawthorn Leslie's, than that he might some day hold his present position as President and receive a vote of thanks from his then chief.

He also thanked the meeting very warmly for the kind way in which they had listened to and received his remarks.

The meeting terminated at 8.55 p.m. There were 176 members present.

PRESIDENTIAL ADDRESS.

By Sir ARCHIBALD C. ROSS, K.B.E., M.inst.C.E.

[Delivered in Newcastle-upon-Tyne, on the 20th October, 1922.]

I would begin with a note of regret that the retiring-President, after one year's service, is not again responsible for the inaugural address this evening. You will all recollect the admirable paper read by him on the 14th October last year, how he pointed out the new ground broken by our Institution in electing a Shipowner as President; the hope he expressed that the dignity and usefulness of the Institution would in no way suffer. I feel I am only endorsing the opinion of all members when I say that his endeavours to fulfil the duties of his office have enhanced the prestige and status of the Institution. This example, gentlemen, I shall endeavour to make my aim during my year of office.

This year you have once again reverted to an Engineer, and perhaps I may be excused if I mention as a matter of interest that during the thirty-eight years of increasing importance of this Institution you have had twenty Presidents, of whom I have the great honour to be the fourth from the old established firm of Hawthorn Leslie & Company. I am, therefore, faced with a dual responsibility, for not only has the standard set by all Past-Presidents to be kept in mind, but also the traditions and examples inherited from such men as Sir Benjamin Browne, Mr. F. C. Marshall and Sir Herbert Rowell.

During these thirty-eight years inaugural addresses have covered a very large range of subjects, and, as with all institutions, the subject matter of papers is a grave responsibility, felt perhaps in my case somewhat markedly as having been Chairman of your Reading Committee for the last four years. It is with some trepidation, therefore, that I purpose briefly to touch on some of the problems which must be uppermost in the minds of all those interested in our various professions be they engineering, shipbuilding or shipowning.

Great developments have taken place in all phases of life in the last few decades, and we are bound to realize and appreciate the natural aspirations and ambitions produced and fostered by the rapid spread of education. Much that was known only to the few fifty years ago is common property of the many to-day, and it is more difficult than in the past to keep abreast of developments and knowledge not only owing to the rapid advances of science which allow us to give less time for mature deliberation, but also because fresh thoughts and opinions are made known to the world almost before lips are closed or ink dry.

If some employer of say seventy years ago could be recalled to-day to conduct one of our establishments or businesses on the lines known to him

and accepted at that period—lines which at the time probably gained him esteem and loyalty as a good master—one doubts whether his success would be of long duration. Compared with those days, say 1850, the conditions of working in engine shops and factories, and the length of the working week show as marked a change as do our manufactures.

People in all walks of life are naturally prone to talk of the good old days, and whereas the giants in engineering of fifty years ago would look with incredulity on our modern practices, so assuredly, if we could lift up the curtain of the future, would we regard the developments of our successors.

Many obvious, but nevertheless important, considerations arise out of these developments, such as: "Can Industry as a whole afford to go on reducing hours of labour? Is our educational system the right one? Are we all pulling our weight? Are we keeping the true sense of proportion or are we developing abnormally in some directions to the detriment of others? In a word—if there is something wrong can we locate it? And if we think we can locate it, is our solution fair to all?"

Now I may be considered as travelling outside the usual province of our Institution if I venture to deal with such questions in this Address, but they are problems which affect the country's prosperity so vitally that we are bound at all times to give them close attention.

In 1919 I had an opportunity of attending a meeting of The American Society of Mechanical Engineers, in New York, and in a very large company of engineers there were four papers read dealing with subjects all centred on conditions of working. Now this I think may be overdone. The primary object of the Institution's existence will always be to discuss and further the developments of our industry from the technical point of view, but periodical consideration of subjects other than technical is advisable. After all, every improvement in design and machinery affects us as does a change in the affairs of nations, be it the elimination of boilers by the application of the internal-combustion engine, the development of air-craft or the curtailment in naval programmes resulting from the Washington Conference. These all have their effects on the costs of production and on the worker, and generally speaking on the necessity of fresh initiative to afford work for all.

What then, one may ask, is wrong with our system? Is it the existence of the capitalist system? Are workshop conditions unfair? Are employers all bad? or is it not possibly that certain ambitious sections of men, very often with little family or public responsibility preach doctrines of destruction with no constructive alternative and so lead the untried, *i.e.*, the learners, the seekers after better times, on the high road of an impracticable Utopia, where the less you do the more will be employed, where work or its equivalent in wages is to be had irrespective of the cost to the consumer, the manufacturer, the owner, or what is even more important the country; where wages can be paid, and goods stocked without any prospect of a market for them and where production is represented by wages paid to manual workers and nothing

more? Gentlemen, so long as money forms the basis of this world's dealings, be it a national exchequer or a village store, an end will come sooner or later if the expenditure exceeds the income.

These impracticable doctrines are apt to omit important factors, which should not be lost sight of at any period. One of these factors may be considered under the heading *Method* including, organization, purchasing, selling, commercial capability, manufacture, education and welfare; and the other factor under the heading of *Service*—not servile or feudal, but merely that honest inclination and desire to do one's best at work in the interests of the community and, what is more important, doing it.

The two are so closely allied that differentiation under these two headings is difficult. Dealing for a few moments with the first—Method—it is more or less an accepted historical fact that not only did Britain give to the world the best inventive and constructive brains in engineering, shipbuilding and shipowning, but that she possessed to a very great extent for a certain period a monopoly of the world's markets—a monopoly attributable partly to her inventors and workers, and partly to her geographical position, which position fosters a stimulating desire for self-preservation.

In engineering and shipbuilding most things can be reduced to the international language of the draughtsman's art and the formulæ of the mathematician and chemist, the natural sequence being that the advantages accruing from inventions and improvements were readily and eagerly assimilated by other countries, and quite apart from the abnormal conditions of international exchange, and the present perturbed state of the nations of the world, we find ourselves only one amongst many efficient competitors. These facts were, of course, staring us in the face in 1914, but if anything were needed as proof of our capabilities as a nation for output it is given by the tabulated returns of the work done in engineering shops and shipyards in Mr. Alfred Cochrane's Paper read before the Victory Meeting in July, 1919. These returns, although dealing with our own home district in particular, furnish ample proof of the country's potentialities, but do not show its limitations, for even that large war output could have been exceeded had it not been for occasional industrial disquiet. This country's resources and plant for ships and engines, not to mention war-time specialities (which one trusts may long be unused if not wholly unrequired), enabled us to weather the storm. In the Paper read by Sir Eustace Tennyson d'Eyncourt before the Institution of Naval Architects in 1921, he buried the wild rumours of the extraordinary rapidity with which the Germans built their war craft. Handicapped as we were by tens of thousands of our best manual workers joining the colours, and by shortage of materials, we were not outdone, and what was done was attained because the salvation of the country depended on output and service. Though the same evil that faced us from August, 1914 to November, 1918 is now absent, the same necessity—that of output and service—faces us now with even greater responsibility if we are to survive economically.

Under Method there is much to be learned from other nations in the matter of extending the sale of our manufactures. It is all to the good that the Government has, perhaps somewhat belatedly, instituted an Overseas Trade Department, helped with credits and has appointed Commercial Attachés to the Embassies and Ministries abroad. But this is not enough; we can no more look to the Government to nurse us through all our anxieties than we can expect the Government to keep us all contented though indolent. We must study closely the methods adopted successfully by others who have in many cases secured markets once the happy hunting ground of the unopposed British manufacturer. We must be more accommodating in meeting the requirements of other nations, not merely trying to persuade them to buy what in our opinion is good for them but showing a desire and willingness to supply them with their wants and to supply these wants more efficiently and if possible cheaper than our competitors.

The British are not noted for being demonstrative and possibly there is a tendency towards a certain degree of that insularity of character peculiar to our country which manifests itself in our dealings even with each other. There should be complete frankness and confidence between manufacturers and a natural inclination to share difficulties and compare notes. No one will deny the value of tradition and the deserved importance of success after long years of research and experimental work not to mention expense, nor can we expect the new starter on the first lap in life's race to attain the honour of fame or prestige of experience as a matter of course; but there are many instances in the world of manufacture where a frank disclosure of the minor difficulties of one maker may help the industry as a whole and avoid the word failure being applied too quickly through lack of investigation. A striking instance of this has been prominently brought to our notice this year in the case of geared turbines.

I have no intention of embarking on a dissertation on this very important question beyond saying that this great development in steam propulsion has, in bringing the engineer and the metallurgist together to confer in open forum, emphasized the importance of mutual exchange of knowledge and experience. I hope we may still see this system vindicated and reinstated in the confidence of our shipowning friends. As pointed out by Admiral Sir George Goodwin on more than one occasion, the geared turbine has made speeds and powers realizable which otherwise would have been impossible, and these powers and speeds were realized as far as the Royal Navy is concerned without any appreciable trouble. In the mercantile service I would go so far as to say that the successful performance of installations has been somewhat unfairly swamped by the prominence given to a few mishaps.

Under the heading Method as mentioned previously I have included education and welfare, questions which fifty years ago were not seriously considered by any management of works or yards, though shipowners had, of course, their apprentice rules and regulations. To-day we are

all bound to consider these important matters. Facilities either in connexion with the works or outside are such as to afford every boy an opportunity of acquiring a knowledge of the technical side of his trade, but whilst I would not deny the privilege and advantage of education for one moment I think we should remember that the possibilities of the marshal's baton being found in every soldier's knapsack does not mean an army of field-marshals. We cannot all be Doctors of Science and the dignity of a good craftsman is worthy of pride as is the dignity of high scholarship. Only by the process of elimination on a fair and equal start can the full benefits of education to our industries be reaped. In connexion with both technical and general education the apprentice of to-day has on the one hand a very distinct advantage in the matter of evening study, owing to the shorter working week and the absence of the 6 o'clock start—on the other hand possibly there is a greater temptation to waste time in the evenings. As facilities for acquiring knowledge increase guidance in the application of knowledge becomes all the more imperative.

Men's desires for better conditions run concurrently with the growth of education and there is a natural leaning towards independence. The old days of smaller works with their more or less fixed number of old hands where the men were known individually or personally, are no longer common and are scarcely possible in our large factories and shops, So the old esprit de corps and loyalty to a particular firm become less obvious. But in our apprentices we can instil these feelings to a very large extent by making them conscious that they are cared for and educated, and controlled by something more human in its bearing than the cold Factory Rules and Regulations.

Very much has been done in the way of welfare schemes. These are of the greatest value in creating a healthy spirit and it is an additional warrant for their existence to hear of the many applications made by the journeymen in factories and yards to have their share of play as well as work under the name of the firm with which they are working. It is also of interest to note that during the recent unfortunate industrial dispute there were instances of men, who, while not at work, voluntarily came to meetings of welfare committees and voluntarily gave their labour at the recreation fields. These welfare schemes are not without expense, but the enthusiasm of both men and apprentices, and their willingness to share in the expense, are signs that there is not so much wrong with the hearts of British manual workers as some are prone to imagine. "Welfare" is not the monopoly of the apprentices but should be shared in by all in the works.

Under the heading Service one hesitates to elaborate the definition I have already given, for two reasons. In the first place because it is so obvious, and secondly because I might be tempted to enlarge on certain features in connexion with Labour which are so much the subject of debate and criticism. After all, Labour's aspirations to-day to hold the reins of government are the outcome of education and their leaders are

professionals at the work and handle their case with great ability. I think it may be stated that there are men who seriously believe and preach a limitation of output in the honest belief that it will ensure less unemployment, just as there are others who are convinced that the cheaper you produce, though not necessarily the less wages you pay, the more work there will be. Experience alone will prove to both parties who is right. The full effect of the great War has not yet run its course. When imperative, we did the work necessary to save the country and it may be a blessing in disguise for the country's industry as a whole that we have had to face and are still facing in many cases, a serious depression especially noticeable in shipbuilding and marine engineering.

Under war conditions and after the war under the influence of the dramatic, but in many cases miscalculated boom that succeeded the armistice in 1918, certain industries were in the position of practically refusing orders—a state of affairs detrimental to the minds and actions of all concerned, damping the salesman's alertness, the producers' interest in improvements in production, and encouraging the manual worker to sell his labour at the highest figure, reflecting in turn on the costs of production and the cost of living. We lived for nearly six years taking little thought for the morrow, and the stern lesson we are all learning to-day shows us in naked truth the necessity for unremitting personal service.

During this period a new generation of apprentices has come into our industry and now in many cases the trade depression is denying them the opportunities to learn their crafts to the extent possible in busier times; the incentive to work and become expert may not appear so necessary, but to them I would say it will not be always thus, they must make the very most of existing opportunities and fit themselves for true service.

This year again has not been free from grave industrial disputes. To many the detailed reasons have not been quite clear, and the advocates on both sides may in the opinion of some have appeared unduly severe.

To arrive at mutual agreement without the interference of outside parties is possible only by better faith and trust between management and workpeople, and between the existing combinations of Federations and Associations and the Trade Unions. The fundamental and most important requirement towards the attainment of this is that all leaders and spokesmen should represent the majority opinion of their respective constituents all of whom in the interest of the industry must bear the responsibility of taking a personal share in the work. This is a real responsibility for all concerned and one which should be impressed on the younger generation of both sides, for both sides are really one—a great body of workers striving for a common goal: the keeping of our country in the forefront of civilization and progress—a goal achieved only by a high sense of duty and service, and by diligent attention to Method.

REPORT OF THE COUNCIL, 1921-1922. SESSION THIRTY-EIGHT.

During Session Thirty-eight the activities of the Institution so far as they concern the holding of meetings and the reading, discussion and publication of Papers dealing with engineering and shipbuilding, have, as the details given below will show, been vigorously continued through the valuable and generous co-operation of the large number of gentlemen who have read Papers, or contributed to the discussion upon them. The Council takes this opportunity of recording its thanks to those authors and contributors. There can be no doubt that both papers and discussion will in many instances have direct and practical effects on certain industries.

Papers and Meetings.

The Papers delivered before the Institution during the Session number fourteen, their titles being as follows :—

Presidential Address, by Sir William J. Noble, Bart.

"The Progress of Aviation since the End of the War," by Major-General Sir W. Sefton Brancker, K.C.B.

"Sea Casualties and Loss of Life," by Sir Westcott S. Abell, K.B.E.

"The Working of a Modern Telephone System," by C. Whillis, M.I.E.E.

"Maag Gearing," by Eng. Lt.-Commander L. J. Le Mesurier, R.N. (ret.).

"Note on Some Proposals made during the War for protecting Merchant Ships against Submarines," by Sir E. H. T. d'Eyncourt, K.C.B., F.R.S., D.Sc.

"Railway Electrification," by Sir Vincent L. Raven, K.B.E., M.I.Mech.E.

"Diesel Engine Flexibility," by W. S. Burn, M.Sc.

"The Use of Compressed Air in Diesel-Engined Ships," by William Reavell, M.I.Mech.E., M.I.N.A.

"Some Notes on Shipbuilding Methods," by John McGovern.

"Some Problems of Marine Diesel Engine Design," by Paul Belyavin.

"Standardization; its Fundamental Importance to the Prosperity of our Trade," by C. Le Maistre, C.B.E., Secretary, British Engineering Standards Association.

"Corrosion; with Special Reference to the Ferrous Metals and the Deterioration of Ships," by A. Pickworth, M.Sc.

"Actual Running Cost of Motorships," by Tage Madsen.

The thirteen Papers printed in the *Transactions* comprise the President's Address, three dealing with naval architecture, the same number with internal-combustion engines, and two each with electrical and mechanical engineering. The two remaining Papers stand by themselves and are those by Sir Westcott Abell and Mr. C. le Maistre.

Sea Casualties

Sir Westcott Abell's Paper is, as its title signifies, an actuarial composition, but it contains so much matter of interest to users and constructors of ships, that it is a valuable and appropriate addition to

the technical publications of the Institution. The Paper drew a voluminous discussion from experts and in June, 1922, formed the basis of a critical Paper* read before the Royal Statistical Society. It was also considered at the First International Shipping Conference, held in London on the 23rd and 24th November, 1921.

Standardization

Mr. le Maistre's Paper was a plea for the better recognition by engineers and shipbuilders of the commercial advantages of standardization. As an inevitable incident, it also showed the remarkable development of the British Engineering Standards Association, of which Mr. le Maistre is Secretary. The Institution has actively co-operated with the B.E.S.A. in many branches of its work—particularly, in recent years in the standardization of ships' details—its acceptance of this Paper is, therefore, merely an additional indication of its approval of the wider application of standardization to engineering and shipbuilding.

Papers on Naval Architecture

Sir Eustace d'Eyncourt's was the first Paper of the Session dealing with naval architecture. He described certain applications of "bulge" protection to merchant ships to enable them to withstand submarine attack. A large wheat carrier, a cargo-and-oil carrier, and the "Plumleaf" class of oil-carrying ships—all protected against submarine attack—are described in the Paper and illustrated by plates.

Mr. John McGovern presented a Paper on the practical side of shipbuilding, describing as it did, the very modern methods for the inauguration of which he has been responsible at the Furness Shipbuilding Co.'s yard at Haverton Hill. Although not indicated in its title, the Paper contained much debatable matter on the "straight-frame" type of ship and this aspect, along with the author's mass-production methods, received its full share of attention in the full discussion on the Paper. Mr. McGovern made his Paper as practical as is humanly possible by adding to it an invitation to members of the Institution to visit his yard and see the methods advocated in practice—an invitation which some contributors to the discussion accepted. The Paper is awarded the Institution's Shipbuilding Gold Medal.

Mr. A. Pickworth's Paper is only a naval architecture contribution in so far as it relates to the deterioration of ships; along with several of the Papers here reviewed, it has strong claims for classification under more than one heading. Apart from its references to marine corrosion, it summarizes very clearly and accurately up-to-date ideas on the causes of the corrosion of ferrous metals.

Papers on the Internal-Combustion Engine

Having in mind the rapidly growing importance of the internal-combustion engine, it is only fitting that three out of the thirteen Papers published during the past Session should have that type of engine in one form or another as their subject. That it should have been necessary to devote a quarter of the year's syllabus to one—and a very special—branch of engineering, suggests that some measures are required for increasing

* "The Study of Statistics relating to Shipping Casualties and Loss of Life at Sea," by J. W. Verdier, O.B.E., 20th June, 1922.

the number of Papers read, and thus giving more adequate attention to the many important sections of engineering in which members of the Institution are interested. This desideratum will receive the Council's careful attention.

Mr. W. S. Burn's Paper on "Diesel Engine Flexibility," as well as treating of the subject forming its title, presented a proposal of the Author's for a "Diesel-air locomotive," the ultimate success of the Diesel locomotive depending, in the Author's opinion, upon the flexibility obtainable from the Diesel engine. The discussion on this Paper included seventeen contributions and divided itself into criticisms of Mr. Burn's statements on flexibility and his proposed locomotive.

Mr. Burn has the distinction of being the first Graduate member to read a Paper before a General Meeting of the Institution. His Paper, with the exception of that part of it dealing with the Diesel locomotive, was originally read before the Graduate Section of the Institution and was awarded a prize of £5 by the Graduate Award Committee. The Council, after considering the merits of the Paper, decided that it should be brought to the attention of the members by being read at a General Meeting and printed in the *Transactions.*

Mr. Belyavin's Paper, for which the Engineering Gold Medal for the Session is awarded, presented a happy combination of theoretical and practical guidance on the design of two-stroke marine Diesel engines. The Author endeavours to show that the earlier difficulties of the two-stroke engine have been removed and contends that it can now be built as reliable as the four-stroke engine. He anticipates successful development of the single-acting two-stroke engines.

The third Paper on the internal-combustion engine was that by Mr. Tage Madsen. This Paper might be divided into two sections, the one consisting of observations on the various phases of the running of marine internal-combustion engines, the other consisting of actual voyage reports of motor and steam-driven vessels. The most interesting of the voyage reports at the time when the Paper was read, was probably that of the maiden voyage of the motorship "Yngaren"—the first trading trip of a vessel driven by the Doxford opposed-piston engine. Some colour was lent to the Paper by its having been written on board the "Yngaren" during the latter voyage, and read within a few days of that vessel's return to home waters.

Electrical Engineering Papers

The two electrical engineering papers dealt with widely different branches of that great section of engineering, namely, the telephone and the electrification of railways. The first of these, by Mr. C. Whillis, Engineer of the Post Office Telephone Department, Newcastle South Section, was a general description of the mechanism and working of a Common Battery telephone system. The Paper was read both in Newcastle and Middlesbrough and in each case was accompanied by practical demonstrations upon apparatus provided by Mr. Whillis.

"Railway Electrification," by Sir Vincent Raven, must be regarded as one of the most important of the Papers read during the session. In

view of the great commercial considerations involved, Sir Vincent's conclusion in favour of the use of electric locomotives on railways—a conclusion which the many contributors to the discussion and readers of the Papers have, significantly, left practically unassailed—is an event to be recorded in the history of railway engineering.

Mechanical Engineering Papers

Eng. Lieut.-Commander Le Mesurier's Paper described the theory, methods of manufacture and testing, and some applications of Maag straight-tooth gears.

Mr. William Reavell, the well-known manufacturer of compressed-air apparatus, described in a short paper auxiliary and emergency air compressors for use with Diesel engines of air- and solid-injection types. The Paper also deals with the use of air for scavenging purposes.

Major-General Sir W. Sefton Brancker's lecture, which was interestingly illustrated by a cinematograph film, had for its object the advocacy of the development of a "flourishing aerial transport industry." Civil aviation, he said, was necessary from the military point of view, because it was the only economical means of having enough efficient aircraft available to meet the requirements of a national emergency; the Government, in his opinion, should give some measure of direct financial assistance to aerial transport to tide it over its initial troubles. General Brancker further advocated the placing of Navy Army and Air forces under one central control.

Discussion on Papers

The following gentlemen, 106 in number, contributed to the discussion on Papers:—

H. A. D. Acland.
J. C. Arthur.
Wilfrid Ayre.
F. G. C. Baldwin.
Maxwell Ballard.
J. T. Batey.
Charles Blow.
Prof. A. L. Bowley.
H. I. Brackenbury.
J. S. Browne.
A. D. Bruce.
Dr. J. Bruhn.
A. L. Bullwinkle.
Sir Joseph Burn.
C. Waldie Cairns.
Thomas Carter.
J. L. Chaloner.
Joseph Chilton.
Alan E. L. Chorlton.
George Clark.
Stanley S. Cook.
S. W. Corns.
W. Cross.
T. A. Crowe.
A. B. Culley.
Elliott Cumberland.
J. Dalziel.
C. S. Darling.
L. C. Davis.
Prof. C. H. Desch.
Sir E. H. T. d'Eyncourt
W. G. Dobson.
A. Ernest Doxford.
Dr. J. T. Dunn.
R. J. M. Elliott.
R. J. Eyres.
Dr. W. H. Hatfield.
Vincent Gartside.
F. W. Gaskins.
J. Hamilton Gibson.
Thomas Graham.
T. Grime.
Andrew Hamilton.
G. M. Harroway.
Eng.-Commdr. C. J. Hawkes, R.N. (ret.).
W. Dennis Heck.
J. W. Hobson.
J. Hodgson.
Norman L. Hudson.
Sir George B. Hunter.
Summers Hunter.
Dr. L. Isserlis.
Bernard J. Ives.
K. O. Keller.
Daniel Kemp.

Henry Kitchen.
W. Veysey Lang.
H. Warren Lee.
Lt.-Col. F. A. C. Leigh.
Eng. Lt.-Commdr. L. J. Le Mesurier, R.N. (ret.).
Col. James Lithgow.
Prof. H. Louis.
S. J. Lucas.
Col. D. R. MacDonald.
G. F. McGregor.
R. J. McLeod.
C. le Maistre.
F. Leigh Martineau.
F. Mills.
Harold Moore.
Dr. John Morrow.
David Myles.
Sir William J. Noble, Bart.
North British Locomotive Co., Ltd.
F. R. Noton.
Ernest Parkinson.
The Hon. Sir Charles A. Parsons.
Major A. P. Pyne.
W. F. Rabbidge.
Major S. G. Redman.
James Richardson.
Sir Archibald C. Ross.
H. B. R. Rowell.
H. Ruck-Keene.
J. Sayers.
D. M. Shannon.
Roger T. Smith.
W. G. Spence.
A. Stone-Hurst.
Prof. G. G. Stoney.
E. V. Telfer.
Harold Thomson.
J. Taylor Thompson.
H. T. Tizard.
T. C. Tobin.
John Tutin.
A. T. Wall.
Robert Wallis.
Commdr. H. L. Walton, R.N.R.
Oswald Wans.
H. A. Watson.
Kenneth Watson.
Prof. J. J. Welch.
T. Westgarth.
H. G. Williams.
G. E. Windeler.

The number of contributions received was 134. The maximum number of contributions received on any one Paper was 17; the minimum, 4. The average was 11; that for the previous session, during which 7 Papers were read, was 9·6. Sixty-seven out of the 134 contributions were received by post.

Meetings

The number of ordinary general meetings held, including the Annual General Meeting, was 14. The average attendance at these was 133. The maximum was 320; the minimum, 69. The average attendance at meetings held during the previous session was 155.

Social Functions

Two purely social meetings were held during the session. These were the President's Reception and Dance, and the Institution Dinner. The Dance, at which Sir William and Lady Noble very kindly received and entertained the members of the Institution and their ladies, was held on the 12th January, 1922, in the Grand Assembly Rooms, Newcastle-on-Tyne.

The Institution Dinner was held on the 3rd March, 1922, in the Grand Assembly Rooms, Newcastle-on-Tyne. Upon the suggestion of the President it was decided to depart from the custom which had up till that time been followed at the annual banquets of this and kindred societies by the adoption of two important innovations. These were, the inclusion of ladies in the company at dinner, and the omission of after-dinner speeches. In place of the latter a short musical programme followed the dinner, while the party was still at table. After the music, the company withdrew to another room for an informal conversazione; this provided an opportunity for members who had not been placed near

one another at table, to renew their acquaintance and thus helped the banquet to fulfil one of its chief functions, namely, that of a social re-union of members of the Institution. The company numbered 289 of whom 80 were ladies.

Branches.

Middlesbrough Branch

The inaugural meeting of the Middlesbrough Branch was held on the 11th November, 1921, when an address was delivered by Sir William J. Noble, Bart., President.

In addition to this, six meetings were held during the session at which the following Papers—all previously read before the Institution in Newcastle—were read and discussed :—

"Working of a Modern Telephone System," by C. Whillis, M.I.E.E.
"Some Notes on Shipbuilding Methods," by J. McGovern.
"Fuel oil; Production and Uses," by W. A. White.
"Problems of Diesel Engine Design," by Paul Belyavin.
"The Use of Compressed Air in Diesel-Engined Ships," by William Reavell.

The average attendance at the seven meetings was about 160. All of the meetings were held at the Cleveland Technical Institute, Middlesbrough.

Mr. G. M. Harroway, at the invitation of the Council, has officiated as Chairman of the Branch throughout the Session.

Cleveland Technical Institute

The Cleveland Technical Institute (established by the joint efforts of a Committee formed by this Institution and the Cleveland Institution of Engineers) has, in its first year of active existence, amply justified the hopes of its founders. The Institute was opened by the Hon. Sir Charles A. Parsons, K.C.B., F.R.S., on the 22nd September, 1921, and since then 78 meetings have been held in its building. These meetings include those of the Institution's Middlesbrough Branch, those of the Cleveland Institution of Engineers, the Foremen Draughtsmen and Chief Engineers' Association and the Institution of Electrical Engineers (Tees-side Sub-section). There were also many popular lectures arranged by the Institute and the Institute's halls were used on occasions for meetings by various local societies apart from those mentioned above.

The Institute publishes a monthly Bulletin and conducts a Technical Intelligence Service. The former gives an average of fifty abstracts of important scientific and technical articles published during the month as well as particulars of the leading British and foreign patents. The Intelligence Service has already dealt with upwards of two hundred enquiries for technical information.

The Institute is also custodian for the North of the British Standardized Steel Samples as issued jointly by the Iron & Steel Institute and the National Physical Laboratory.

Gifts.

"Great Eastern" Name-plate

Mrs E. M. Crawford, carrying out the wish of her husband, the late Mr. W. A. Crawford, a former member of the Institution, presented the Institution with the makers' name-plate of the "Great Eastern." The

plate is framed and hangs in the Library corridor. The thanks of the Council have been conveyed to Mrs. Crawford and are again recorded here.

The Institution is indebted to the Parsons Marine Steam Turbine Co., Ltd., for a signed photogravure of Sir William Orpen's portrait of the Hon. Sir Charles A. Parsons, K.C.B., F.R.S., Honorary Fellow and Past-President. Sir Charles A. Parsons' Portrait

Representatives.

The Institution has been represented during the Session on the undermentioned bodies by the gentlemen indicated :—

Representative on the Advisory Committee of the William Froude National Tank—The Hon. Sir Charles A. Parsons, K.C.B., F.R.S.

Representative on the Conjoint Board of Scientific Societies—The President.

Representatives on the British Marine Engineering Design and Construction Committee—F. T. Dickinson, Andrew Laing, C.B.E., D. B. Morison and Prof. R. L. Weighton, D.Sc.

Representatives on Lloyd's Technical Committee—J. T. Batey, W. H. Dugdale, M. S. Gibb, C.B.E., and Andrew Laing, C.B.E.

Representatives on the Consultative Committee to the Board of Trade—George Jones, Col. D. R. MacDonald, D. B. Morison and D. Myles.

Representatives on the British Corporation for the Survey and Registry of Shipping—F. T. Dickinson and G. M. Harroway.

Representative on the Council of Armstrong College—The President.

Representative on the Governing Body of the Marine School, South Shields—D. W. Fitzgerald.

Representative on the Selective Committee of the Ministry of Labour Appointments Department—D. Myles.

Representative on the Education Committee of the Federation of British Industries—The Secretary.

Representative on the Electrical Engineering Department Committee, Armstrong College—R. P. Sloan, C.B.E.

Representative on the Sectional Committee on Pipe Threads, British Engineering Standards Association—Andrew Laing, C.B.E.

Representative on the Sectional Committee on Pipe Flanges, British Engineering Standards Association—Andrew Laing, C.B.E.

Representative on Committee on Standardization of Gears, British Engineering Standards Association—Stanley S. Cook.

Representative on Gateshead Education Committee, Joint Advisory Committee—A. P. Pyne.

Chairman of North-East Coast Institution Panels, British Engineering Standards Association Committee on Ships' Details—Col. D. R. MacDonald.

Representative on the International Navigation Congress, General Organization Committee—The Secretary.

The Roll.

The net total of members at the 31st July, 1922—after deducting the number of members who have died or resigned during the session, and those whose names have been removed from the register for non-payment of their annual subscriptions—is :— The Roll

Honorary Fellows	22
Fellows	18
Members	486
Associate Members	542
Associates, Grade "A"	71
Associates, Grade "B"	67
Graduates	388
	1,594

At the commencement of the session the members totalled 1,585.

The Institution has to regret the loss by death during the session of 15 of its members, whose names are as follows :—

James Bain.
Sir Francis Barker.
Samuel Buchanan.
Sir George J. Carter.
Charles Christie.
James Dickie.
Anthony Strong Frazer.
Henry E. Hollis.
C. S. Hunting.
Johan Johnson.
J. A. Mannell.
Alfred Monty.
Edward Seabury.
C. H. Tate.
Henry Withy.

Distinctions Conferred upon Members.

The congratulations of the Council were conveyed to Sir James Readhead, *Member*, upon his being created a Baronet, and to Mr. R. H. Millican, *Associate*, upon his election as Lord Mayor and Alderman of the City and County of Newcastle-on-Tyne.

Other Activities.

Standardization of Ships' Details

The Institution's Panels engaged in the standardization of ships details, have continued their meetings throughout the Session. As the result of their efforts, in conjunction with those of the Clyde Panels, the British Engineering Standards Association has issued, during the past twelve months, standards for the following items of ships' equipment :— Table Glass and Crockery, Oil Lamps, Locks for Ships-joinery work, Hinges for the same, Vertical Type Bollards and Stud Link Cables (Anchor Cables). The Panels are busy with many other standards. The thanks of the Institution are due to Colonel MacDonald, the Chairman of the Institution Panels, and to the Sub-chairmen and members of the Panels.

Mile Posts

The replacement of certain of the Mile Posts referred to in last Report has been completed. Negotiations are in progress with the Hydrographic Department of the Admiralty with the object of obtaining the Admiralty certificate for the true distance measured by the Posts.

Standardization of Fuel Oils.

The suggestion made by Mr. T. Westgarth in the discussion on Mr. Madsen's Paper, to the effect that the Council of the Institution should appoint a Committee to consider the standardization of fuel oil—following as it did on cognate remarks by Mr. Madsen himself and Mr. Summers Hunter—has been followed by the Council. The Institution

is in touch with both the Institution of Petroleum Technologists and the British Engineering Standards Association, and the subject will continue to receive the Council's attention.

Finance.

The Balance Sheet and Accounts for the 1921-2 session, are attached. The general revenue for the session, as shown in the accounts, was £5,039 6s. 11d. and the expenditure £5,185 3s. 8d. Finance

The Endowment Fund has benefited during the session by a donation of £500 from Mr. A. Ernest Doxford. M.A., *Past-President* and *Fellow*. Scholarship

The Furness Shipbuilding Company, Ltd., has generously presented the Institution with the sum of £500. The income from this capital sum is to be devoted to the maintenance of the newly formed Middlesbrough Branch.

The thanks of the Institution have already been conveyed to Mr. Doxford and the Furness Shipbuilding Company for the above donations; the Council have now much gratification in placing these thanks on record.

The Scholarship for 1922 (value £100 per annum, tenable for two and conditionally for three years) is awarded to Mr. F. H. Todd, who took the highest number of marks in the Scholarship Examination.

Mr. Todd was educated at the North-Eastern Counties' School, Barnard Castle, and Rutherford College. Until recently, he was an apprentice draughtsman in the drawing office of Messrs. Sir W. G. Armstrong, Whitworth & Co.'s Naval Yard, and he is now studying as a Naval Architect student at Armstrong College.

The second, third and fourth places in the examination were taken, respectively, by Messrs. William Learmonth, Bertram Lillie—who are awarded prizes of the value of £5 each—and Mr. L. P. Tapping, who is awarded a prize of the value of £3.

There were sixteen competitors for the 1922 Scholarship, which is the largest number since the Scholarship was founded.

Satisfactory reports have been received from the Armstrong College authorities upon the work of the holders of the Institution Scholarship during 1921-2—Messrs. John Tutin, E. J. Rang, W. U. Doling and Leslie Blair. Mr. Tutin has obtained his B.Sc. degree with Honours in Naval Architecture. Mr. Rang has passed his third examination for the degree of B.Sc. with Honours in Civil Engineering. Mr. Doling has passed the second B.Sc. examination in Marine Engineering. Mr. Blair has passed the second B.Sc. examination in Civil Engineering.

Library.

The following works have been added to the Library during the Session :— Library

"Catalogue of British Scientific and Technical Books, 1921."
"Brassey's Naval Annual, 1921-1922."

"Report on Heat Insulators." Committee of the Food Investigation Board, Special Report No. 5 (1921).

"First Report of the Gas Cylinders Research Committee." Department of Scientific and Industrial Research (1921).

"Electric Ship Propulsion," Commander S. M. Robinson, U.S.N.

"Report of the Fuel Research Board for the years, 1920-1921." Second Edition. "Low Temperature Carbonization." Department of Scientific and Industrial Research (1922).

"Steam Turbines," W. J. Goudie.

"Boiler Plant Testing," David Brownlie.

Presented by Kindred Societies:—

Current Proceedings.

Presented by Lloyd's Register of Shipping:—

Register (Vols. 1 and 2), 1921-1922.

Report by the Chief Engineer Surveyor on Fires on Board. Dr. James T. Milton.

Register of Yachts, 1922.

Presented by l'Association Technique Maritime:—

Bulletin de l'Association Technique Maritime, No. 26, 1915-1920.

Presented by E. & F. N. Spon, Ltd.:—

Marine Diesel Engine and Semi-Diesel Engine Operation and Management. Eng.-Lt. Harold Atkinson, R.N.V.R.

Presented by Crosby Lockwood & Son:—

Entropy as a Tangible Conception. S. G. Wheeler.

Presented by Andrew Reid & Co., Ltd.:—

Oil Fuel at Sea. "Visco."

Presented by Smith's Dock Co., Ltd.:—

Smith's Dock Monthly. Vols. 1 and 2.

Presented by the Canadian Department of Mines:—

Gas Producer Trials with Alberta Coals. John Blizard and E. S. Malloch.

The Preparation, Transportation and Combustion of Powdered Coal. John Blizard.

The Production of Copper, Lead, Nickel, Silver, Zinc and other Metals in Canada during the calendar year 1920.

The Production of Coal and Coke in Canada during the calendar year 1920.

Report of the Canadian Department of Mines for the fiscal year ending 31st March, 1921.

Annual Report on the Mineral Production of Canada during the calendar year 1920.

Summary Report of Investigations made by the Mines Branch during year ending 31st December, 1920.

Summary Report of Investigations made by the Mines Branch for 1920.

Barium and Strontium in Canada. Hugh S. Spence.

Presented by M. Haramiishi:—

On the Application of Electric Arc Welding to two Vessels. M. Haramiishi.

Presented by the Société des Ingenieurs Civils de France (T. J. Gueritte):—

The Design of Circular Bridges. B. de Fontviolant.

Presented by the National Advisory Committee for Aeronautics, Washington, U.S.A.

Bibliography of Aeronautics.

Sixth Annual Report of the National Advisory Committee for Aeronautics, 1920.

Seventh Annual Report of the National Advisory Committee for Aeronautics, 1921.

Presented by Det Norske Veritas :—

Register over Skandinaviske Skibe, 1922.

Rules and Tables for the Building and Classification of Steel Vessels, 1922.

Presented by the Department of Commerce, U.S.A. :—

Scientific Papers of the Bureau of Standards, Washington—

No. 422. Studies in Colour, Sensitive Photographic Plates and Methods of Sensitizing by Bathing. F. M. Walker, junr., and Raymond Davis.

No. 424. Mathematical Theory of Induced voltage in the High-tension Magneto. F. B. Silsbae.

No. 425. Characteristic soft X-ray from Arcs in Gases and Vapors. L. Mohler and Paul D. Foote.

No. 426. Thermal expansion of Nickel, Monel Metal, Stellite, Stainless Steel and Aluminium. W. H. Souder and Peter Hidnert.

No. 427. Some effects of the Distributed Capacity between Inductance Coils and the Ground. Gregory Breit.

Technologic Papers of the Bureau of Standards—

No. 200. An investigation of Oxy-acetylene Welding and Cutting Blow Pipes, with especial reference to their Design, Safety and Economy in Operation. R. S. Johnston.

No. 203. Influence of Phosphorus upon the Micro-structure and Hardness of Low-carbon, open Hearth Steels. E. C. Grosbeck.

No. 204. Cutting Fluids. E. C. Bingham.

No. 205. Tensile properties of some Structural Alloy Steels at high temperatures. H. F. French.

No. 206. Effect of Heat Treatment on the Mechanical Properties of 1 per cent. Carbon Steel. H. J. French and W. G. Johnson.

Presented by the Proprietors :—

Kempe's Engineer's Year Book for 1922.

Presented by Bureau Veritas :—

Register of 1922.

Presented by Sir W. G. Armstrong, Whitworth & Co., Ltd. :—

The fitting of Oil-Fuel Burning Installation of Q.S.T.S. " Berengeria " (with plans).

Presented by the British Corporation for the Survey and Registry of Shipping :—

Register of Vessels, 1922.

Presented by S. J. Lucas :—

Articulated Locomotives. (Paper read before the Institution of Locomotive Engineers, 1920). Lt.-Col. E. Kitson-Clark.

Presented by the Department of Scientific and Industrial Research :—

On the Electro-Deposition of Iron. W. E. Hughes.

Presented by the British Marine Engineering Design and Construction Committee :—

Standard Conditions for the Design and Construction of Marine Boilers and Shafting for Marine Engines.

Presented by the British Association :—

A Retrospect, 1831-1921.

The number of books borrowed from the Library during the Session was 414; the number of borrowers was 138. The figures for the previous Session were :—Books borrowed, 324; the number of borrowers was 123.

Report of Graduate Section.

The Graduate Section, which resumed its activities in October, 1920, vigorously continued its work during the 1921-1922 Session, under the

guidance of the following Committee:—E. V. Telfer, M.Sc., Chairman; J. L. Taylor, B.Sc., Hon. Secretary; J. C. Arthur, M.Sc., J. Calderwood, M.Sc., T. Cato, R. H. Davison, Alan Dent, F. Mills, W. D. Horsley.

Apart from joint meetings arranged in conjunction with the local junior sections of kindred societies (which are referred to later) the section held eight meetings during the Session. One of these was a purely business meeting; the following Papers were read and discussed at the remaining seven meetings:—

"An International Language and Engineering," by E. V. Telfer, M.Sc.
"Steam Locomotive Design and Construction," by F. Mills.
"Atlantic Trade Routes and the Future of the Auxiliary Sailing Ship," by A. C. Hardy.
"Aircraft," by J. Calderwood, M.Sc.
"The Efficiency of the Motor Ship," by John Tutin, B.Sc.
"The Testing of Diesel Engines," by J. C. Arthur, M.Sc.
"Some Possible Developments in Steam Reciprocating Marine Engines and Boilers," by T. A. Crowe, M.Sc.

In addition to the above meetings, the Section visited the undermentioned works, etc.:—

Messrs. Hawthorn, Leslie & Co., Ltd., Forth Banks Locomotive Works.
The Lemington Glass Works.
The Cowpen Coal Co., Ltd.; Cowpen Isabella Pit.
Newcastle-upon-Tyne Electric Supply Co., Ltd.; Carville Power Station, Wallsend. (Two visits.)
Messrs. Swan, Hunter & Wigham Richardson Ltd.; Wallsend Shipyard.

The thanks of the Council and of the Graduate Section have already been conveyed to the above companies for their great kindness in permitting the visits and for their trouble in conducting the parties through their works. It is only fitting, however, that these thanks should be placed on record in this Report. The visits are of great educational value to the Graduates, who have shown their appreciation of that fact and of the privileges granted to them by the various firms, by attending the visits in large numbers.

While recording with satisfaction the attendance at visits, it has to be reported with the greatest regret that the attendance at the ordinary meetings of the Section has been most disappointing. In view of the excellence of the Papers read and discussed at these meetings, and also bearing in mind that the meetings give the Graduates an unique opportunity of professional intercourse with members of their own age and technical experience, it is hoped that the Graduates will, by their attendance at the meetings to be held during the coming session, provide matter for more favourable comment in next annual report.

Study Circles

The work of the Internal-Combustion Engine and Strength of Ships Study Circles, which began in 1920, was continued throughout the session.

At the beginning of the present session each member of the Internal-Combustion Engine circle undertook the study of one branch of the subject which was sub-divided as follows:—

(1) Thermodynamic principles and design.
(2) Engine cooling and the stresses due to temperature differences.

(3) Mechanical design.
(4) Lubricating oils, fuel oils.
(5) Metallurgy.
(6) Engine testing.

The study circle met every Wednesday evening, and at each meeting some member opened a discussion on the branch of the subject that he was studying. In some cases, a précis, or else a short Paper was read. By these discussions it was possible for the members to obtain information on a wider range of branches of the subject than could have been the case by individual study of the subject as a whole, and it is hoped that the various Papers read may be of value to other Graduates, wishing to study the subject, who have been unable to attend the meetings. They may be consulted in the Institution Library.

The result of the work in this study circle seems fully to justify its further development.

Although many interesting discussions took place, the functioning of the Strength of Ships circle was considerably restricted by the inability to fix upon a night suitable for all members. This difficulty was partly due to the competition of evening classes which were compulsory for the younger Graduates.

Subscription

During the past two sessions it was very evident to the Graduate Section Committee that the subscription required of a Graduate was much lower than that required by any kindred institution. On the other hand, it was also evident that the facilities provided by the Institution were greater than those offered by other institutions, and especially such institutions as have headquarters or branches in Newcastle-upon-Tyne. To the Committee, the position of the Institution's finances did not seem to warrant such favourable consideration for the Graduates; and after due reflection, both in committee and at a general meeting of the Graduate Section, it was decided to request the Council of the Institution to consent to the increasing of the Graduates' subscription. This suggestion has been adopted by the Council with a view to its becoming operative next session (1922-1923).

Awards for Papers

Awards to the value indicated are being made to the following authors of Papers read before the Graduate Section :—

	£	s.	d.
Mr. J. Calderwood	3	0	0
Mr. F. Mills	2	10	0
Mr. J. C. Arthur	2	10	0
Mr. J. Tutin	1	10	0
Mr. T. A. Crowe	1	10	0
Mr. A. C. Hardy	1	0	0

Bibliographical Papers

The first prize, value £5, for Bibliographical Papers by Graduates is awarded to Mr. Henry K. Rodgers, and the second, value £3, to Mr. Frank McAllister, who both submitted Papers on "Types of Ships and Reasons for their suitability for Particular Trades." Both Papers were very favourably reported upon by the examiners.

The Bibliographical Papers and those read before the Graduate Section may be consulted in the Institution Library.

Joint Papers

The following papers were read at meetings arranged by the Joint Committee of the Institution's Graduate Section, the Associates' and Students' Section of the North of England Institute of Mining and Mechanical Engineers and the Students' and Graduates' Section of the local branch of the Institution of Electrical Engineers :—

Arranged by the Institution's Graduate Section :—

"The History of Iron and Steel Manufacture," by Prof. Henry Louis, D.Sc.

"Corrosion," by A. Pickworth, M.Sc., M.I.N.A.

Arranged by the North of England Institute of Mining and Mechanical Engineers :—

"The Installation of a Ward-Leonard Winding Plant," by J. T. Pringle.

"Surveying," by Harry Dean.

Arranged by the Institution of Electrical Engineers :—

"The Engineers of To-morrow," by J. R. Beard.

"The Use of Scientific Instruments," by B. A. Robinson.

In addition to the above meetings a joint debate was held, the subject being—

"Economy in Engineering."

At the invitation of Mr. E. Angus Leybourne and the Powdered Fuel Co., Ltd., a joint visit was paid to the powdered-fuel plant at the Lambton & Hetton Colliery Co.'s Philadelphia Power Station, at Fence Houses.

The Joint Committee further arranged a very successful dance which was held in the King's Hall, Armstrong College, on the 30th March. Sir William and Lady Noble very kindly received the guests.

SCHOLARSHIP FUND. BALANCE SHEET, 31ST JULY, 1922.

Liabilities.

	£ s. d.	£ s. d.	£ s. d.
Capital—			
As at 31st July, 1921 ...			1,545 5 0
Revenue Account—			
As at 31st July, 1921 ...		767 11 7	
Add			
Interest	86 8 10		
Advertisements *less* Discount	636 7 8		
		722 16 6	
		1,490 8 1	
Deduct			
Scholarship and Prizes awarded this session ...	359 4 6		
Printing and other expenses	431 7 6		
		790 12 0	
			699 16 1
Scholarship awarded—			
Balance unpaid			100 0 0
Outstanding Accounts			268 16 10
			£2,613 17 11

Assets.

	£ s. d.	£ s. d.
Loan on Mortgage to Wear Navigation and Sunderland Docks		1,500 0 0
£400—5% War Loan 1929-1947		380 0 0
Balance at Bank—		
On Deposit...	450 0 0	
On Current Account	241 2 2	
		691 2 2
Debtors...		42 15 9
		£2,613 17 11

MEASURED MILE POSTS FUND. BALANCE SHEET, 31ST JULY, 1922.

Liabilities.

	£ s. d.	£ s. d.
Amount of Fund, 31st July, 1921	90 4 11	
Add		
Amounts recovered from Employers' Associations	11 0 10	
		101 5 9
Creditors		12 15 10
		£114 1 7

Assets.

	£ s. d.
Tyne Improvement Commissioners—Loan on Bond	100 0 0
Debtors	9 17 9
Balance at Bank	4 3 10
	£114 1 7

NORTH-EAST COAST INSTITUTION OF

BALANCE SHEET,

Liabilities.	£ s. d.	1922. £ s. d.	1921. £ s. d.
Creditors		838 13 9	572 18 0
Gold Medal Fund—			
As at 31st July, 1921	357 9 1		
Interest on Investment	16 8 10		
	373 17 11		
Less Gold Medal awarded... ...	9 10 0		
		364 7 11	357 9 1
Graduates' Award Fund—			
As at 31st July, 1921	290 8 0		
Interest on Investment	12 18 11		
	303 6 11		
Less Award...	3 18 0		
		299 8 11	290 8 0
Thomas Reed Fund—			
Donation	250 0 0		
Interest on deposit	10 19 0		
		260 19 0	253 9 7
Donation by Furness Shipbuilding Company, Limited—			
Middlesbrough Branch Fund		500 0 0	
Payments by Life Members and Life Associates—			
As at 31st July, 1921		935 11 0	935 11 0
Subscriptions received in advance... ..		68 3 0	50 8 0
Suspense Account		40 11 0	47 9 6
General Capital and Endowment Fund—			
As at 31st July, 1921	10,024 17 1		
Additional donations during this Session towards the Endowment Fund of the Institution	558 8 0		
	10,583 5 1		
Less Balance transferred from Income and Expenditure Account for year ending 31st July, 1922	145 16 9		
		10,437 8 4	10,024 17 1
		£13,745 2 11	£12,532 10 3

This is the Balance Sheet referred to in our Report to the Members, dated 18*th October*. 1922.

SISSON AND ALLDEN,
Chartered Accountants.

ENGINEERS AND SHIPBUILDERS (INCORPORATED).

31st July, 1922.

Assets.	£ s. d.	1922. £ s. d.	1921. £ s. d.
Subscriptions in Arrear		221 0 6	216 10 7
Stock of Transactions—			
As at 31st July, 1921	197 7 4		
Additions	200 0 0		
	397 7 4		
Less Sales	141 1 4		
		256 6 0	197 7 4
Office and Library Furniture, etc.—			
As at 31st July, 1921	467 0 0		
Less—Depreciation	23 0 0		
		444 0 0	467 0 0
Library—			
As at 31st July, 1921	436 16 1		
Less—Depreciation	43 16 1		
	393 0 0		
Additions	22 14 2		
	415 14 2		
Books received in exchange for Transactions	26 0 0		
Cash at Bank on deposit	170 0 0		
		611 14 2	606 16 1
Investments—			
General Account :—			
Bonds of Tyne Improvement Commissioners	915 5 0		
£400. 5% War Loan, 1929-1947	380 0 0		
National War Bonds, 1928	550 0 0		
	1,845 5 0		
Gold Medal Fund—Bond of Tyne Improvement Commissioners	274 0 0		
Graduates' Award Fund—Bond of Tyne Improvement Commissioners	215 15 0		
		2,335 0 0	2,335 0 0
Thomas Reed Fund—			
Deposit at Bank		250 0 0	250 0 0
Middlesbrough Branch Fund—			
Deposit at Bank		500 0 0	.. ...
Investments of Endowment Fund—			
Tyne Improvement Commissioners' Bond...	300 0 0		
£5.547 6s. 9d. 5% War Loan, 1929-1947 ...	5,270 0 0		
£1,850 4% Funding Loan, 1960-1990 ...	1,480 0 0		
Exchequer Bonds at 5¾% (1925)	250 0 0		
£750 4% Victory Bonds at Cost	601 0 9		
Balance at Bank	0 0 9		
		7,901 1 6	7,392 13 6
Debtors		93 0 2	61 0 10
Cash at Bank and in hand		1,133 0 7	1,006 1 11
		£13,745 2 11	£12,532 10 3

On behalf of the Council:

A. Ernest Doxford, *Chairman, Finance Committee.*

R. H. Winstanley, *Honorary Treasurer.*

INCOME AND EXPENDITURE ACCOUNT

Expenditure.	£ s. d.	1922. £ s. d.	1921. £ s. d.
To Transactions, Papers and Excerpts ...	1,188 17 10		
„ Lithography, etc.	321 6 9		
„ Binding	72 3 9		
		1,582 8 4	1,545 1 2
„ Stationery and Circulars		226 10 7	398 5 10
„ Reporting and other expenses of Meetings		61 1 1	58 19 2
„ Rent of Institution's Rooms		310 0 0	310 0 0
„ Rates, Insurance, etc.		172 1 7	147 13 4
„ Salaries		2,017 10 10	1,880 17 8
„ Office Expenses, Coal, Cleaning, etc.		225 11 1	222 10 4
„ Postages, Telephones, Parcels, etc.		337 16 9	319 17 7
„ Audit Fee		12 12 0	12 12 0
„ Law Charges		60 3 0	.....
„ Travelling Expenses		30 17 9	48 8 3
„ Annual Dinner Account		15 1 3	.....
„ Reserve for Liability in respect of Members of Middlesbrough Branch		40 0 0	.....
„ Depreciation—			
Furniture	23 0 0		
Books	43 16 1		
		66 16 1	66 2 6
„ Donation to Board of Scientific Societies...		10 0 0	10 0 0
„ Donation to Faraday Society		16 13 4	.....
		£5,185 3 8	£5,020 7 10

To the Members of

THE NORTH EAST COAST INSTITUTION OF
ENGINEERS AND SHIPBUILDERS (INCORPORATED).

We have audited the Books and Accounts of the Institution for the year ending 31st July, 1922, and we report that we have obtained all the information and explanations we have required and that, in our opinion, the Balance Sheet, dated 31st July, 1922, is properly drawn up so as to exhibit a true and correct view of the state of the affairs of the Institution according to the best of our information and the explanations given to us and as shown by the Books of the Institution.

SISSON AND ALLDEN,
Chartered Accountants.

NEWCASTLE-UPON-TYNE,
18*th October*, 1922.

FOR SESSION ENDING 31ST JULY, 1922.

Income.		1922. £ s. d.	1921. £ s. d.
By Annual Subscriptions		3,160 2 1	3,247 5 3
„ Entrance Fees		55 13 0	65 2 0
„ Interest on Investments and Bank Interest of General Capital ...	148 4 1		
„ Interest on Investments of Endowment Fund	370 10 10		
		518 14 11	511 10 0
„ Income Tax Recovered		50 3 10	47 6 9
„ Transactions of current Session sold and copies of Papers supplied		185 17 1	231 7 0
„ Transactions of current Session added to Stock ...		200 0 0	144 7 6
„ Amount in respect of use of Premises		100 0 0	100 0 0
„ Books received in exchange for Transactions ...		26 0 0	26 0 0
„ Special Subscriptions from employers towards meeting deficiency of Income		742 16 0	454 1 0
„ Amount received towards cost of investigations *re* proposed Research Association		.....	72 5 0
„ Balance transferred to General Capital Account ...		145 16 9	121 3 4
		£5,185 3 8	£5,020 7 10

WAVE POWER TRANSMISSION.

(A Brief Statement of the Physical Principles Involved.)

By W. Dinwoodie.

[Read in Newcaste-on-Tyne on 27th October, 1922.]

The system of power transmission forming the subject of the following paper is the outcome of the invention of a Roumanian mathematician and engineer, Mr. G. Constantinesco, by whom the invention was announced shortly before the recent war. Financial backing was forthcoming through Mr. Walter Haddon, and an early result of their efforts was the now famous C.C. Interrupter Gear, for the control of machine gun fire through a sector swept out by the propeller of tractor type aircraft.

During the war development along commercial lines was of course much impeded, but since that time has been steadily progressing. The writer of this very incomplete description of the system is indebted to Mr. Haddon, and Messrs. W. H. Dorman, of Stafford, for kind permission to acquire the necessary information, to take photographs of plant, and other assistance.

In endeavouring to prepare a paper on the subject of Wave Power Transmission, one immediately realizes that the matter dealt with must come under two main headings, first a discussion of the theory and second a description of the practical machines and appliances which are an outcome of these considerations. While no doubt a description of the design and operation of the plant would be of greater interest to engineers, nevertheless, it is obvious that a full appreciation of the points met with in the description of the developments of the system cannot be attained without a knowledge of the principles underlying them.

Since, with the exception of alternating current electricity, the physical phenomena associated with the system differ so much from other means of transmitting power, and, in addition, because a knowledge of such phenomena adds greatly to the interest of the subject, the writer has ventured to preface his remarks by a consideration of wave motion in so far as that subject bears on the ground to be covered by the paper. In order to understand the principles of the system a knowledge of the fundamental properties of wave motion is very desirable, since under

given circumstances the waves employed undergo the same modifications as similar waves in other media. Those desirous of studying the matter more fully, and of acquiring in a most interesting form further knowledge of the subject of waves could hardly do better than read the series of lectures on "Vibrations, Waves and Resonance" delivered by Dr. Erskine-Murray before the Royal Society of Arts.

When a body executes a motion at regularly recurring intervals of time, the motion is said to be periodic. When this motion is, in addition, continually and periodically reversed in direction, it is said to be vibratory. A wave is the transmission of such a motion through a medium. Consider a straight line composed of particles of air connecting the source of a continuous note with one's ear, and assume that the sound is produced by the vibration of a plane diaphragm. On the outward stroke of the diaphragm the particles near it would be compressed, because of the inertia (or tendency to stay still) of the particles more remote, while the compression is rendered possible by reason of the elasticity of the air.

While this compression is taking place, however, the elasticity of the air causes an expansion outwards giving rise in the same way to a zone of compression slightly more remote. On the return of the diaphragm the air in contact with it expands back again until the mean position is reached. The continuation of the motion of the diaphragm beyond this point (again by reason of the inertia of the air) creates a region of reduced pressure into which air expands from more remote positions. Thus the vibrations impressed on the particles of air adjacent to the diaphragm give rise to similar vibrations in other more remote particles by reason of the elasticity, while, owing to the inertia of the air, these vibrations are each slightly displaced in time from the original. The continuation of the motion of the diaphragm results in all the particles of air between the latter and the ear performing a similar vibration with the effect of producing waves of compression and rarifaction in the air, those particles adjacent to the drum of the ear imparting their motion thereto and giving rise to the sensation of sound.

In this form of wave motion, the movement of the particles is to and fro along an axis lying in the same direction as that of the wave's progression. Such waves are called longitudinal waves, as opposed to those, such as ocean waves, where the motion of the particles of the medium is on an axis at right angles to the direction of motion of the wave, shown by a cork floating on the surface of the water. The latter are known as transverse waves, and with ripples or capillary waves have many characteristic associated phenomena which do not enter into the present consideration of longitudinal waves.

The fact that one can hear many sounds from diverse sources simultaneously and distinguish between them, proves that many waves may be propagated simultaneously through the same part of the same medium without becoming distorted. The effect, in such a case, on the particles of the medium is a displacement corresponding to the algebraic

sum of all the individual displacements due to the several waves. The phenomena is a characteristic of all such waves.

The reflection of waves is also an everyday phenomenon of accoustics, and is equally associated with waves in other media. Reflection, indeed, plays an important part in the wave system of power transmission as will be seen later when the system as practically developed is described.

In speaking of the properties of vibrations and waves the unit of time in which the motion of any particle is completed is called the period, and the number of such periods or cycles per second the frequency. Referring back to the sound analogy, when the diaphragm has passed its outer extreme position it is shown that a zone of high pressure is progressing through the medium, as a result of the vibrations imparted to successive particles of air each differing slightly in phase (*i.e.*, a time relationship) from that of the adjacent particles. In other words, the vibrations of particles successively more remote from the source, pass (in the same direction) a given point in their paths at slightly later instants.

The distance traversed by this first region of compression when the diaphragm again reaches its outer extreme is known as the wave-length and in waves of the type to be considered in this Paper this distance is almost independent of all factors except the elasticity and the density of the medium in question.

Newton's expression for the velocity of a longitudinal wave in a homogeneous medium is—

$$V = \sqrt{\frac{E}{D}}$$

Where V = Velocity
E = Modulus of Elasticity
D = Density, or mass of unit volume,

and this expression is seen to be independent of the wave-length or frequency.

It follows that the product of the wave-length and the frequency equals the velocity in any given medium.

The choice of medium in the application of this theory to practical requirements is of course unlimited. A gas or mixture of gases might be used. Liquids such as oils and water are the media at present used in development of the system, while the transmission of impulses along an enclosed steel wire was an early adaption by the inventor, to meet the requirements of a synchronized trigger release gear for machine guns. Thus, where in the course of this Paper water is specifically referred to as the medium, it must be understood that analogous effects might have been produced in any elastic media.

Of course, one must realize at the outset that the assumption generally made in hydraulics that water is incompressible is only relatively true. By comparison with gases, this is practically correct, but compared with metals it is very far from the truth. The Modulus of Elasticity of water is approximately 250,000 lb./sq. in., while for wrought iron it is about 25,000,000 lb./sq. in. Thus the ratio is of the order of 1 : 100.

In the following paragraphs, therefore, where water columns enclosed in steel pipes are considered, the expansion of the pipe is for present purposes a negligible factor of the volumetric change due to pressure.

Consider a pipe line of great length and, say, 1 inch bore, closed at one end, completely filled with water, while a fluid-tight plunger works in the cylinder formed by its other end. Suppose this plunger to be connected to a crank of small throw by a connecting rod of long length in proportion to this throw. Then, if the crank be rotated at uniform angular velocity, the plunger will reciprocate in the cylinder with very nearly Simple Harmonic Motion. The name Simple Harmonic Motion is due to the fact that most of the vibrations met with in the study of sound may be shown to be compositions of few or several of such vibrations, which have certain simple and definite mathematical characteristics.

Let OP represent the crank rotating about O, and D the plunger moving in the path aa'. Then if 1, 2, 3, 4, . . . 8, are points dividing the semi-circumference into eight equal arcs, the point P will pass

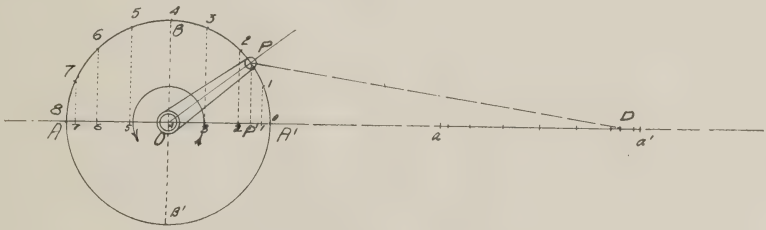


FIGURE 1

through them at equal intervals of time. The plunger will similarly move through the distance A'A in this time, but it is seen that its velocity will not be uniform but a maximum at O, and decreasing to zero at A, prior to increasing to a maximum in the opposite direction on the second half revolution of the crank OP. If O be the mean position, it is seen that at any instant represented by OP the displacement of the plunger is given by the projection OP' of OP on the line AA', or expressed mathematically, the displacement from O will always be OP cos α, where α is the angle included (anti-clockwise) between OA and OP.

If the time or angular displacement of the crank be plotted as a base and the displacement plotted as ordinates, positive to the right of O and vice versa, then the extremities of these ordinates will lie on a cosine curve (or very approximately so, depending on the ratio OP : PD). The curve is called the Harmonic Curve when used to show the relationship between the displacement of a particle executing Simple Harmonic Motion and the time; while later it will be seen that other functions of this motion will also bear similar relations to time, etc.

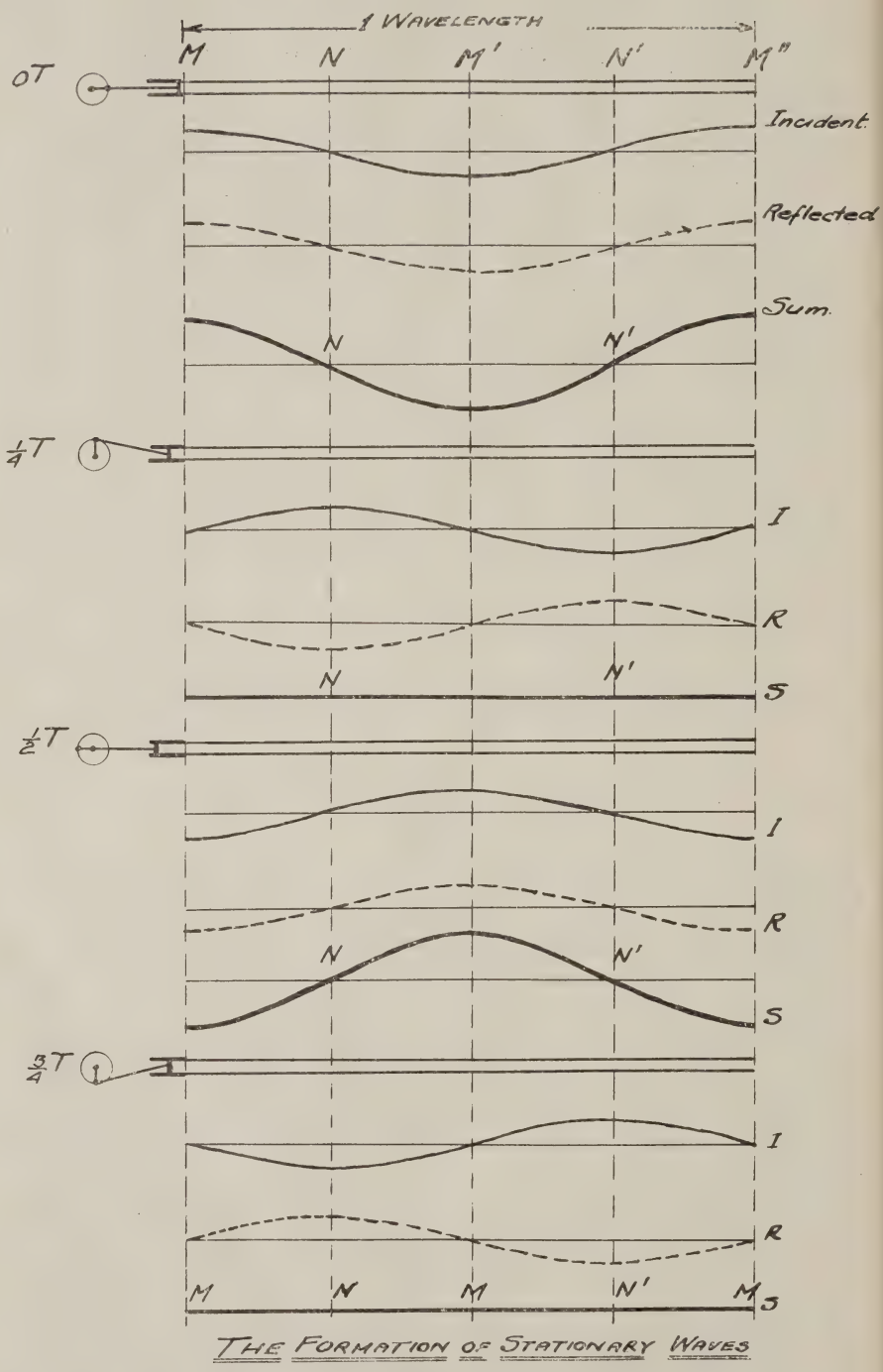

1 WAVELENGTH
M
N
M'
N'
M''
0T
Incident
Reflected
Sum.
N
N'
¼T
I
R
N
N'
S
½T
I
R
N
N'
S
¾T
I
R
M
N
M
N'
M
S
THE FORMATION OF STATIONARY WAVES

FIGURE 2.

Returning to the closed water-filled pipe, suppose the stroke of the plunger referred to is 1 inch, and the pipe 300 feet long, then the half displacement of the plunger (*i.e.*, from mean position) will be ·375 cubic inches (approximately) and the volume of the water in the pipe-line 2,700 cubic inches. If the crank were rotated very slowly this displacement would give rise to a pressure rise of about 40 lb./sq. in. In this case the effect of the inertia of the water is reduced to a minimum. Were the compression carried out infinitely slowly the inertia effect would be zero, while as the speed of rotation of the crank increases, the inertia of the water acts to an increasing extent as a buffer against which the layers of water near the piston are compressed, until with a period of 1/40th second a displacement of the order mentioned acting into such a pipe line gives rise to a compression zone whose pressure is of the order 600 lb./sq. in. This figure is given to illustrate the effect of inertia, and of course is not absolute, since many other factors influence the result.

The cycle of pressure change is, in this case, analogous to that obtaining with the vibrating diaphragm considered earlier. Here the velocity is found to be approximately 4,800 ft. per second, and thus if the frequency is, say, 40 cycles per second (to name a frequency often adopted in percussive tools), then in 4,800 ft. of medium there will be 40 zones of compression, the distance between each crest being $\frac{4800}{40}$ or 120 ft., which is, for this frequency, the wave-length.

Now consider for simplicity a pipe of this length closed at one end and filled with water and a plunger oscillating in a cylinder formed at the other end, with a frequency of 40 cycles per second. Waves of compression and expansion will be propagated through the water column, and will be reflected back from the stop. (For the first consideration of the effects of this reflection it will be assumed that the reflected waves, on reaching the generator after reflection, have no further effect on the system, while the diagram is similarly prepared only to illustrate the formation of Stationary Waves.)

In Fig. 2 the top set of curves represents the state when a zone of high pressure is leaving the plunger face. The last propagated high-pressure zone will be at the stop, and the penultimate will have been reflected back to the plunger. The incident wave is shown in a full line, and the reflected wave in a broken line, the two curves being separated for clearness. The resultant effect upon the particles of the medium is the algebraic sum of the individual effects, this condition being shown by the thickened curve below the incident and reflected waves.

The following three curves show the state of affairs $\frac{1}{4}$ cycle later. Each wave has progress by $\frac{1}{4}$ wave-length, and the sum at all points along the wave-length is seen to be zero.

The third set of curves illustrate the state of affairs $\frac{1}{4}$ cycle later, *i.e.*, after half a revolution, when the amplitudes are again additive. At $\frac{3}{4}$ revolution the two superimposed waves cancel each other and the

displacement is zero all along the water column, while the position after one complete cycle is a repetition of the conditions represented in the first set of curves.

It is seen that the points on the water column at N and N′ experience no pressure change during the complete cycle, while between these points the pressure alternates between limits which reach a maximum of twice the amplitude of the incident wave at the points on the lines MM. The condition is illustrated in Fig. 2A, where the full lines show

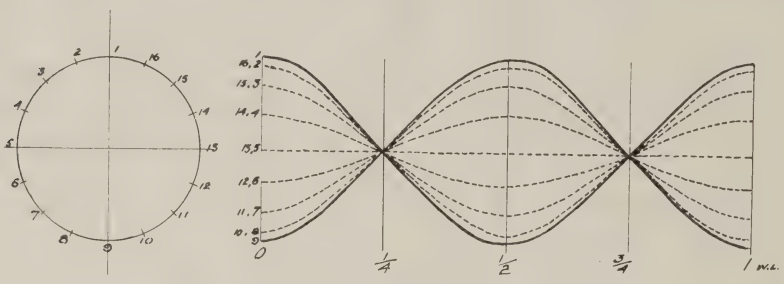


FIGURE 2A.

the amplitude of the pressure change at points along the column, while the broken lines illustrate the state of the water column at various points in the cycle. This condition, when the troughs and crests of the wave do not progress, is called a Stationary Wave. In the above circumstances, however, the effect of the continued reflection of waves has been disregarded.

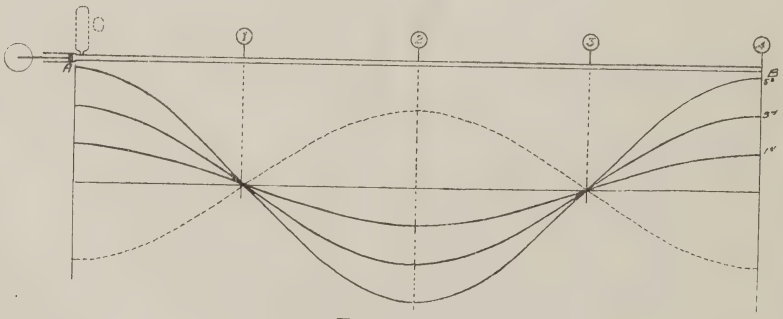


FIGURE 3.

Fig. 3 is another diagram of such a pipe line, and it will be observed that when the third high-pressure zone is leaving the plunger face, the first is arriving at that point after reflection from the stop B. A wave of twice the original amplitude is thus propagated on this cycle and the subsequent one. On the next cycle, however, the reflected wave will be of twice the original amplitude, and the fifth cycle will thus propagate a wave whose maximum pressure will be thrice that of the original wave. This process will be repeated at every alternate cycle and the maximum pressure would, in much less than a second, exceed the limit imposed by the strength of the pipe, and a burst would result.

Now the above would represent the state of affairs when the machines operated by a wave generator where either shut down or were absorbing less than the total available generator output, a condition which obtains at all times except perhaps when an attempt to overload the generator is made. It is, of course, essential that this prohibitive limitation should be overcome, and this is achieved by placing in communication with the pipe line near the generator cylinder an enclosed volume of water shown at C. This vessel is known as a "capacity" and is analogous in its action to a condenser in alternating-current electricity or a spring in mechanics. The inward stroke of the plunger causes an inflow to the capacity in addition to the propagation of a pressure wave along the column. This inflow causes a rise in pressure in the capacity and a corresponding storage of energy. As the revolution of the crank proceeds and the pressure at the plunger face falls, the capacity, by reason of the elasticity of the water and the pressure difference, returns this energy to the system in expanding once more to normal pressure. This inflow to the capacity at instants of high pressure in the neighbourhood of the plunger, and the return of the energy thus stored on the reverse stroke of the generator is, by suitable design, made to correspond to the total generator output when the stationary wave of the desired amplitude has been produced in the closed pipe line. The output of the generator under these circumstances is theoretically zero; actually, of course, energy is supplied to maintain the stationary wave, and to compensate for the energy dissipated by internal and inter-molecular friction, etc.

This arrangement, the description of whose operation is necessarily incomplete since its action if considered in detail is dependent on many other factors, satisfactorily provides for the independent stopping and starting of machines operated from a pipe line. The next point arising is the facility with which machines may be tapped in on such a pipe line.

Consider a pipe line in communication with which are four wave-power tools, fitted with stop cocks, tapped in as shown in Fig. 3. It will be remembered that in the consideration of the stationary wave it was shown that the particles at $\frac{1}{4}$ and $\frac{3}{4}$ wave-length were permanently at the mean pressure of the system, while those at multiples of half wave-lengths experienced a maximum pressure change of twice that of the original wave. As might be anticipated, machines tapped in at points 2 and 4 will be self starting, and will be stable in running, while those at 2 and 3 will not start when placed in communication with the stationary wave. If, however, the motor at 4 is in operation, then a progressive wave will be superimposed on the stationary wave, and since the nodes and antinodes of such a wave will travel along the column, at no point therein will the pressure change be permanently zero, and the machines at 1 and 3 will start. Were (2) the only machine in operation, then machines between it and the generator would be self starting, while beyond (2) the stationary wave alone would persist.

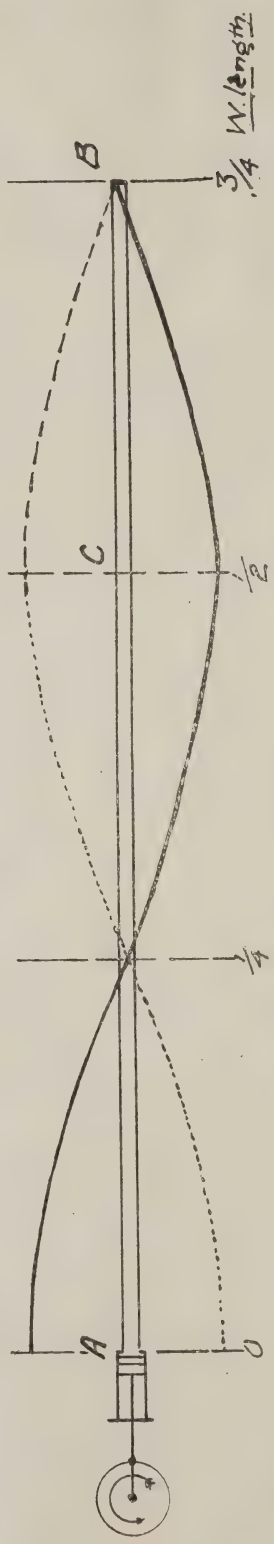


FIGURE 4.

The conclusion, therefore, is that to be stable in running and self starting, machines should be placed in communication with a transmission column at points which are multiples of half wave-lengths. Similarly in order to enable a stationary wave to be set up and maintained in the transmission column, the length should be similarly governed.

The question of the limits between which such dimensions must be observed next arises, and the information is forthcoming on consideration of the effects produced by pipe lines of lengths other than multiples of half wave-lengths. Instead of a generator running at 40 cycles per second on a pipe line of 120 ft. (one wave-length) suppose that the line is reduced to 110 ft. It is seen that a stationary wave cannot normally be produced with the frequency unaltered. The result is that the generator takes the easier course, as it were, and the speed tends to adjust itself so that the length of the transmission column is in effect a multiple of half wave-lengths. In the case cited above, the frequency (*i.e.*, the generator speed) would tend to become that for which 110 ft. represents the wave-length, and since the velocity must remain constant, this frequency would be 4,800/110 or 43·7 cycles per second, an increase of speed of about 9 per cent.

In order that the frequency shall remain constant the speed of the generator is governed within a variation not likely to exceed 5 per cent., and, in the effort to resist the tendency to speed adjustment, much power will be wasted; but had the line been composed of 470 ft. in lieu of four wave-lengths (480 ft.) the adjusted wave-length would be 117·5 ft. Thus the same linear error in the pipe line would result in a speed increment of only 2 per cent. The effect of such difference from multiples of half wave-lengths is thus proportional to the ratio between the difference and the total length of the pipe line, and it is found that beyond 300 ft. (with the frequency 40) the governing of the generator permits of this adjustment without loss of power whatever the length of the pipe line.

With a permissible speed variation of 5 per cent., therefore, one may run with a line of within 5 ft. on one wave-length, this latitude being increased in direct proportion to the number of wave-lengths, while the maximum possible deviation from a half wave-length (*i.e.*, 30 ft.) is permissible at lengths of 360 ft. and over.

It is important to note the effect when a pipe line much outside the limit allowable is used, say one of $\frac{3}{4}$ in lieu of a wave-length. Here, of course, the generator cannot possibly adjust its speed by the requisite amount. Such a column is shown diagrammatically in Fig. 4. If T seconds be the period of the oscillating plunger and at O seconds a zone of high pressure is assumed to be leaving the plunger face (with the capacity pressure at its maximum), then at T seconds this zone will have reached the stop B and will have been reflected to C. After 1·5 T seconds this reflected zone will have reached the plunger face, where normally the pressure would have been a minimum. The arrival of

the reflected wave will have had the effect of maintaining the pressure at the plunger face at its mean value, thus preventing the outflow from the capacity. Subsequent cycles will repeat this process, while the capacity will, in effect, be cut out of the circuit with the speedy result of a burst becoming possible.

This concludes a brief description of those physical principles which underly the laying out of a Wave Transmission system. It now remains to discuss the operation of suitable " receivers " or means of converting the energy of the arriving waves into useful work.

In order that a machine shall be capable of operating thus it must be " resonant " to the waves supplying energy to it, or in other words, capable of utilizing at all instants the effort of the supply, in whatever sense or direction it may tend to act. The action of a pendulum is analogous. If a pendulum be struck lightly in one direction at intervals which correspond to the natural period of vibration, it is able to absorb the energy imparted and by its absorption do " useful work," *i.e.*, develop a correspondingly greater amplitude. If the impulses were caused to arrive much more rapidly and at irregular intervals of time, the energy dissipated would be much more, while the amplitude would be negligible. This principle is often applied in the removal of the garden roller from a rut in which it has come to rest.

Wave-power machines may be classified under headings as follows :—

1. *Reciprocating*—(*a*) Continuous Wave—where the generating plunger moves with maintained Simple Harmonic Motion.

(*b*) Impulsive Wave—a type in which a single harmonic motion is transmitted in wave form at regular intervals, greater than the period of the motion itself.

2. *Rotary*.—Under this heading will come types of Synchronous and Asynchronous motors, monophase and polyphase, in which direction research is at present proceeding.

Of course, many other classifications might be made, but these will suffice for the brief descriptions possible in a paper of desirable length.

In the following paragraphs an installation transmitting power by continuous waves will be described. Of such applications most attention has been paid to the development of a rock drill of the hammer type, and this form of reciprocating tool will be dealt with as a representative type.

In order to propagate waves of harmonic form in an enclosed water column, a surface in contact with the column is given simple harmonic motion. For obvious practical considerations the requisite displacement is distributed over two cylinders in which plungers are reciprocated by means of two cranks set at 180 degrees. This part of the generator may be visualized as resembling a horizontally opposed high-speed plunger pump, with each cylinder communicating with a large water vessel (the capacity) from which the transmission line is lead away.

The whole system from the generator plunger to the tool plunger, is to be filled with water, air being eliminated. To accomplish this,

and the lubrication of the generator bearings, a transverse shaft is driven by a worm on the front end of the crankshaft, engaging with a worm wheel, and upon this two cams are formed, also at 180 degrees. Two plunger pumps working against return springs are operated by these cams, through suitable tappets, the left hand pump forcing oil to the bearings and the other supplying water to the system, and providing for the maintenance of the volume. Chiefly in order to facilitate the elimination of air, and to maintain the stresses in one direction, a minimum pressure of 100 lb./sq. in. is maintained in the pipe line. The water pump must therefore be capable of a delivery when the capacity pressure falls below this, and must be so arranged that the delivery may be variable to correspond with the varying deficiency due to the water flush used with the drill and unavoidable leakage.

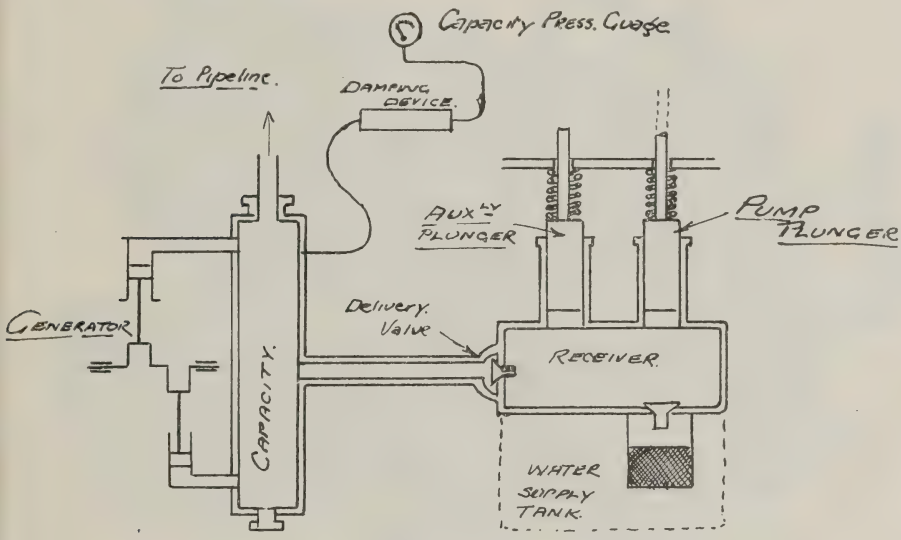


FIGURE 5.

These conditions are satisfied by the addition of an auxiliary plunger of similar displacement, working against a spring which is set to lift at a pressure of 100 lb./sq. in. This is placed in communication with the delivery chamber of the water pump, as shown in Fig. 5. This illustrates (of course, diagrammatically) the water system of the generator. On the upward stroke of the pump plunger, water is drawn in from the sump through the inlet valve. On the downward stroke, in the event of the minimum pipe line pressure being equal to, or more than 100 lb./sq. in. the delivery valve does not open, but the auxiliary plunger is raised against its spring. The whole of the displacement of the plunger is thus stored at a pressure of 100 lb./sq. in. or a little in excess of this. As long as the auxiliary plunger is lifted from its seat (*i.e.*, during most of the stroke) the pressure in the delivery chamber is 100 lb./sq. in., while the delivery valve to the capacity is a light disc valve operated by pressure difference, and will admit water to the

system at moments when the pressure therein falls below the minimum desired.

To facilitate the charging of the system and the elimination of air, air release valves are fitted at the highest point of the water circuit, over the generating cylinders and, if necessary at convenient points in the pipe line (similar air release valves are found in the water chambers of the rock drill). The transmission line is connected to the capacity through a stop cock, by means of which the pipe line is disconnected from the generator while the latter is attaining full speed, or while it is being shut down. One necessity for this is due to the

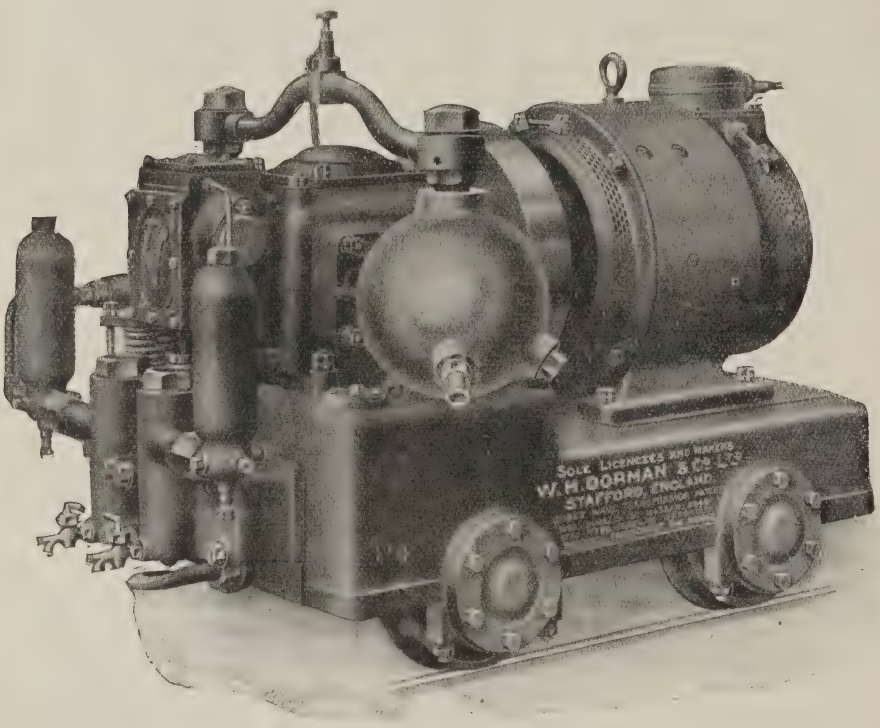


FIGURE 5A.

fact that a pipe line of $\frac{1}{2}$ wave-length, say, will, at half generator speed (therefore half frequency), behave as a quarter wave-length. Since the generator must pass slowly through all speeds while starting up and stopping, it follows that at some speed the effect of a quarter wave-length would be produced were the pipe permanently in communication with the generator. A very few cycles near this frequency would, it has been shown, result in a burst due to the cutting out of the capacity.

The above are the principal features of the generator sectional and other views of which are shown in the appended illustrations.

The pipe line of a wave-transmission installation may be built up of any piping of suitable bore whose walls are capable of withstanding the

pressures adopted. As in the case of electrical transmission, the cross section of the pipe required depends on the power to be transmitted and vice versa. The relationship is (approximately) that the power a pipe of given bore is able to transmit varies directly as the cross-

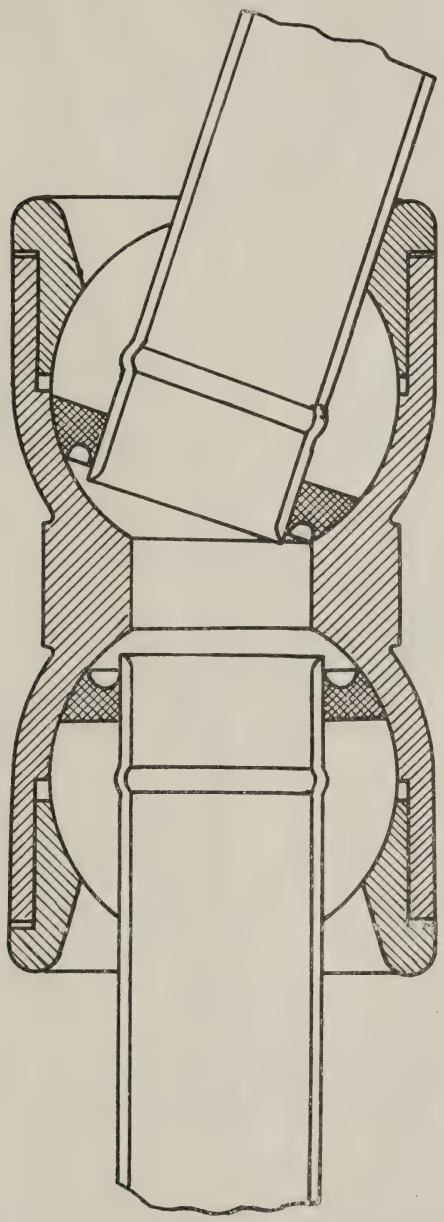
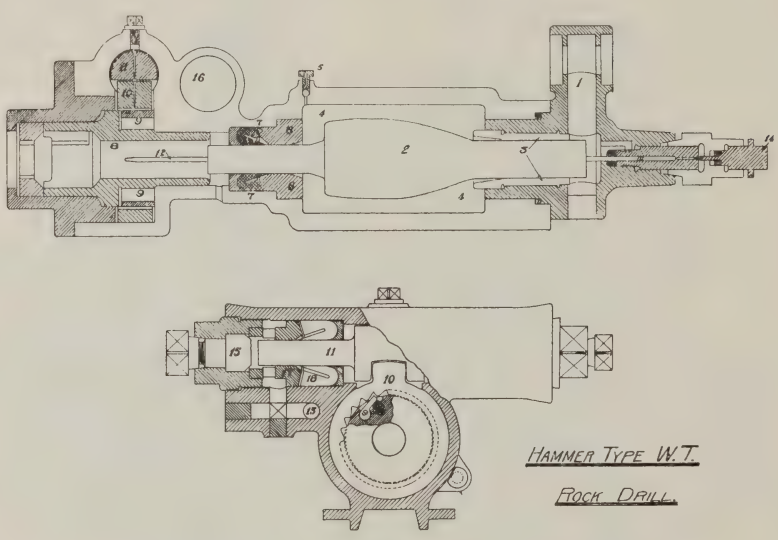


FIGURE 6.

sectional area, or as the square of the diameter. An idea of the numerical values found to obtain is given by the following case. With a pipe of one inch bore, a frequency of 40 cycles per second and a pressure range of 1,500 lb./sq. in., it is possible to transmit up to 15 h.p. without loss of efficiency.

Where flexibility of the pipe line is desirable (*i.e.*, near the generator and the tool, where the tool is of a portable nature) a piping such as that illustrated in Fig. 6 is employed. At the ends of two lengths of solid-drawn steel tubing, flatted ball pieces are pressed on and secured. These fit into spherically seated recesses machined in a union piece, and are secured by a suitably shaped retaining nut. A special packing ring is pressed over the projection of the tube beyond the flatted ball piece as shown. The piping does not tend to straighten out under pressure, while a length tested to destruction at the National Physical Laboratory which yielded at 9·2 tons per square inch, demonstrated that the walls of the tube give way before the joint or the packing.



1. WAVE INLET.
2. HAMMER.
3. MEAN PRES. LEAK BUSH.
4. CAPACITY.
5. AIR RELEASE VALVE.
6. STOP BUSH.
7. HAMMER PACKING.
8. CHUCK.
9. PAWLS.
10. RATCHET RING.
11. ROTATION PLUNGER.
12. WATER TUBE.
13. INLET TO ROTATION.
14. WATER FLUSH CONTROL.
15. STOP (R.P.).
16. ROTATION CAPACITY.
17. ,, M.P. LEAK BUSH.
18. ,, PACKING.

FIGURE 7.

This form of tubing is particularly suited for use with wave-transmission installations, since high alternating or mean pressures do not tend to straighten it out, as they would a hose, while sections may be added or removed as desired.

The rock drill which is to be dealt with as representative of reciprocating machines in general, has, of course, much in its design peculiar to the very severe duty it is called upon to perform. The scope of this paper, however, does not permit of a discussion of such points. The tool, of which a general view is shown in Fig. 7, consists of four main elements, a reciprocating hammer, a means of holding the

drill steel, a means of rotating this steel in a manner independent of the hammer blow, and a means of supplying a jet of water at mean pressure to the hollow drill steel. The hammer (*vide* Fig. 7) is a double-ended plunger rendered massive in the central section, the foremost or striking plunger being of smaller diameter than the rear. This is held, as shown, in two bushes (a packing being provided at the front end) and surrounded by a suitable water space. The rear of this hammer enters a space with which the wave may be placed in communication, by means of a suitable cock. On the arrival of the impulse this hammer is driven forward into the capacity and strikes the drill steel, while by reason of the difference in diameter of the rear and front plungers, the water in this capacity is compressed. As the pressure falls in the pipe line, this energy stored in the capacity acts on the differential area and forces the hammer back to its former position in readiness to receive the subsequent impulse. The water surrounding the hammer body acts as a fluid spring, absorbing energy in one half cycle which it returns to the system in causing the return stroke of the hammer. This cycle is repeated at the frequency of the system; in the case considered this results in 2,400 blows per minute.

The rotation of the drill steel must be carried out in such a way that there is no force tending to produce rotation during the striking movement of the hammer. This is carried out by the mechanism shown in Fig. 7.

The drill steel is provided with two lugs on opposite sides of the shank, this fitting loosely into a member known as the chuck, in such a manner that the rear of the steel abuts the striking face of the hammer. Pawls are arranged on the outside of this chuck, which is embraced by a ratchet ring having a lug formed on it as shown. This lug is received in the slotted barrel of a double-ended plunger, one end of which is placed in communication with the incident waves by means of a channel to the inlet chamber, while the other works into an enclosed volume of water (*i.e.*, a capacity). The pawls are set so that the incident wave causes the plunger to rotate the ratchet ring through a small arc without rotating the chuck. This movement must, of course, sychronize with the blow of the hammer, and gives rise to an increase of pressure in the capacity, which, during the following half cycle, causes the return of the plunger, and, by the engagement of the pawls with the ratchet ring, the rotation of the chuck and drill steel. This rotation, it is seen, must synchronize with the return stroke of the hammer.

The water flush is carried out by means of a water tube passing centrally through the drilled hammer from the rear of the inlet piece. A small passage to the rear of this tube is provided, such that in passing, the pressure alternations are almost damped out, and a needle valve, operated by the drill runner, is fitted by which the volume of the flush is controlled.

This concludes a very brief description of the Drill, which, together with the description of other plant, the writer hopes to augment at the time of the delivery of this Paper. A complete description on this side of the subject would occupy space equivalent to a paper of unwieldy size, and thus it would appear better to allow these points to be dealt with verbally when time need not be wasted on description of machine parts and general arrangements, which are readily apparent from the illustrations it is hoped to provide.

DISCUSSION.

THE CHAIRMAN, Mr. Harold Thomson, *Vice-President*, said that it would be agreed that Mr. Dinwoodie had delivered a very interesting lecture, and he could only hope the members present had all followed the lecture as clearly as Mr. Dinwoodie had delivered it. Personally he did not feel quite capable of criticizing some of Mr. Dinwoodie's earlier remarks. Wave transmission was a very difficult subject, whether applied to light, sound or fluids; but he had no doubt there would be some members present who would like to ask certain questions. He was not quite clear about one or two small points himself. For instance, what happened if air got into the system? Mr. Dinwoodie had on his machine two air vessels, which, presumably, contained air; and if one could afford to have air vessels here did it make very much difference if air was present in other parts of the system? Again, if this system was adopted in shipyards, for working drills, etc., would it be necessary to study the theory of wave transmission before being able to lay out the piping system? He rather gathered that under certain conditions matters corrected themselves, but he was not quite sure on that point. If engineers in shipyards and works had to make calculations, regarding the positions of nodes and so on before they added additional tools to a system he wished them luck, but was rather glad he had not to do it himself as the matter seemed rather complicated. Mr. Thomson

In regard to variations in speed, would it be impossible to drive a machine of that kind with a constant speed in the motor? He rather gathered that the machine slowed itself down, and there might be some difficulty if they were using a machine with alternating current motors. With regard to rock drills, was it necessary to calculate the system of pipes so that these were in correct proportion to the wave lengths? In the event of the wave transmission system being developed, could portable hose of a rubber character be used? Would the expansion of the rubber pipe have a detrimental effect on the wave system? The Author said that the maximum pressure was 1,600 lb. to the square inch. Did that mean that the distribution pipes had to stand that pressure? If so, it might present difficulties in some classes of work.

Mr. Dinwoodie had explained that the firm had not yet made a complete study in order to apply the system to riveting and caulking tools and had really concentrated on rock drills with which they had had very satisfactory results, and the rock drill was now very efficient and he (Mr. Thomson) wished them every success in developing it further. He hoped some of those present who were enthusiasts on the subject of pneumatic rock drills would promptly tackle Mr. Dinwoodie and explain to him how much better the pneumatic tools could do the same kind of work, and ask the Author any questions which they might feel inclined to do.

Mr. Thomson

The Chairman asked, later, whether it was possible to tack an extra tool on to the system at a moment's notice.

Mr. Wilson

MR. R. W. WILSON asked what length of stroke the piston would make when it struck a blow.

Mr. Myles

MR. DAVID MYLES, *Fellow*, said, in connexion with the statement that a wave at sea was not a forward movement of the particles of water, that those who had been down at Tynemouth during a north-easterly gale must have been quite convinced that the water did move forward; but it had to be remembered that the conditions there were quite different from those obtained in deep water. At sea, where there was depth of water in proportion to the wave, it would be found that there was no forward movement of the water, and it was only when the wave came into shallow water that the particles of water moved forward. He had met men of considerable experience who did not agree with this and mentioned the effect of a wave striking the side of a ship, but they overlooked the fact that the ship obstructed the free movement of the wave and so caused the water to come on board.

If the branch pipe for driving any of the machines was taken off from the main pipe, the action of the water in the main pipe would set up another wave motion in the branch pipe. That, it seemed to Mr. Myles, would have a very disturbing effect on the wave action in the main pipe. Perhaps Mr. Dinwoodie would state whether that was so or not, and if it was so, what the effect of it was. He thought the Chairman had mentioned that so far there has not been much experience with ordinary chipping or caulking or riveting hammers, but it would be interesting to know if there had been any trials with those tools and what the results were—whether they appeared to be encouraging or otherwise. It seemed to him that wave motion ought to be a very economical method of transmitting power from the generator to the user, and it would be interesting to know if Mr. Dinwoodie had any figures or any information on that point.

Mr. Kimber

MR. H. G. KIMBER, *Associate Member*, said that the flexible pipes shown on the screen consisted of a large number of the special flexible joints mentioned by the Author. In practice one would expect such joints to develop slack and also to rust. If either of these things occurred there might be rather excessive leakage which would interfere with the transmission of power.

A figure was given in the Paper for the modulus of elasticity of water. When working at the high pressures peculiar to this system was it possible to exceed the elastic limit, and if so what happened?

Mr. Hobson

MR. J. W. HOBSON, *Member*, asked Mr. Dinwoodie if he would kindly explain what provision was made for working the plant in frosty weather.

Mr. J. C. ARTHUR, *Chairman of the Graduate Section*, asked Mr. Dinwoodie if he could give some figures as to the actual transmission efficiency of the wave transmission plant. Another point was this: Since the generator was a very high-speed pump working against a rather solid resistance, was any trouble experienced with a vibration of the generator itself? It seems probable that if any slackness developed in the connecting-rod bearings the generator would shake itself to pieces very quickly. Mr. Arthur

VOTE OF THANKS.

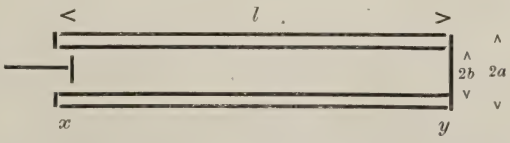
The CHAIRMAN (Mr. Harold Thomson, *Vice-President*) said they had had a most interesting Paper and, if possible, an even more interesting reply to the discussion. They were very much indebted to Mr. Dinwoodie, and he was sure he was expressing their wishes when he asked them to give him a most hearty vote of thanks. Mr. Thomson

CORRESPONDENCE.

Mr. H. G. GREEN (University College, Nottingham).

In the theory of wave transmission one of the important quantities is the expression of capacity, or the effective changes of volume for unit changes of pressure. In it there have to be considered both the contraction of the liquid itself and the expansion of the containing vessel. In the latter factor the investigation given in the Theory of Wave Transmission, p. 22 (G. Constantinesco), is not fully satisfactory as it is assumed that the mass of the walls of the tube can be concentrated on the inner surface, irrespective of the thickness, and, what is more important, that radial contractions are independent of longitudinal tensions. The quantity involved is small, but the whole term is so fundamental that initially, at any rate, it should be carefully determined. Mr. Green.

We will consider a straight cylindrical tube of length l, of internal radius b and external radius a, containing water. A pressure p above normal is subsequently applied by means of a piston.



Notation.—E Young's modulus, μ rigidity, κ volume elasticity, σ Poisson's ratio.

Case I.—The pressure of the piston on the tube itself is supported at x, *i.e.*, the tube is under a corresponding longitudinal tension.

Mr. Green

Then under the hydrostatic pressure p the internal radius is increased by

$$\delta b = \frac{b}{a^2 - b^2} \cdot \left(\frac{a^2}{2\mu} + \frac{b^2}{3\kappa}\right)p.$$

and the length by $\delta l = \dfrac{lb^2p}{3\kappa(a^2 - b^2)}$.

[See Lamb's Statics, p. 338, Ex. 4, and p. 334.]

If l^1 be the new length for the same volume enclosed

$$\text{then} \quad l^1(b + \delta b)^2 = lb^2$$

$$\text{or} \quad l - l^1 = \frac{2l\delta b}{b},$$

and we have for the movement of the piston

$$\frac{2l}{b} \cdot \frac{b}{a^2 - b^2}\left(\frac{a^2}{2\mu} + \frac{b^2}{3\kappa}\right)p + \frac{lb^2p}{3\kappa(a^2 - b^2)}$$

$$= \frac{lp}{a^2 - b^2}\left(\frac{a^2}{\mu} + \frac{b^2}{\kappa}\right).$$

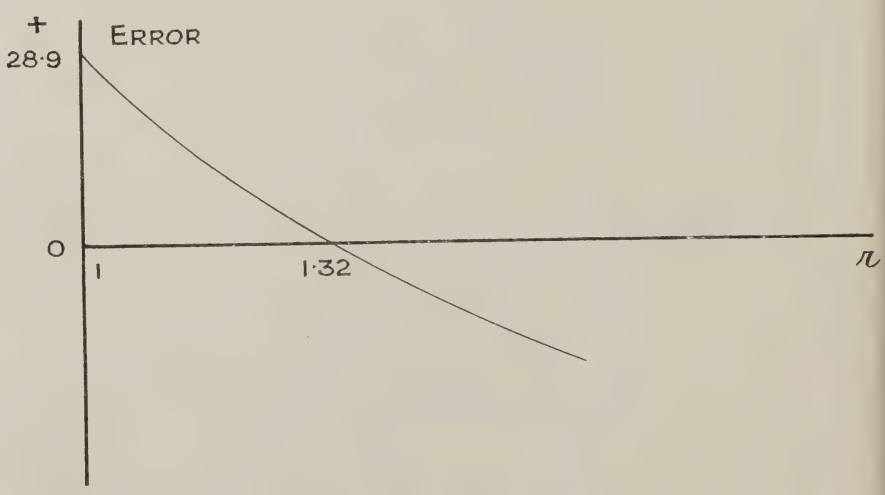
Constantinesco's method gives $\dfrac{2 \cdot 5\, lpb}{\mathrm{E}\,(a - b)}$.

The percentage error in this result is therefore

$$\left\{\frac{2{\cdot}5\,(1 + r)}{\left(\dfrac{r^2}{\mu} + \dfrac{1}{\kappa}\right)\mathrm{E}} - 1\right\} 100 = \left\{\frac{2{\cdot}5(1 + r)}{2(1+\sigma)r^2 + 3\,1 - 2\sigma)} - 1\right\} 100$$

where $\quad r = \dfrac{a}{b}\,(> 1)$.

For wrought iron (·1 °/₀ C) σ = ·280 [Kaye and Laby's Tables]; the error is zero for r = 1·32 and tends to 28·9 °/₀ as r tends to 1.



In the case given (Constantinesco, *Wave Transmission*, page 4) r = 1·4 (near the critical point).

New result gives 1·598 cms. } Supposing the pressure given as
Constantinesco's 1·511 cms. } that *above* normal.

CASE II.—The piston pressure supported at y, *i.e.*, the tube not under longitudinal tension Mr. Green.

The general equations of elasticity lead to a result for the movement of the piston.

$$\frac{lp}{a^2 - b^2}\left(\frac{3a^2 + b^2}{6\mu} + \frac{2b^2}{9\kappa}\right).$$

while Constantinesco's assumptions would lead to

$$\frac{lbp}{\mathrm{E}\,(a-b)}.$$

The percentage error is

$$\left\{\frac{1 + r.}{\left(\frac{3r^2 + 1}{6\mu} + \frac{2}{9\kappa}\right)\mathrm{E}} - 1\right\}100 = \left\{\frac{3\,(1 + r)}{(3r^2+1)\,(1+\sigma)+2\,(1-2\sigma)} - 1\right\}100$$

which tends to zero as r tends to 1.

Note.—In any experimental work, if the piston positions were measured relative to the free end of the pipe further allowance would have to be made of amount $\frac{2\sigma\, b^2\, pl}{(a^2-b^2)\,\mathrm{E}}$ for the longitudinal contraction of the pipe.

It is clear from the above results that the assumptions for the concentration of mass in calculating the stresses are justifiable for and only for very thin walled tubes, and that the neglect of radial effects due to longitudinal tensions leads to considerable discrepancy.

It is a matter of great importance to investigate this give of the pipe walls in the dynamical problem. The mathematical investigation, which is somewhat involved, indicates a considerable reduction in the velocity of propagation of the waves. Experiments have been made on the velocity of sound waves for water in glass tubing by Kundt and Lehmann, and by Dvorak,* which show reductions of velocity of reasonable agreement with those deduced by theory. The agreement is, however, not close enough to allow calculation to replace experiment for exact purposes and can only be used as indicating the order of the change and as a general confirmation of the theories employed. In the notation of Mr. Constantinesco we must have then the physical constants of the tube entering not only into the capacity but also into the inertia of the pipe line.

While the necessity for condensers on the line is patent the general reasons given do not seem quite satisfactory. The question is hardly so simple as that of a single reflection while the idea of an indefinitely large accumulation of energy is contradictory to one of the great points of the system—that of the supply being automatically adjusted to the demand. What really happens is that in a case of excess supply a corresponding stationary wave is established for which work done in one part of the stroke is recouped in another part.

Some experimental data of this automatic adjustment and of the

* See Barton's *Sound*, pp. 533-5.

Mr. Green

efficiencies of motors and pipe lines working at rates different from those of their design would be valuable, for although a complete installation would probably eliminate much of the extra power needed after week-end stoppages and the like, occasions must still arise when variations must be made. Knowledge also is needed of the efficiencies working with oils suitable for low temperatures before wide application can be contemplated in this country.

MR. DINWOODIE'S REPLY.

The Author

Mr. DINWOODIE thanked the meeting very much for the kind way in which they had received his Paper. It was very kind of them to listen to him as they had done, considering he had no experience of the tools and operations in which they were most interested.

Reply to the Chairman's first remarks.—The presence of air in the transmission column would have a very bad effect on the efficiency of transmission. Thus provision is made for its easy elimination. The air vessels mentioned are on the discharge side of the supply pumps, and, apart from the fact that the air is trapped in a dome, this air is not in any way in communication with the water column. Air *in the column* induces surges of high pressure, distorts the wave, and gives rise to peak pressures much in excess of those ordinarily experienced. An oscillograph record of the pressure changes in the water column with air present was shown on the screen to illustrate this point.

The only point on which men in charge of a wave-transmission installation need be informed (with regard to length) is the number of feet in the half wave-length. The total distance from tool to generator should then be arranged to be a multiple of that unit. With regard to operation it should be noted that generators should be started up and shut down with the transmission column out of communication with same; this for the reason that, with the alteration of speed the frequency reaches values at which the actual length of line becomes an odd multiple of quarter wave-lengths with the effects already described, *i.e.*, the possibility of a burst taking place.

It might be noted that the velocity of the wave in any medium, divided by the frequency, would give the wave-length. This distance would in any case be constant for any installation.

With regard to speed, if the installation is set out in accordance with the principles indicated, the generator would *resist* any tendency to alter the speed. Thus constant speed in the prime mover is desirable. The smaller the speed variation in the prime mover, the more precise must the measurement of the length of the column be. In a case where the wave-length was, say, 120 feet, and the generator was running at a speed of 40 cycles per second, there would be no tendency for speed

variation. If, however, the line were increased to 130 feet, the speed would tend to that value at which the wave-length would be 130 feet, since 4,800/130 = 37 cycles per second approximately. In that case there would be a tendency, therefore, to slow down. If carried into effect then there would be no further tendency to alteration. In using synchronous motors this point would not arise were the distances appropriate to the speed in the first place. The Author

It is necessary to calculate as indicated, the lengths of transmission column in the case of rock drills, so that these bear the stated relation to the wave-length, within limits as defined by the governing of the prime mover, if the equipment considered be that ordinarily in use at the present time for rock drilling. By the inclusion of suitable capacities rock drills or other tools may be operated independently of the distance between them and the generator. Since this point lies outside the ground covered by any experimental work known to the Author it has not been included in the Paper. It would, for reasonable treatment, demand an altogether more careful consideration of the theoretical aspects of the system.

It seems, in the Author's opinion, that rubber hose would be impossible for the efficient transmission of power by reason of the expansion which would take place.

With regard to the use of a maximum pressure of 1,600 lb. per square inch, the Author has not heard of a burst having taken place due to this maximum pressure. Solid drawn steel tubing was in use.

An extra tool may be added to the equipment at any moment provided that :—

(1) If for use simultaneously with the others, the output of the generator at full load is sufficient.

(2) If not for use simultaneously with others the power required does not exceed the generator output at full load.

Reply to Mr. Wilson.—The hammer is arrested at half displacement, so that if the particles in the pipe line had an amplitude of 23 mm. the hammer would have a displacement of half that—11½ mm. The dimensions quoted are those of the D.O.I. Generator (which was shown on the screen).

Reply to Mr. J. W. Hobson.—In *working* the plant in frosty weather no precaution need be taken, since freezing is ordinarily impossible while the water is in a state of vibration.

In the intervals between shifts, however, it would be necessary to adopt the most economical of the following plans :—

(1) Empty the pipe line at the end of operations, and recharge when next starting.

(2) Bury the main length of the transmission line, and suitably lag the remainder with non-conducting material.

The Author

(3) In certain cases, adopt a non-freezing medium. In one application of the system (C.C. gear for machine guns) a mixture of oils was used. Brine might be a possible medium.

(4) Leave the generator running light.

Reply to Mr. H. G. Kimber.—The joints mentioned were, of course, used only where necessary. The Author might have given the impression that the line was composed of joints at about 5 inch centres with 1 inch bore piping, but that was an error possibly due to the slide. At a quarry it was found most convenient, for example, to use joints at 12 feet centres (12 feet) for the main line.

In the case of slackness developing, the fact would be visible by reason of the leak, and the tightening of the retaining nut would be the cure. The removal of any one such nut exposed the packing, should this need replacement.

With regard to rusting of the joints and leakage, it would be an economy in a case of a plant of this nature to provide for the periodical inspection and greasing of the joints. The time taken would in any case be small. The joints having surfaces in contact and not a line contact would not be susceptible to leakage due to pitting, and grooving was very unlikely. Were the motion of the water of the same nature as the flow in the case of hydraulic machinery, of course, these joints would give rise to prohibitive losses. It is probable that the power is transmitted mainly through the central portion of the water, the amplitude at the periphery being very small. Again, fluids have the property of transmitting pressures equally in all directions.

With regard to the elastic limit, if the Author could have been informed as to the point at which the alteration of the volume of water under pressure gives rise to a permanent "set" or distortion, he would feel more able to go on with the reply.

Reply to Mr. J. C. Arthur.—With regard to the efficiency, an experimental method was described in which the motion of the hammer of the rock drill was recorded by a scriber caused to bear on the rim of the generator fly-wheel. From this curve the velocity of impact of hammer was deduced, and with the weight of the hammer, the number of blows per minute, and the electrical in-put to the motor, provided a means of obtaining a figure for the overall efficiency. With the type of drill then used—a much simpler hammer mechanism—figures between 80 per cent. and 85 per cent. were obtained. These results cannot, of course be universally applied. The later rock drills have not been the subject of such tests, while in general the absence of instruments analogous to the ammeter, voltmeter, and watt-meter of A.C. electricity much hamper the preparation of numerical data.

With regard to vibration, the stresses are in one direction on each connecting-rod, the minimum pressure being 100 lb. per square inch.

They are also completely balanced. A visitor to the works might check this by balancing a penny on its edge on a generator when the latter is operating a rock drill. This has frequently been done. The Author

Reply to Mr. D. Myles.—With regard to an analogy illustrating the fact that wave motion does not of itself involve flow of the medium considered as a whole, the transmission of a sound across a room, or through partitions seems the clearest. If the total distance from the tool to the generator were always a multiple of half wave-lengths, then in all cases of branch pipes the reflected waves would arrive at the generator at appropriate moments in the cycle. The only point arising is that it becomes more advisable to have such distances within a smaller limit of error, so that the speed which is normal for any one branch will be the universal speed to which all branches will tend.

With regard to the tools mentioned, the Author is not able to speak with knowledge of the plan of development which the firm (Messrs. W. H. Dorman & Co.) are following, but he thinks that it is felt that the one line of research and development, *i.e.*, the cradle-mounted rock drill, is a sufficient undertaking for an individual firm at the present time, trade conditions being as they are. As to why the rock drill was selected the Author is unable to say, but could point out that the subject is more complex than riveting, caulking and chipping hammers. The point revealed by the experimental models made is the need for making the generator of suitable design for the tools, and the power transmitted in proportion to the demand so that the vibration experienced by the operator is reduced. Vibration, due possibly to these factors, was the chief difficulty in the case of these first designs, and it has not yet been decided to proceed with this side of the question. The rock drill itself is the greatest encouragement with regard to the success of such tools.

Reply to Mr. Green.—At the present time (the Author understands), it has not been possible to bring research work on the subject of wave transmission into line with Mr. Constantinesco's mathematical analysis. In this connexion it might be said that practical work is as yet on the very fringe of the subject.

With regard to the final paragraphs of Mr. Green's remarks, the Author would emphasize the fact that the treatment of the question of condensers or capacities was far from complete. A statement to this effect is included in the text, lest the latter should be considered as anything more than an indication of the effects noted. Such considerations were reduced to the minimum necessary to introduce a description of the plant operating on the principles outlined, and of the general characteristics of such. It is hoped that it was made clear at Newcastle that in the case of varying output with a constant supply the effect was that of the combination of the corresponding stationary and progressive waves, while the alternative consideration of the two functions pressure and current, together with the introduction of a power factor (as in

The Author

alternating current electricity) was pointed out. For the given circumstances the effect might be considered as that due to the combination of a wave of power factor zero (the stationary wave of appropriate amplitude) and a second of power factor unity, the latter being responsible for the net energy transmitted. The idea of an indefinitely large accumulation of energy is certainly opposed to the Author's hope and intention which was to convey to those interested, many of whom had no previous acquaintance with the subject, the simplest possible description that could be called consistent with the effects actually to be observed.

WORKING PRACTICE IN THE DESIGN OF LARGE DOUBLE-ACTING TWO-STROKE ENGINES.

By A. E. L. CHORLTON, C.B.E., M.Inst.C.E., M.I.Mech.E., M.I.E.E.

[Read in Newcastle-upon-Tyne on the 3rd November, 1922.]

The development of the Internal-Combustion Engine during the last decade has been much more in the use of liquid than in gaseous fuel, although in the previous ten years the latter probably received the greater attention. The internal-combustion engine using liquid fuel has become the prime mover for all forms of transportation. In the air it is supreme; it is equally so for road service; on the sea its progress is most marked; curiously enough, for transportation by rail, where its possibilities are great, but little has been done.

For marine work the engine has rapidly grown very considerably in size due to the increased powers required, and the lower propeller speeds preferred. Up to now the use of the single-acting engine has been almost universal, but the time has arrived when the high first cost and heavy weight have practically compelled engineers to turn to the double-acting type as an avenue of relief. In this development there is no doubt that the four-stroke will receive as much attention as the two-stroke cycle; but, as the Author's experience with double-acting engines has been principally with the latter type, he will confine himself to it in this paper.

In land practice for the largest powers, the double-acting principle had become practically standard before the war, the larger proportion of the engines being of the four-stroke cycle type. This was, in the main, due to the higher speeds at which they could be run—an important point when the driving of electric generators is considered. The two-stroke operated better at the lower and variable speeds, required for blowing engines, etc.

It is hoped that the practical experience made previous to the war with the two-stroke type, as reflected in the progressive design of the main parts, will be interesting and of service to designers now engaged on the problems connected with the double-acting oil engine. These double-acting engines used gas as a fuel instead of oil, and, though this means that some allowance must be made when translating the practice from one fuel to the other, it does not otherwise very materially affect their comparative value, for the mean pressures used are much the same (possibly less). The total heat passing is greater for the gas, though the maximum temperatures and pressures are less.

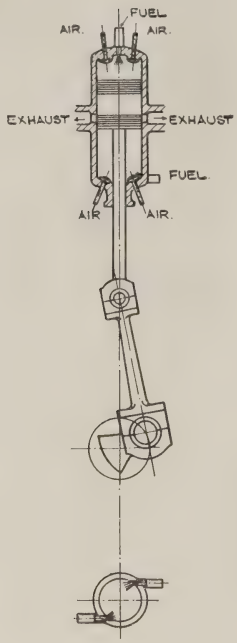


FIG. 1.

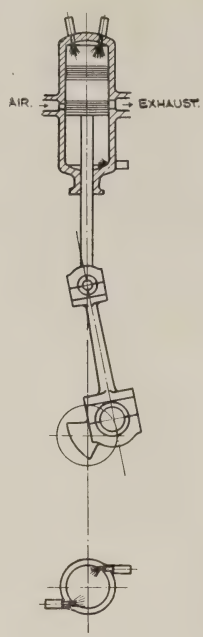


FIG. 2.

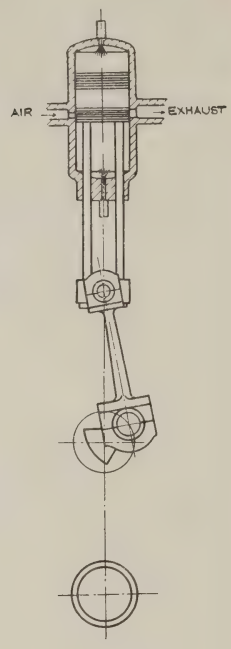


FIG. 3.

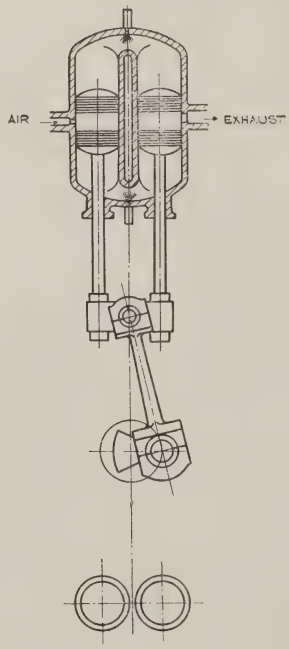


FIG. 4.

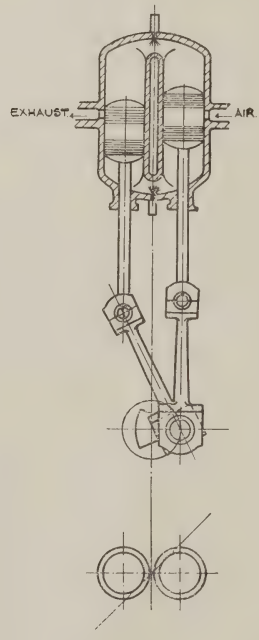


FIG. 5.

The Fluid Factors governing the design of two-stroke engines—as the disposition, shape and areas of inlet and exhaust ports, form of cylinders, amount of scavenging, exhaust pipe and its reactive influence, etc.—are of fundamental importance, but it is not proposed to deal with them in this paper. The object is to treat the subject purely mechanically; in

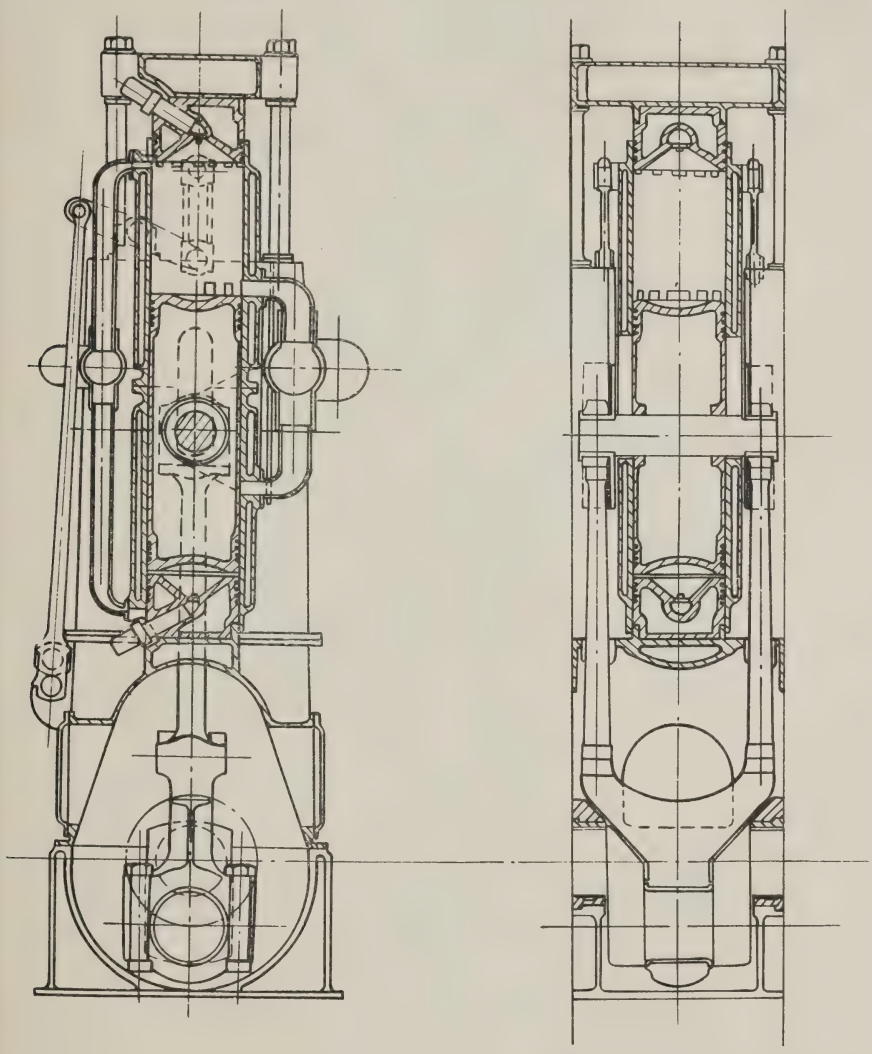


Fig. 6.

brief, to take the parts of the engine, one by one, giving the original design, experiences made, and the final design resulting therefrom. That is to say, it is confined to Factors of Design.

One may, with advantage, first examine briefly the types the double-acting two-stroke-cycle engine may be grouped under. It may be either Valve Scavenging or Port Scavenging, and of the following forms:—

(1) Single piston, single piston rod.
(2) Single piston, double piston rod.
(3) Double piston, single piston rods.
(4) Special.

Originally, the majority of two-stroke engines employed mushroom-shaped air valves placed at the cylinder ends with exhaust ports at the centre; then followed the opposed-port-scavenging type. Will there be a

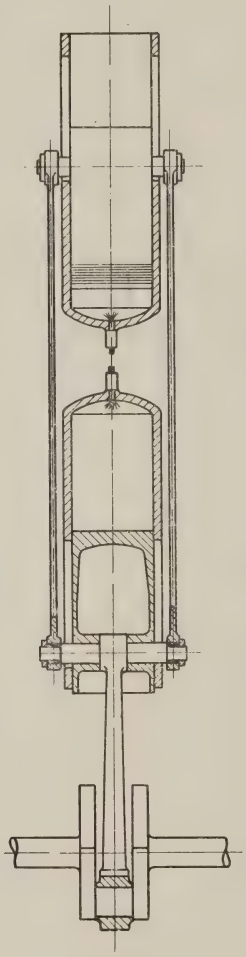
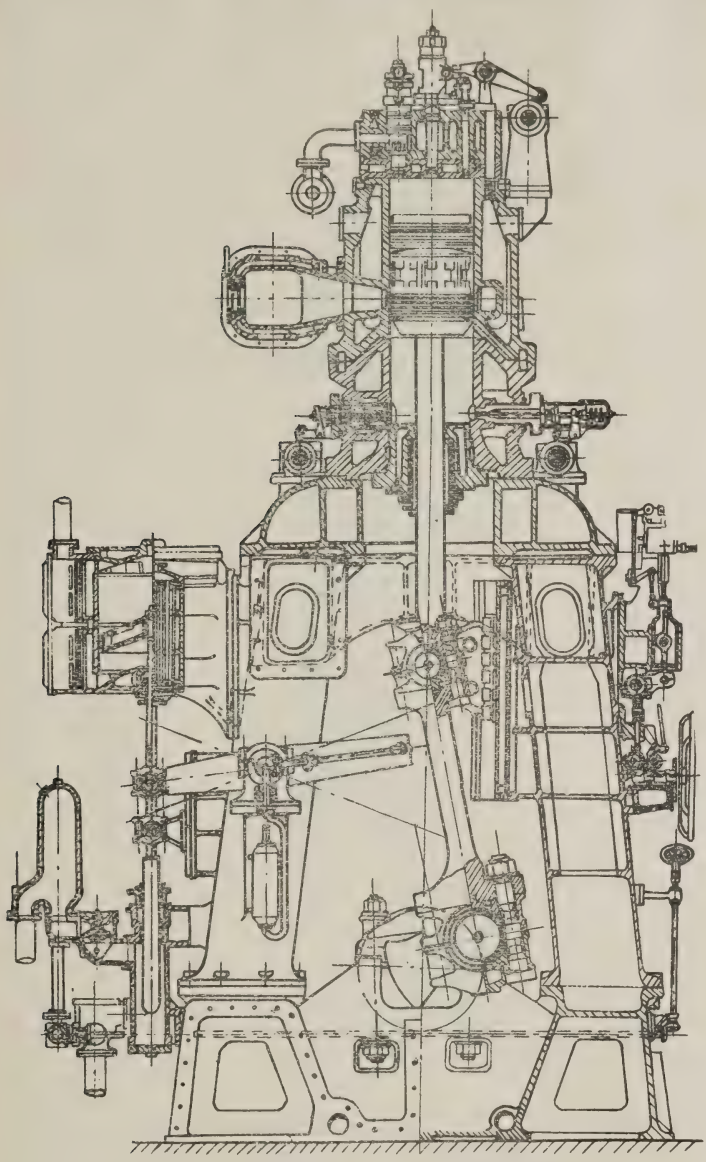


Fig. 7.

reversion to the valve type when the double-acting marine oil engine is evolved? The Author very much doubts it, as the valveless engine is very attractive.

(1) The first type is represented by Figs. 1 and 2, which are valve and port scavenging respectively. The objection to this type is principally that the combustion chamber at the crosshead end, due to the piston rod, is not of an altogether desirable form for the high-compression oil engine,

probably requiring the use of two fuel injectors. Two tangential fuel valves set at right angles to the cylinder centre are shown in the figure.

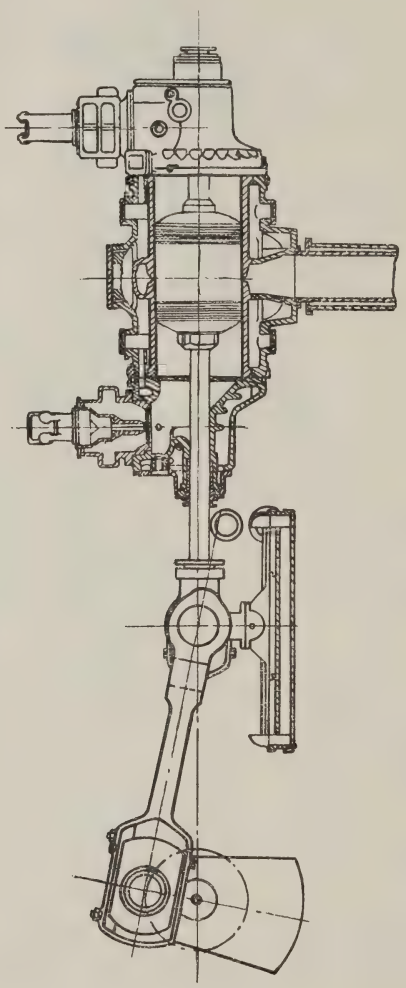


	ENGINE.	SCAVENGE PUMP.
BORE	480 m/m.	650 m/m
STROKE	710 m/m.	500 m/m.
R.P.M.	120.	

FIG. 8.

On the other hand, it is the most standard form of any double-acting, reciprocating prime mover, and hence, appeals strongly to the engineer.

(2) The type with two rods from the piston to a common crosshead, as shown at Fig. 3, is adopted to surmount the foregoing restrictions; that is, it gives a better combustion chamber, and therefore, will be



BORE 1100 m/m.
STROKE 1400 m/m.
R.P.M. 30 TO 90.

FIG. 9.

preferred by some. Its objections are possible unequal expansion of the two rods with the consequent strains that may be set up, extra cost, and that two piston-rod packings per engine are used.

In both these types when opposed-port scavenging is used, the double amount of heat passing the customary ports and bars, must have a material effect (hot one side and cold the other), and this is probably the

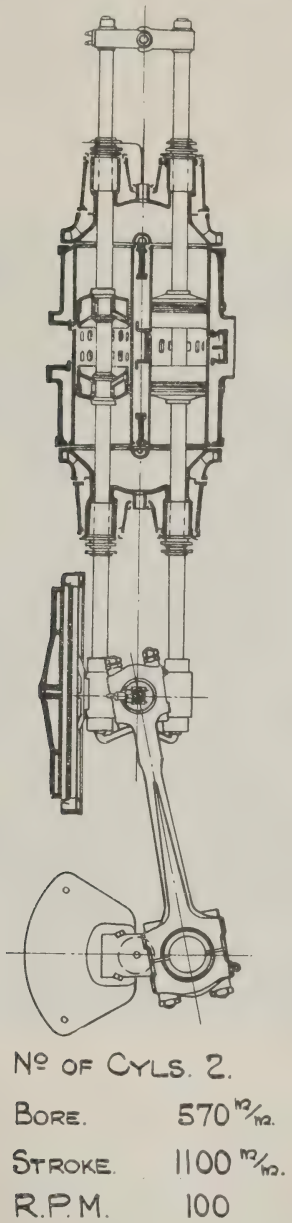


FIG. 10.

main reason which appears to deter single-acting advocates of the two-stroke-cycle type of engine from venturing into double acting. Ports at the cylinder ends or valve scavenging overcome these difficulties, the latter being particularly dealt with in what is to follow.

(3) In the double-piston Duplex-cylinder type, the first difficulty, long ports and bars unequally heated, is overcome and also a suitable combustion chamber provided at the crosshead end of the cylinder. Two piston-rod packings are, however, required. The two cylinders per crank may be considered a gain for large powers, on the score of heat flow, whilst the scavenging, as against the opposed-port type, should be superior. The standard type, Fig. 4, has a common crosshead for the two piston rods and a variation of it, Fig. 5, has a connecting rod for each piston. This latter, due to the offset cylinders, gives one piston a lead

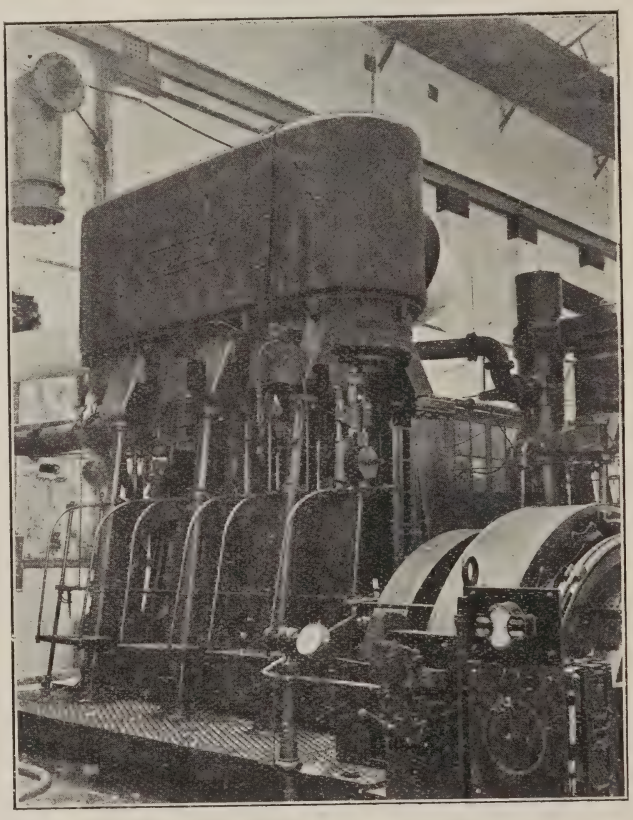


Fig. 11.

over the other, allowing of some supercharging; but it has the disadvantage that in reversing the lead is negative.

(4) Of special types, that of the North British Diesel Company shown at Fig. 6, is a very interesting example. Another form in which piston rods with their consequent packings are not required, is given at Fig. 7.

As illustrations of some of these types from engines in service, the following are interesting: that installed on the "Fritz" is shown in Fig. 8; a 2,000 h.p. single-cylinder gas engine in Fig. 9, and a double-piston engine, giving about 800 h.p. per crank, at Fig. 10. The main dimensions of these engines are given.

Fig. 11 is an illustration of a triple-expansion marine engine converted into an internal-combustion engine of the Duplex type. (Six engines were converted.)

Turning now to the main object of the paper, we may consider the evolution of design of the principal parts of the engine as follows:

Cylinders.	Piston rods.
Cylinder covers.	Piston rod packings.
Pistons.	Crankshafts.
Piston rings.	Materials, etc.

Their relative importance depends somewhat on the point of view, *i.e.*, time before replacal or first cost only. In the years the Author was engaged on the large two-stroke engine, perhaps the piston-rod packing was the most difficult proposition, as regards time, *i.e.*, frequency of renewal and adjustment; but as to actual failure coupled with first cost, he would place pistons first, cylinder covers second, and cylinders third. At the end practically the whole engine design had become standard save the packing. This part is, therefore, dealt with at some considerable length with sketches of types, later in the paper.

Cylinders.—Two-stroke double-acting cylinders, according to the classes already mentioned, may be for valve or port scavenging, single and duplex, with some special types, of which probably only experimental engines have been made. The experience of the Author was first in connexion with cylinders of the valve scavenging type, and later with duplex cylinders port scavenging. He has had experience with the opposed-port type, but not double-acting. The cylinders of the double-acting valve-scavenging engine, in the earlier examples, followed out the practice then coming into vogue for other large four-stroke internal-combustion engines; that is to say, they were cast all in one piece. This was undoubtedly a difficult casting from the foundry point of view, apart from any considerations that might arise due to the stresses set up when the cylinder was in use in the engine.

In the larger sizes the cylinders were very long, over 8 feet for an engine of 750 h.p. (expansion stresses) and the jacket cores complicated, due to the exhaust branch with the ports having to be cast in. The main barrel core was in itself a straight proposition; the difficulty was to get an absolutely clean surface of dense metal for the bore with a suitable metal in the jacket, and it became a difficult undertaking to balance this requirement with that of the strength required in the jacket in order to withstand the requisite water test pressure.

In the earliest cylinders, therefore, it was the custom to use a fairly strong iron, but sufficiently high in silicon to obtain freedom of flow to ensure a good casting. The hard and dense face of the metal for the bore was obtained by entirely covering the barrel core with chills made in small squares of 2 to 3 inches and about $\frac{1}{2}$-inch thick. Many cylinders were cast of this type and in practice gave successful results. The reason for the change from the single-piece cylinder was in the main due to questions of renewal, when the bore had worn; this was usually

aggravated at the exhaust ports. It was also desired to get a harder working surface.

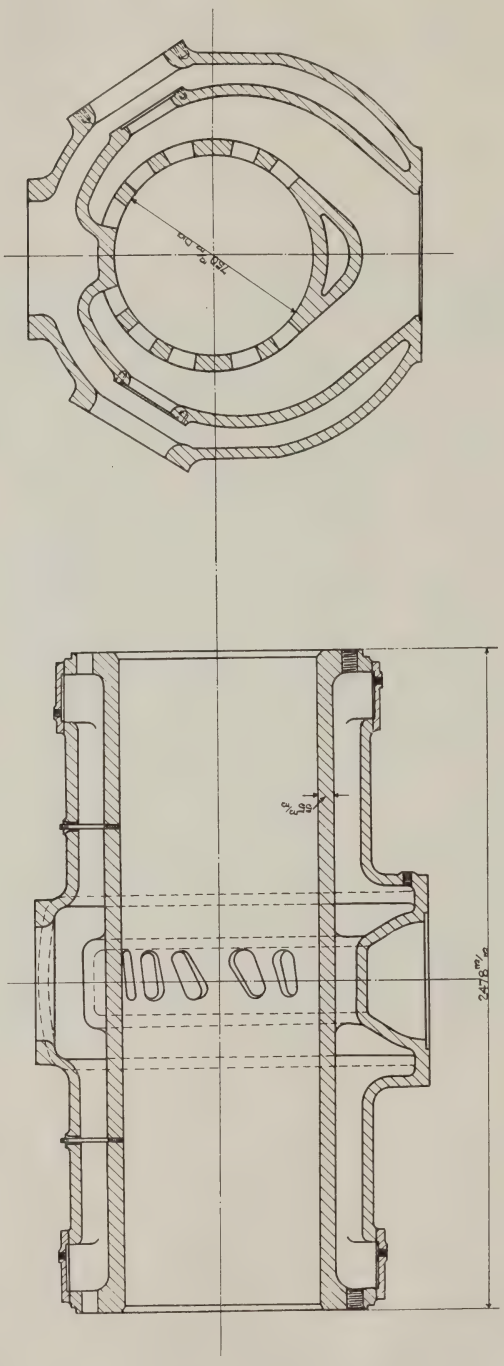


Fig. 12.

The next step, therefore, was the insertion of liners. By this means the mixture for both the main housing and the liner could be made

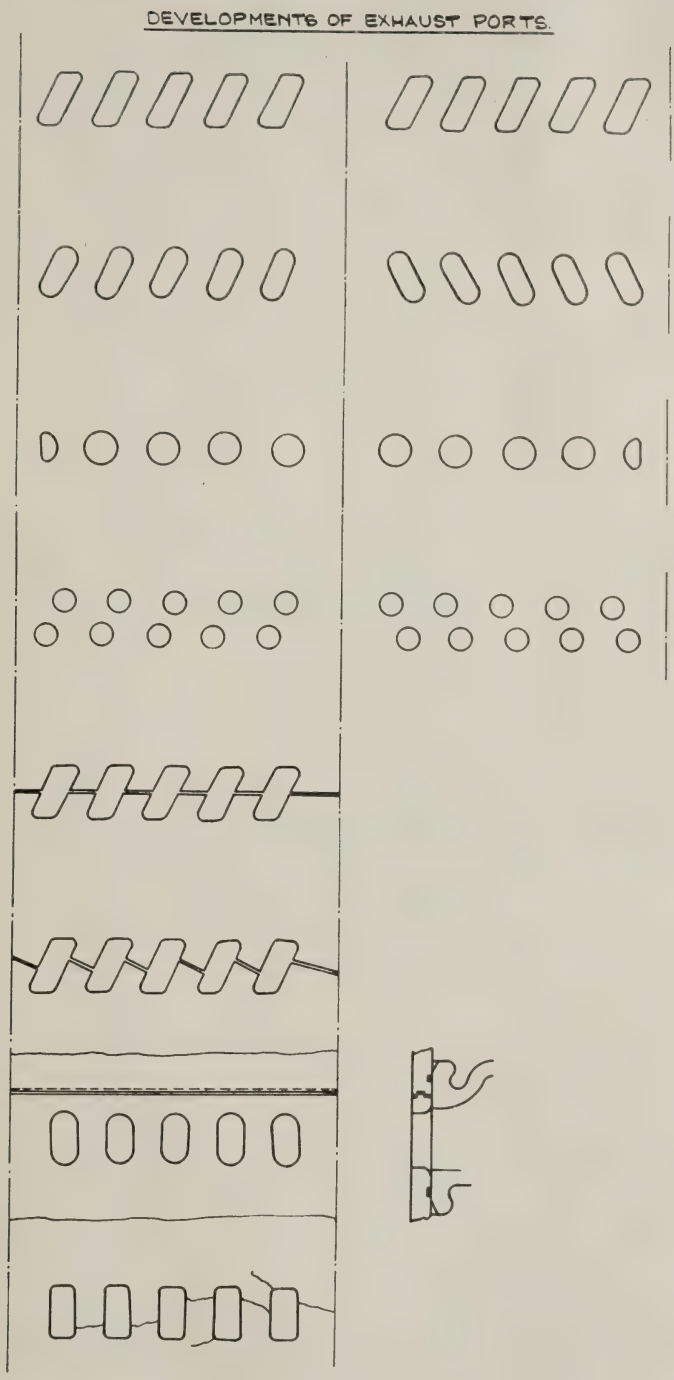


Fig. 13.

suitable for each purpose and the exhaust ports could be machined in. In principle, a liner seems simple until the extra expansion is thought of. This was undoubtedly a considerable deterrent; so much so, that the builders of four-stroke engines kept to the single-piece cylinder for a long time after the manufacturers of the two-stroke type had commenced fitting liners. In the two-stroke by dividing the liner about its centre and inserting it from each end, the expansion trouble was overcome. The difficulty that arose with these liners was that they were put in rather too tight, and in service owing to the expansion due to the heat, they burst the outer casing. Ultimately, it was found that the forcing pressure should be practically only that which could be put on with a couple of through bolts. Ordinary rubber sliding joints were used at the centre and were quite satisfactory. The liner itself was split either through the exhaust ports or preferably in the housing joint at one side of them.

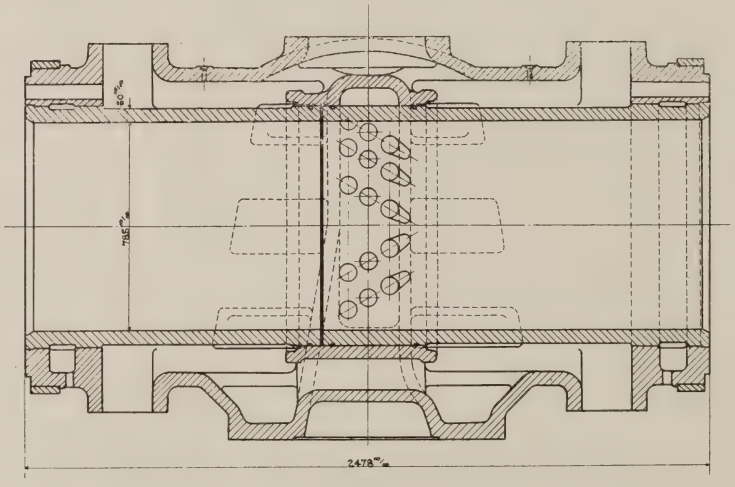


Fig. 14.

The wear that took place over the long bars of the exhaust ports in the early engines was serious. These bars wore quickly causing the cylinder to become barrel shaped and resulted in the rings catching in the long ports and breaking. Another difficulty was that with the long uncooled bars between the ports, cracking almost invariably took place. In considering the somewhat rapid wear that occurred in the early engines, inefficient lubrication and inferior oil must not be lost sight of, apart from unsuitable material, etc.

Fig. 12 gives a cylinder cast all in one piece with long ports. In some earlier forms than this, the ports were much more square ended. Owing to the wear at this part of the cylinder the ports were under consideration before the separate liner was adopted, being gradually modified in shape, until finally round holes were arrived at—Fig. 13 gives various types of exhaust ports. It is interesting to note that the

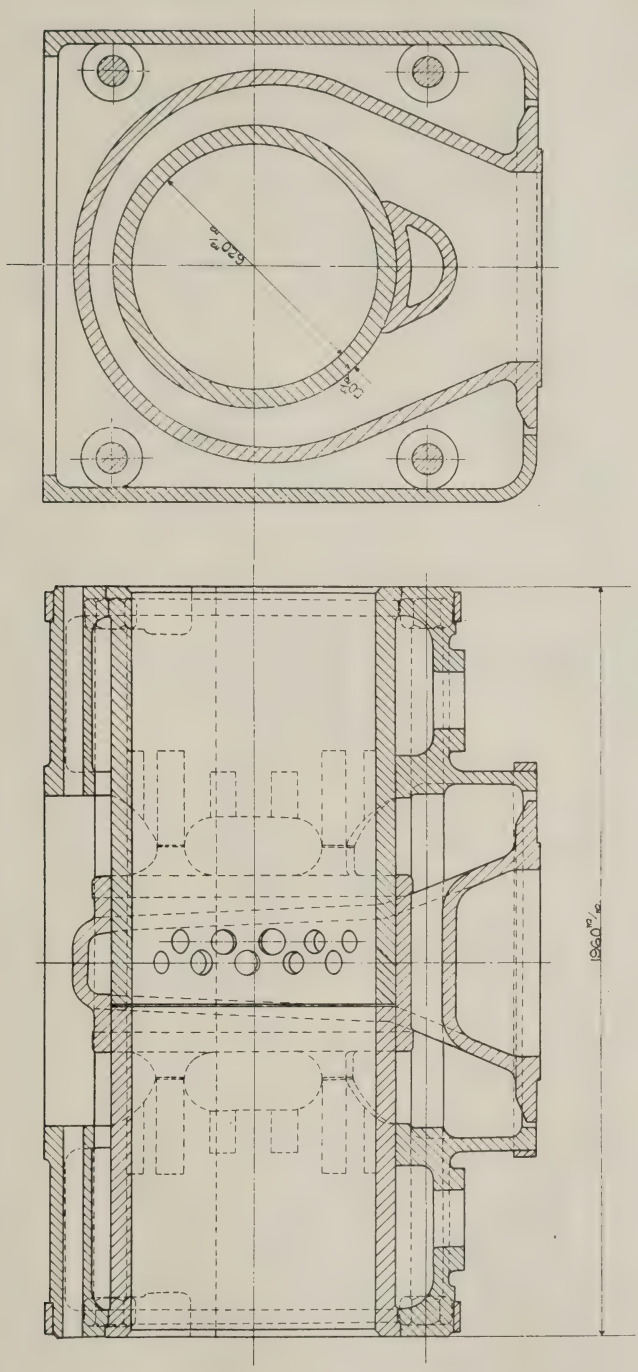


Fig. 15.

Uniflow Steam Engine followed the same evolution many years after, apparently quite without any attention to the experience already made with the combustion engine.

Fig. 14 is of a cylinder fitted with a liner showing how the round holes were arranged in this case for an existing cylinder. The reduction in area from the original ports did not prove a disadvantage in running, though the gas speed was increased by more than 50 per cent; the tendency was to decrease the reactive effect of gas vibration.

In a later design of cylinder, the exhaust belt was inserted in the form of a separate casting, as shewn in Fig. 15. This allowed of still greater freedom both in casting and machining, and also of expansion for all

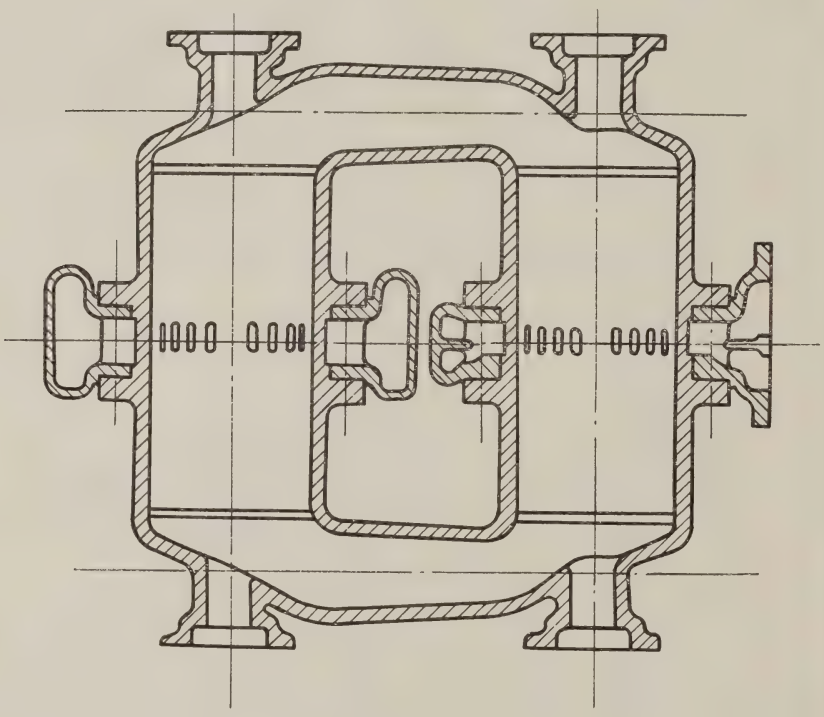


Fig. 16.

the parts. It will be seen that in this design, the opposed-port type can be readily adopted as the air branch would be inserted from the opposite side. In this type it is probable that to get the requisite port area without the extra long bars, double exhaust ports would be used set some distance apart and cooling introduced in between, to prevent the distortion which does arise with the long uncooled bar.

The question of cooling the bars was considered and tried (examples are shown later). The proposition is not an easy one. In all these designs, the increased thickness of metal at the end of the cylinder at the juncture of the liner and jacket is objectionable, owing to the high heat flow, and considerable ingenuity has been exercised by oil engine designers to

overcome it where the joint is made with the cylinder cover. This feature of most designs is probably a greater disadvantage for the higher compression oil engine, than for the lower compression gas engine with the larger combustion chamber.

In the cylinders shown, the register is on the extreme outside, and jointing material was normally employed (Klingerite). Such a joint

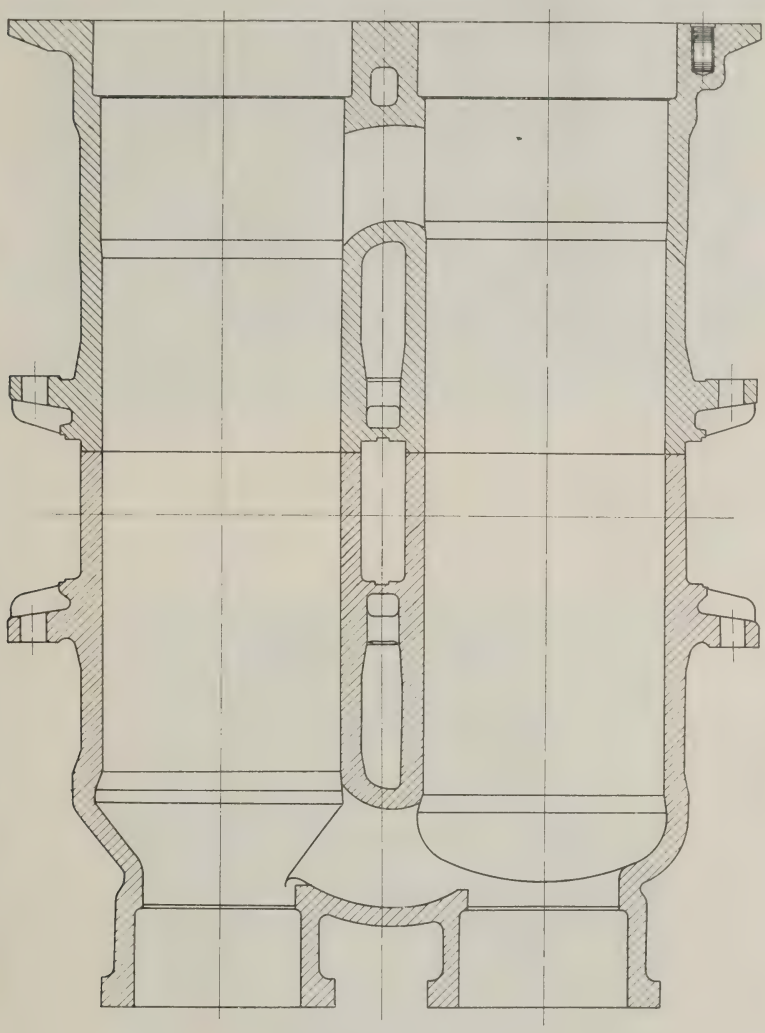


Fig. 17.

gave trouble in practice, if not very carefully made. Any leakage caused a cutting action from the erosion of the hot gases, and if allowed to persist, resulted in a crack being set up. The metallic inset ground joint of the oil engine is a better proposition though more expensive.

With the experiences made with the type of cylinders just described, both for the reason given and for others, the Author designed a duplex cylinder. The first consideration in this design was to do away with the

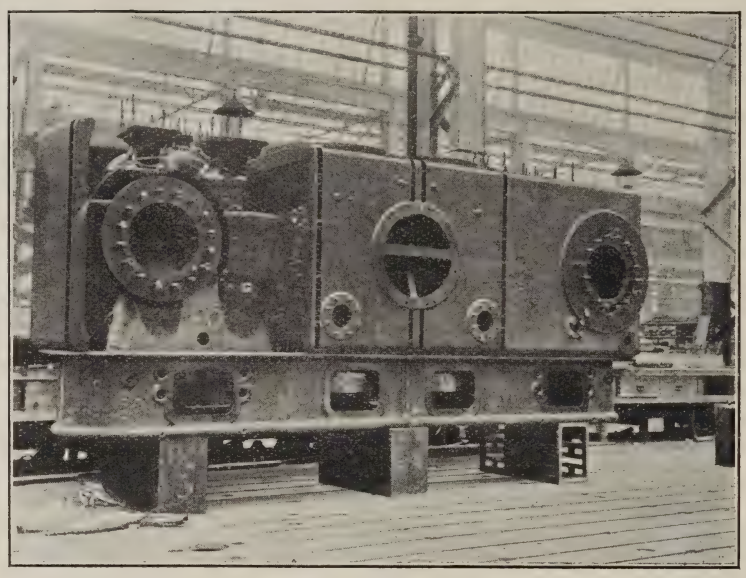


Fig. 18.

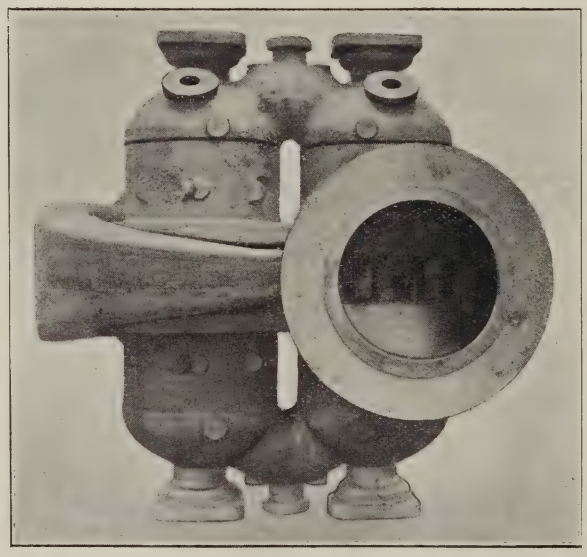


Fig. 19.

double-wall casting throughout, and to adhere as much as possible, to simple pipe-like forms. The casting then could be made of the most suitable material and free from initial foundry strains. Fig. 16 indicates the cylinders of an early engine, and here is clearly shown the simplicity

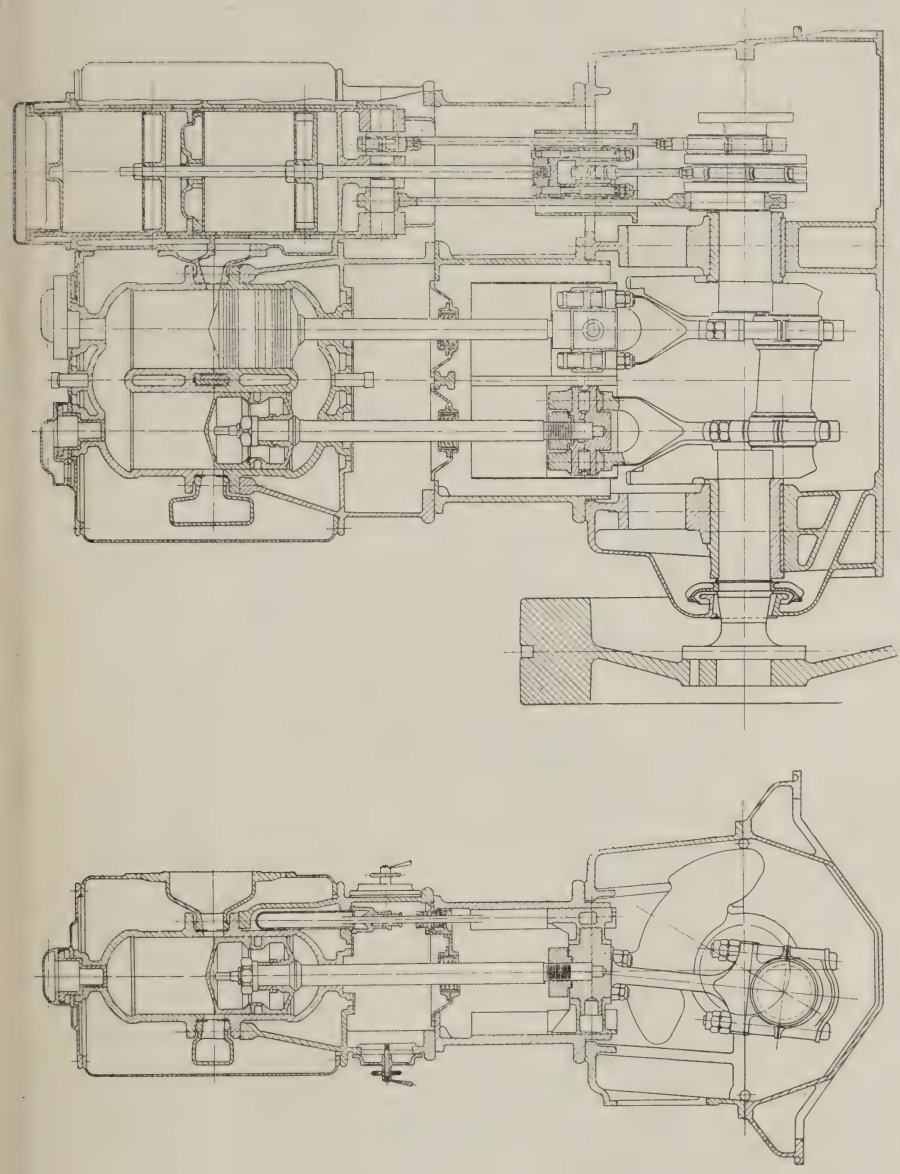


Fig. 20.

of the type. The cylinders are split about the ports and form simple U tubes bolted together. Such a form of engine enables port scavenging to be adopted and in effect the scavenging is superior, from the fact that no bye-passing to exhaust will occur. The cooling round the combustion chamber is good and no difficulties arise from the thick metal at the joint such as the early engines had and as is common in engines with remov-

able covers. As against this the engine is more difficult to dismantle, from the fact that the complete top half of the U shape of cylinders must

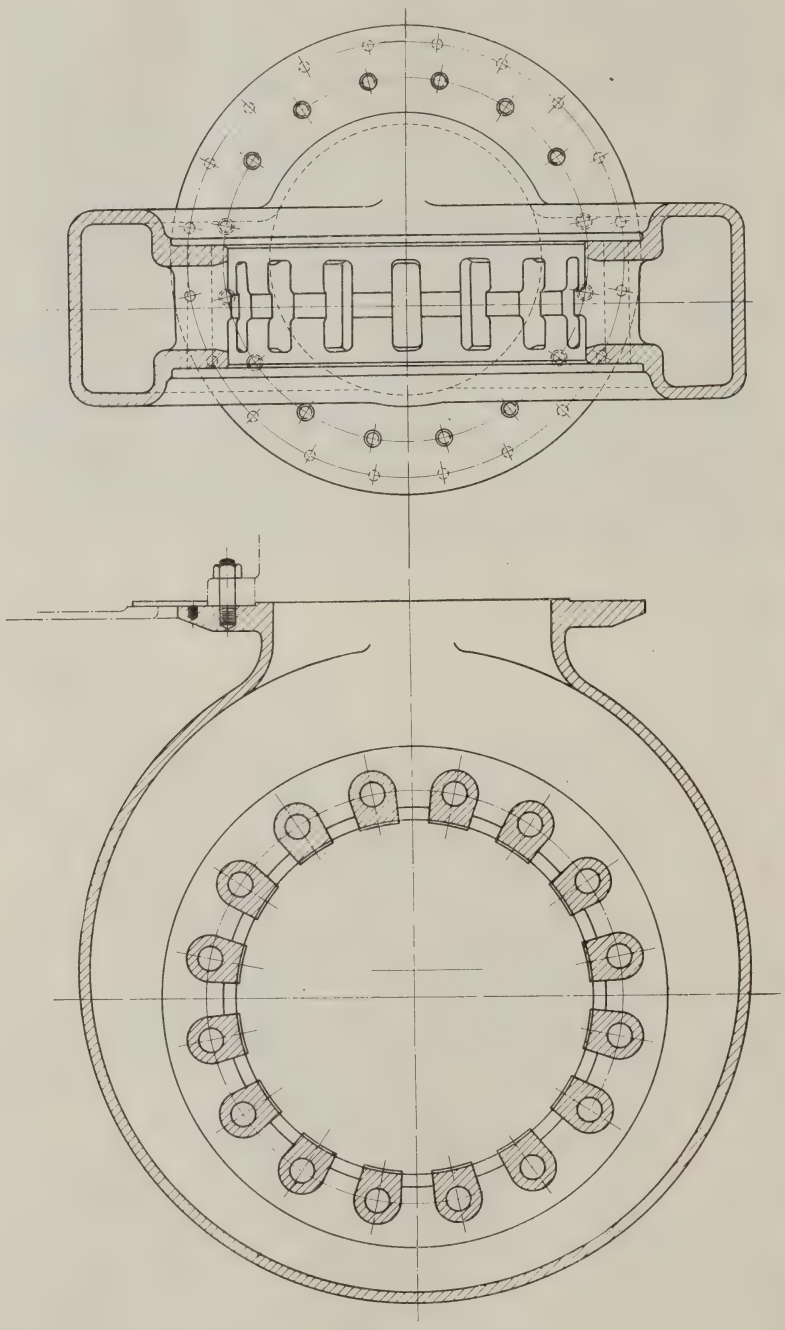


Fig. 21.

be removed to expose the pistons; an example avoiding this is given in Fig. 17. To surmount the jacket difficulty, the whole duplex cylinder

was mounted in a water tank as shown at Fig. 18. No doubt with the experiences made of this construction modified to make it suitable for oil engine work, many detailed differences would be introduced.

It is, however, a simple design and for large powers per crank keeps down the size of the cylinder. Fig. 19, shows a further modification of the type, whilst Fig. 20 gives a section from an engine complete with the air pump. In this type separate boxes for inlet and exhaust were used (Fig. 21)—an advantage from a manufacturing and upkeep point of view. An example with water-cooled bars is shown in Fig. 22. It is an advantage to cool the bars if it can be done simply and effectively. The

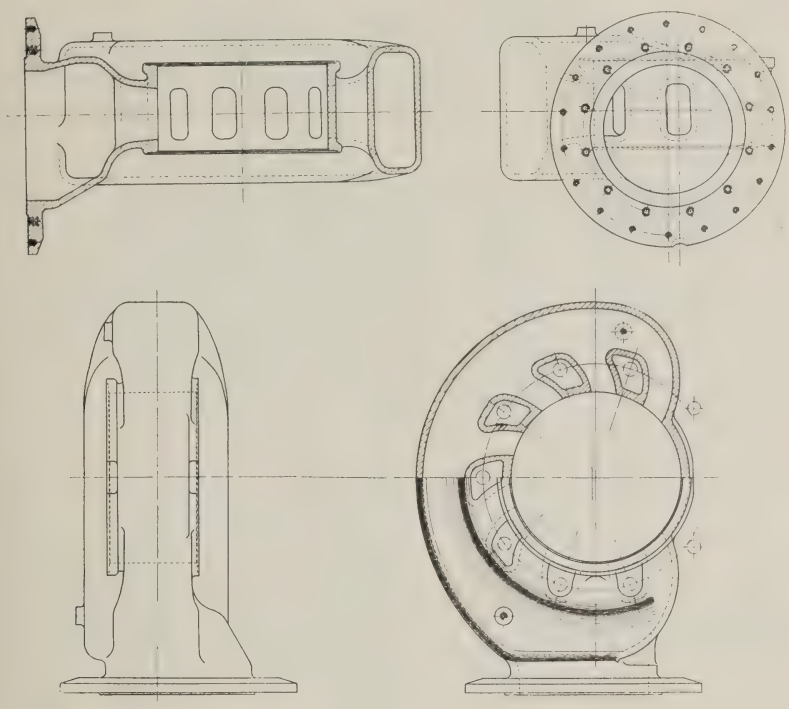


Fig. 22.

risk with small passages as were tried in earlier engines was that these passages furred up from the cooling water used. It is interesting to note that though the engine mentioned above was at work in 1912, we find in Germany during 1917 a duplex engine being developed by Klein Brothers, an example of which has been shown at Fig. 10, and a design for a 7,000 h.p. two-crank engine by the same firm is illustrated by Fig. 23.

The earliest application of the duplex principle was for the conversion of several triple-expansion marine steam engines, one of which was shown in Fig. 11. Ultimately, it could hardly be said that this engine gave good enough service to continue in everyday use, but the main difficulty was due to the nature of the gas, *i.e.*, it was too dirty and gave trouble with the piston-rod packing and the pump valves.

Looking at this conversion from an oil-engine point of view, the Author thinks that a more successful engine would have resulted than with gas, if the compression pressure had been kept low, for an oil engine is an easier proposition than a gas engine due to freedom from dirty gas and late fuel injection (freedom from pre-ignition). Actually though these engines would be said from current Diesel practice to be much too light (even taking into account the difference of compression), they did not shew this in service. Where they did fail other than as mentioned, was that the crosshead arrangement did not allow of the ready

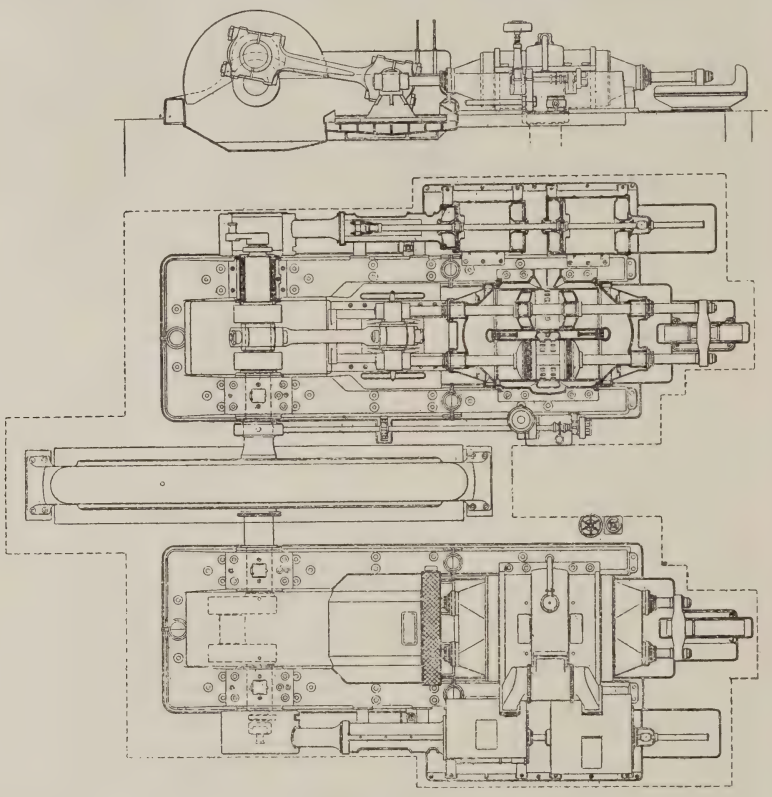


Fig. 23.

adjustment necessary always to maintain the piston rods running dead true, an essential for internal-combustion engines.

When considering the application of the duplex-cylinder engine, one might say that with the use of high-conductivity alloy pistons, cylinders of 24-inch might be adopted without water or oil cooling, giving a total h.p. per crank with a moderate m.e.p. of over 1,000.

Cylinder Covers.—The cylinder cover of the combustion engine using gas. and not exceeding, even with blast-furnace gas, a compression pressure of 175 lb. per square inch, is probably, when compared with the oil engine, less similar than any other part. The heat transfer may be greater but the pressure loading is less, and though this is true, as has

been stated for other parts, it is more concentrated in the cover. Furthermore, due to the difference in clearance volume there is a very material difference in design. Compare Figs. 24 and 25, which illustrate the covers of a valve-scavenging gas and a valve-scavenging oil engine respectively. The different duties of a cylinder cover, heat and pressure, have led designers to divide the cover into two parts, putting in front of the pressure portion a heat absorbing pad. This form has been dealt

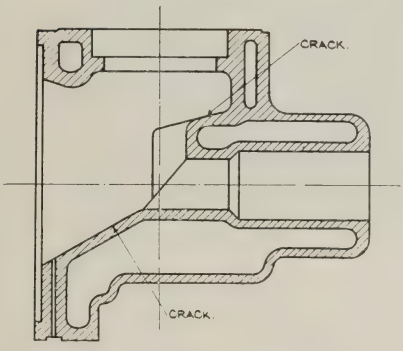


Fig. 24.

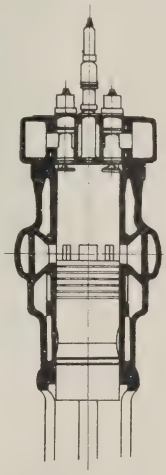


Fig. 25.

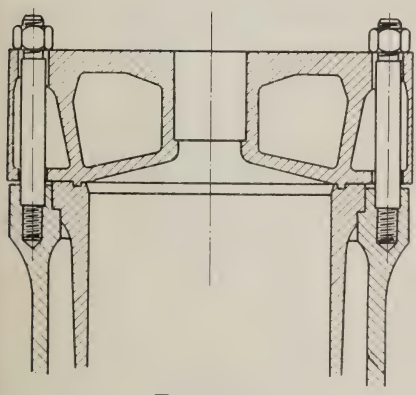


Fig. 26.

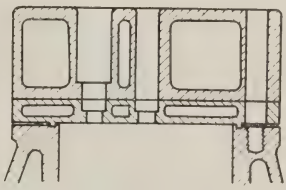


Fig. 27.

with in papers lately read, particularly for the four-stroke type. The valve openings make an undoubted complication. For the opposed-port type or the duplex with but one opening at the end, it does not seem necessary to adopt the double type. Compare Fig. 26, standard two-stroke and Fig. 27, double-cover for long stroke.

The practical experience made with valve scavenge covers was somewhat similar to that with the cylinder. Thus the covers were originally cast without any allowance for variation in expansion in casting and

working, *i.e.*, both skins of metal were tied together in more than one place. After many failures, the design arrived at, Fig. 28, was quite free in this respect. Another very important point, is to see from actual close inspection, that the cores in the mould are always accurately placed to ensure perfectly even metal all over for every casting. In practical effect, any works manufacturing internal-combustion engines should have its own foundry.

Relief or safety valves were used in the early engines, but in the later ones they were discarded, as they were always a nuisance from leakage, etc. Will the oil engine follow suit? It seems that with the engine using

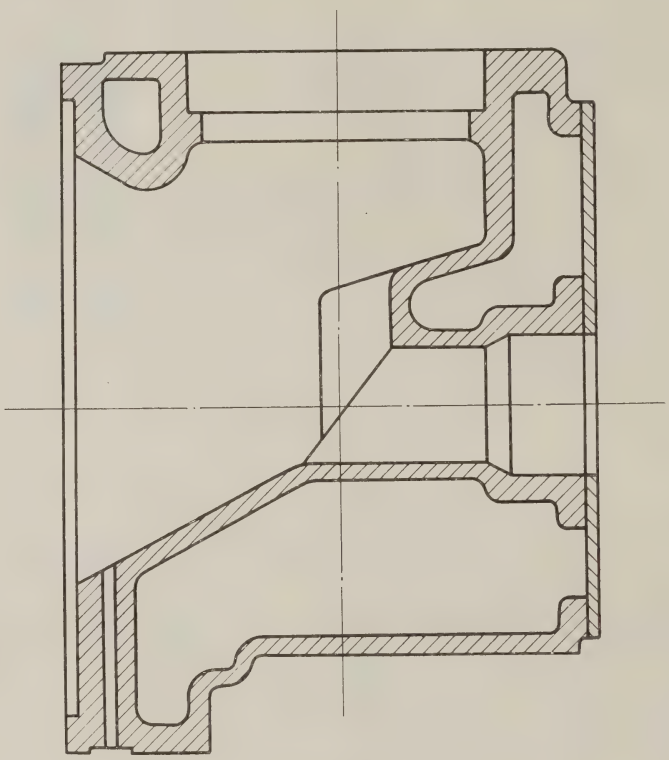


Fig. 28.

a compression of 300 lb. or even lower, this may possibly be looked for. Thus, with a moderate compression and an easy rating, it should be possible to use successfully a simple and inexpensive form of cover.

Pistons.—Probably the piston was the part in the earliest two-stroke double-acting engine which gave most trouble from failure. Many failures from cracks took place, the cause of which it was often impossible to diagnose, and as all pistons were water cooled, a crack meant an immediate shut down; thus, to overcome this, all manner of variations in design were tried: thicker ribs, thinner ribs, radial ribs, tangent ribs, no ribs, in one part, in two parts, with a through rod and back nut, with a palm end on the piston rod, etc.

In a two-stroke engine of the single-acting type, the need for closing the ports during the stroke involves a piston of more than half a cylinder in length. In a double-acting engine, however, though this is considerably reduced, the question of provision for expansion is more important, because of the increased heat flow (two ends) and that power in alternate

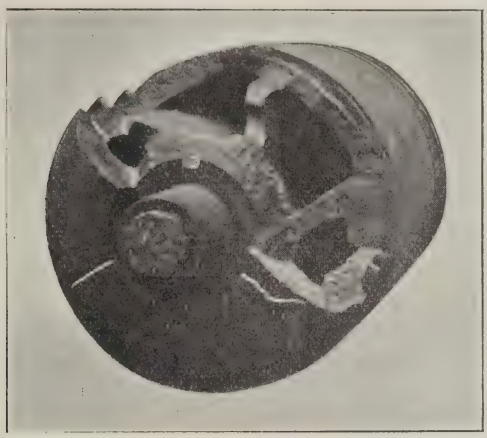


Fig. 29.

directions has to be transmitted. The variation in expansion between the cool ribs and the exterior surface at the ends exposed to the heat of the combustion, was probably the cause of the cracking, though there may have been initial casting strain. Fig. 29 shows a broken piston. It is difficult to determine the cause.

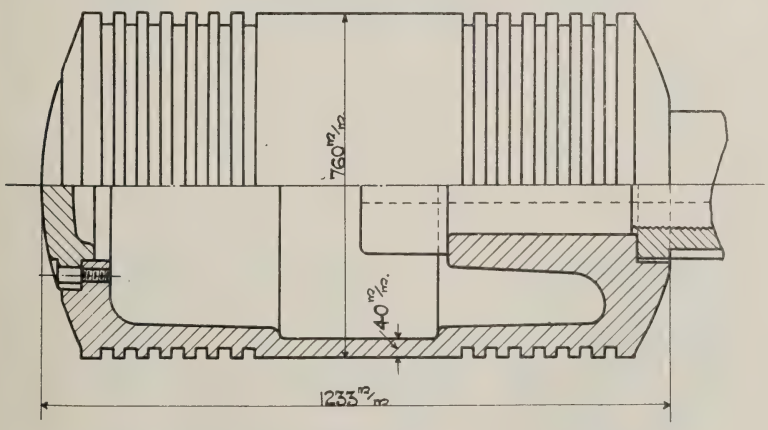


Fig. 30.

Pistons in the earlier engines were made of cast iron, an early type is shown at Fig. 30, a later at Fig. 31, and a still later one, with steel ends, at Fig. 32—all for a 30-inch cylinder. In another size a piston like Fig. 33 was used with a through rod and tangent ribs. A piston in two parts is indicated in Fig. 34. This was a still later type. Due to

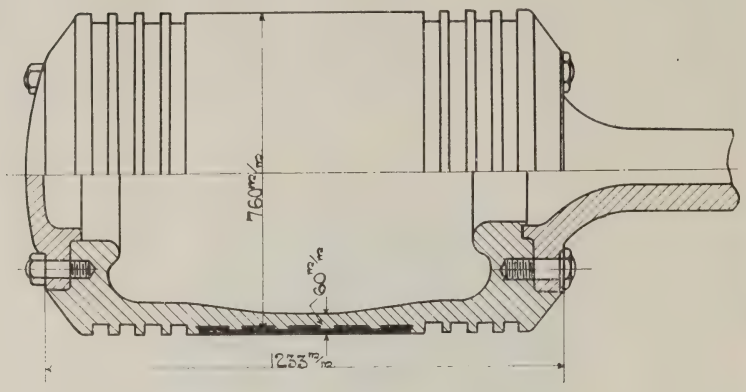


Fig. 31.

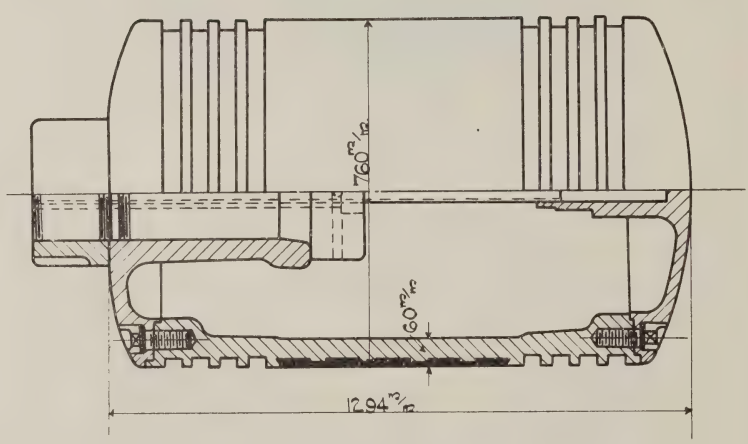


Fig. 32.

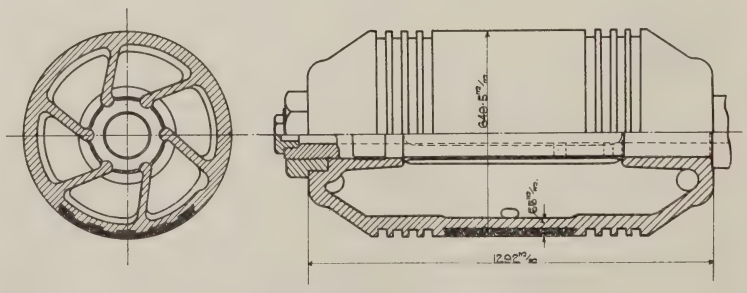


Fig. 33.

the many failures with the cast-iron piston, when the duplex type came to be developed, steel pistons turned out of the solid forging were used (Figs. 35 and 36). Some early ones failed, but afterwards these pistons were quite successful. Anti-friction metal, sometimes in an adjustable pad, was frequently used to support the piston in the horizontal engine (See Figs. 31, 32 and 33), and was undoubtedly a help in reducing wear

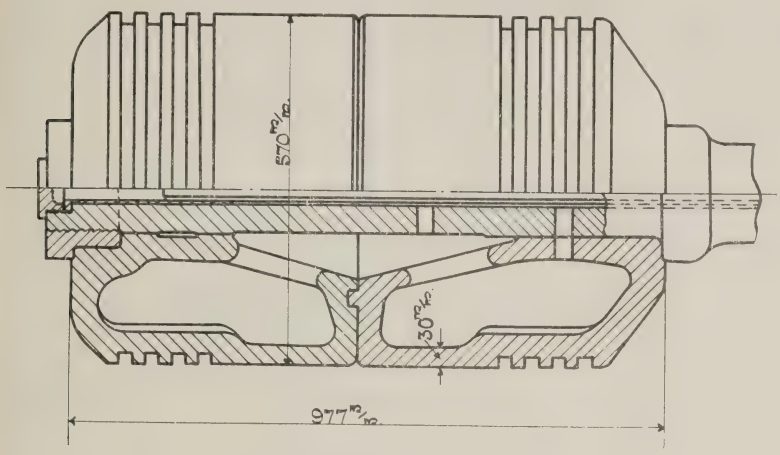


Fig. 34.

and as a steady for the rings in passing the ports. It might be used in vertical engines for similar steadying purposes, or when steel cylinders are used. Because trouble had been experienced with the stopping pieces falling out, the grooves in the two-part piston, Fig. 37, were milled, leaving a section in, to prevent rotation of the ring instead of the usual loose piece.

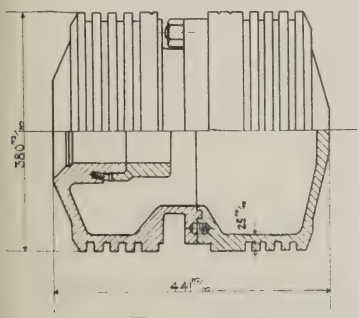


Fig. 35.

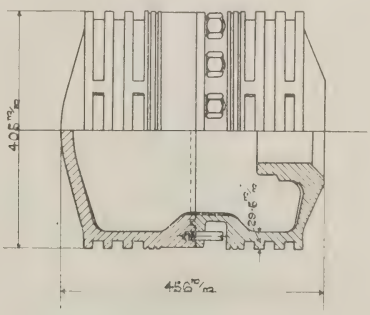


Fig. 36.

As aluminium alloy pistons have proved so successful in many cases, diameters of over 20 inches being used, it seems quite possible that up to 24-inch diameter they will ultimately be adopted without any internal water or oil cooling. This would represent an undoubted gain, as the water- or oil-service gear to a piston is not an unmixed blessing. A suggested form of double-acting piston is given in Fig. 38, made in two parts to allow for expansion and with the alloy in compression when

under load. No studs or parts likely to loosen by expansion are used. In this case an uncooled piston rod will involve special care with the packing; that is, it should be water cooled.

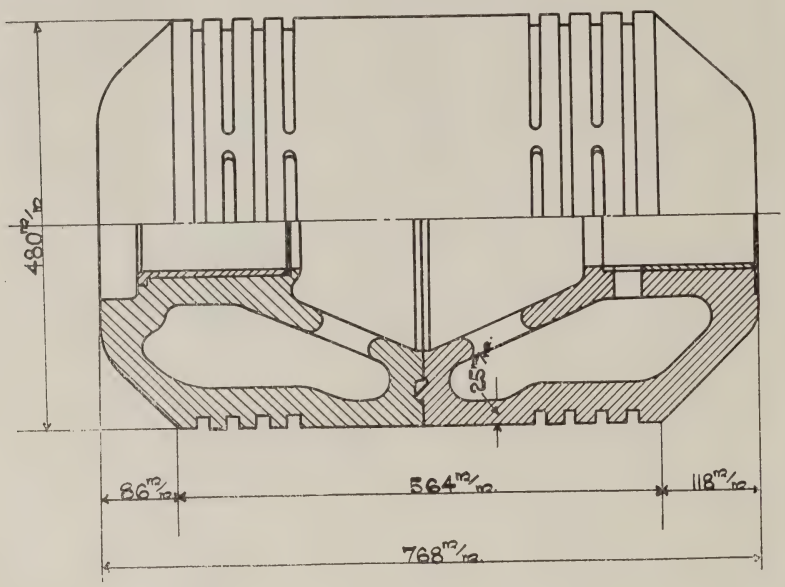


Fig. 37.

The type of double piston shown in Fig. 10 (Klein) has some advantages (expansion) for long-stroke engines, but for the opposed port engine it is difficult to fit due to the air being blown through the

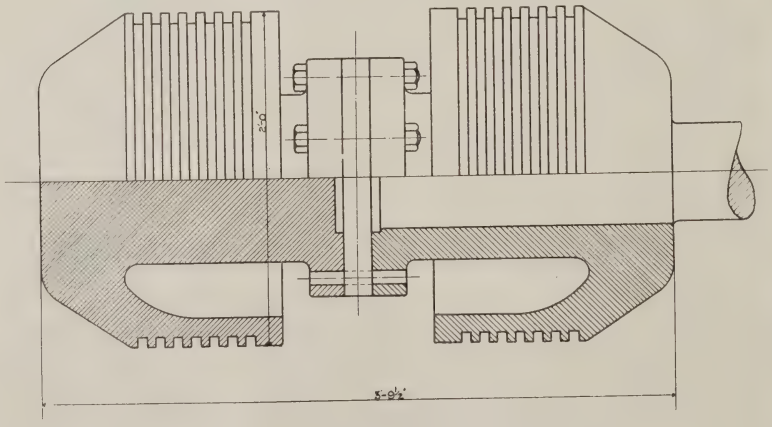


Fig. 38.

space between the pistons. An aluminium alloy piston of 8-inche diameter for an aero engine giving 125 b.h.p. per cylinder, is illustrate at Fig. 39, and indicates the amount of heat transmitted with comparatively thin section.

A later advance is in the use of forged high-conductivity-alloy pistons. These have greater strength and retain this at higher temperatures—"Y" metal is an example.

Piston Rings.—One fundamental difficulty in connexion with all forms of metallic packing for internal-combustion engines—whether for the piston or the piston rod—arises from the fact that they always start up under considerable pressure set up on the compression stroke, thus differing from those on steam engines.

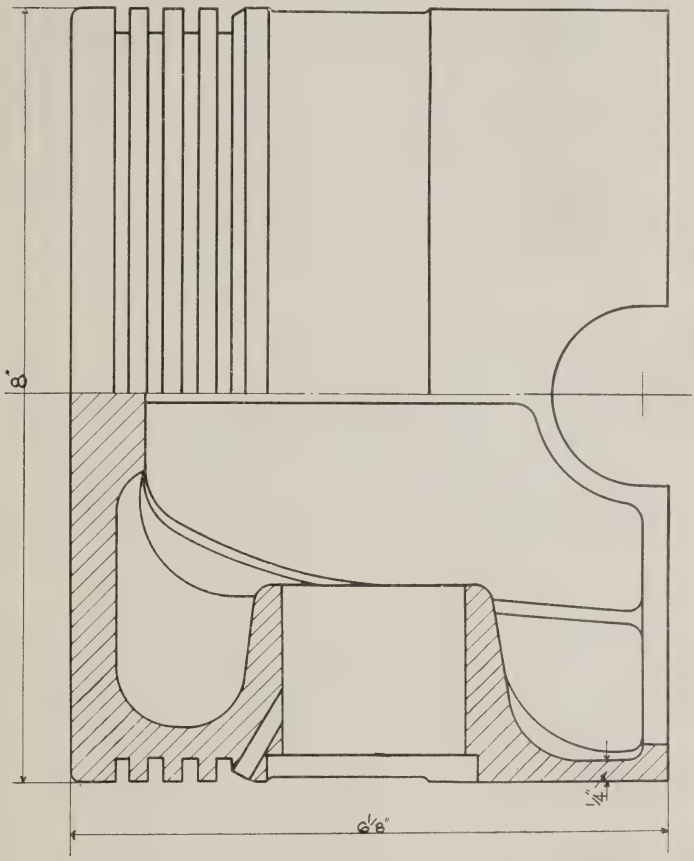


Fig. 39.

On the first revolution of the two-stroke motor, compression pressure occurs varying in intensity up to say 500 lb. per square inch, and if the packing rings do not completely bed, and in consequence are not tight, the oil is blown away through the leaky part creating an initial difficulty. In a steam engine, of course, at initial starting, only very low pressures are used, until the rings bed properly and are tight. This very important consideration, is, in the opinion of the Author, hardly taken sufficient account of. For a small cylinder accuracy and early tightness against leakage, can be attained by grinding. This may apply to a piston rod for a large engine as well

as a cylinder for a small one, but when cylinders of 24-inch diameter and over, are reached, even if grinding were a commercial proposition, the cylinder would not remain true when heated up, if it were ground in the cold state. Thus, rings that are now supplied with a ground

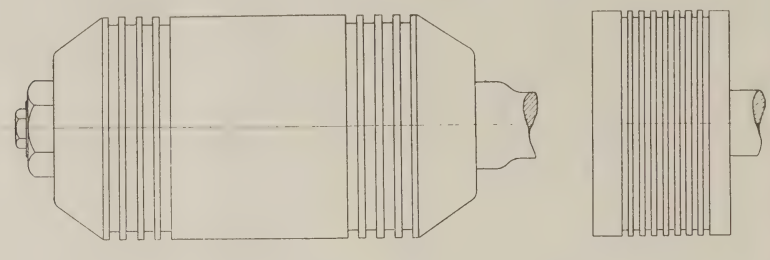


Fig. 40.

finish on the working surface may not fulfil what the extra expense involved is intended for. In actual practice we find that, to obtain tightness in running (but, in the Author's opinion, actually to attain

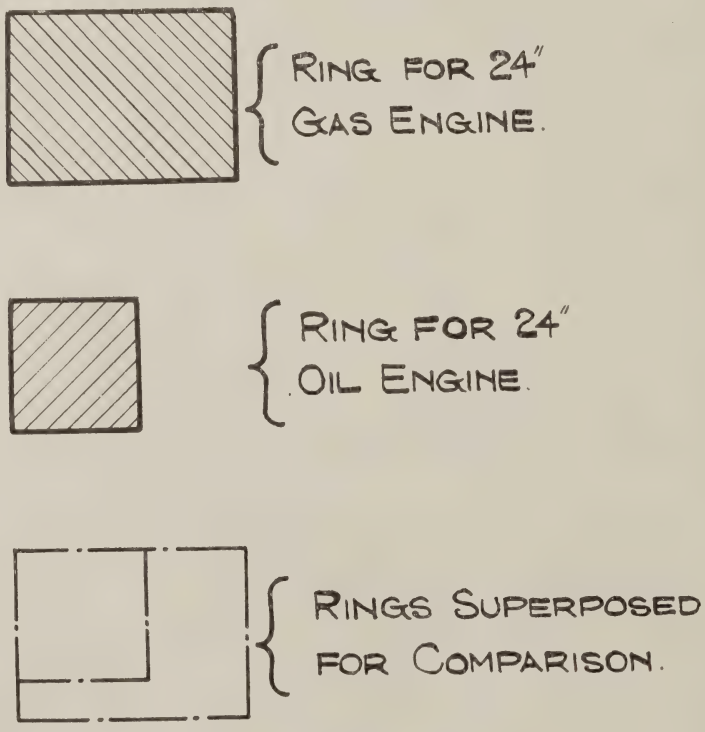


Fig. 41.

a quick bed at starting) comparatively strong rings are used, exerting an outward pressure of 7 lb. per square inch, in some cases over, whereas in the large engines he was concerned with, this figure was ultimately reduced to as low as 2 lb. per square inch with advantage.

We find further that as a characteristic of Diesel and high-compression oil engines, the first ring on the piston next the working face is placed much farther back therefrom than in engines working with lower compression; this is more particularly against pressure than temperature. Fig. 40 shows the pistons of a gas and a Diesel engine of approximately the same bore and power, one working on the two stroke and the other on the four. The different distances of the first ring from the inner edge are noticeable. The high pressure in the oil engine gets through the joint in the ring, passes behind and forces it out against the cylinder producing wear at the pressure end. Piston-ring

Fig. 42.

sections for a gas and an oil engine, each having a cylinder of 24-inch bore, are shown separate and also superposed for comparison in Fig. 41.

The Author suggests that by specially taking account of these factors more suitable rings may be used with less wear on the cylinder. The points to observe are:—

(*a*) Light pressure rings to prevent wear.

(*b*) Quick bedding to prevent initial leakage.

(*c*) Joint pieces or overlap to prevent leakage to the back of the rings nearest pressure.

As to (*b*), a ring section as shown in Fig. 42 might be used, which, having practically only a line contact at starting, would rapidly wear to a joint and increased bearing surface.

(*c*) Arrangements to prevent leakage to the back of the ring are an advantage, reducing cylinder wear. They may, for the pressure rings, be made of the limit type to allow the wear to be sufficient only to form

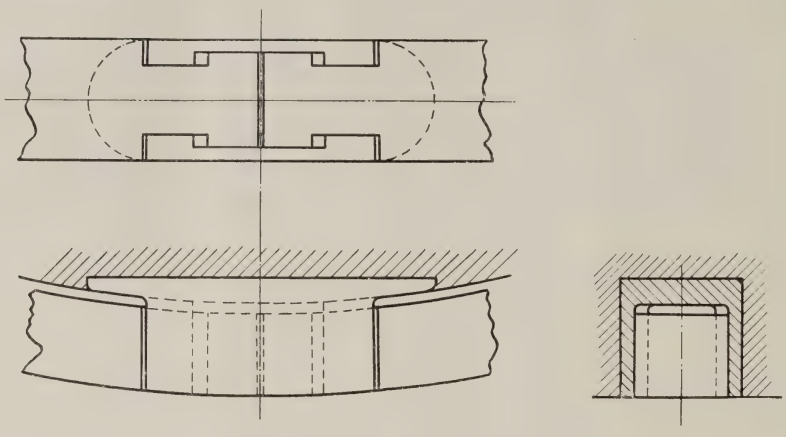


Fig. 43.

a good bed and then lock until readjusted. Great care must be taken in this design to prevent the arrangement getting out of the groove, a matter of considerable difficulty. (See Fig. 43.)

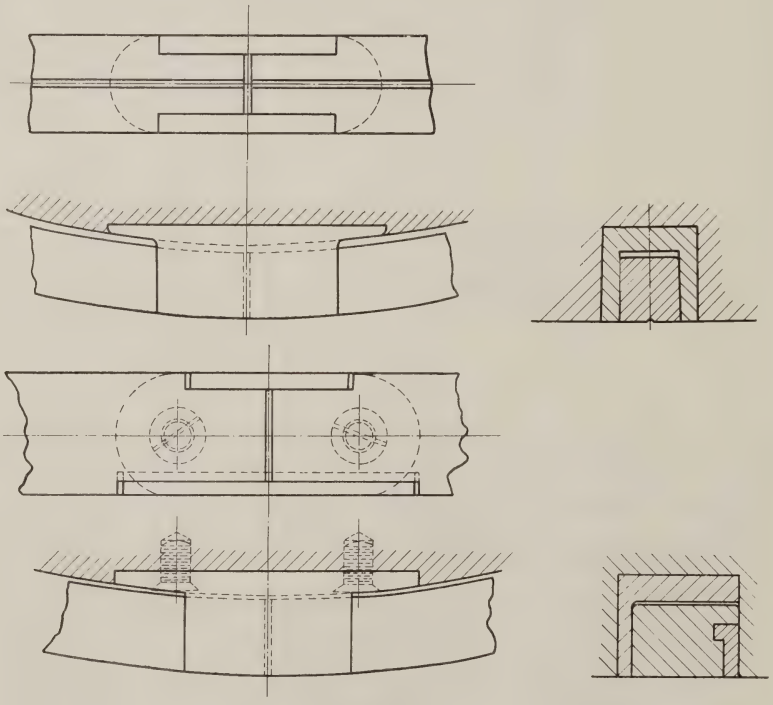


Fig. 44.

The piston rings for a two-stroke engine are a special problem. Either the rings must be designed to run successfully over the cut-away

portions of the cylinder at the exhaust, or the ports must be so shaped and modified as to prevent wear and cause no disturbing or breaking action on the rings in passing over them. The integral strength of the ring is, therefore, important, and the first progress made, after many rings had broken, was to increase the section—this was a distinct improvement. The next step was to reduce the ports to a series of round holes. A different type of ring, narrow but deep, was developed by a German maker. Joint pieces to cover the gap in the rings and prevent the gases from getting to the back and pressing the rings out were used (Fig. 44).

A form of ring made out of forged "Y" metal (aluminium alloy) has lately been tried quite successfully; attention is drawn to this, as it has the advantage of high conductivity and greater strength than cast iron, whilst it is of an anti-friction nature. Rings are sometimes made with channel section to reduce weight, and the inertia pressure at stroke reversal, i.e., prevent side wear. This part of the subject is dealt with at great length, as the success of the engine primarily depends on a tight piston with good lubrication.

Piston Rods.—In the oil engine the initial pressures are higher than in the gas engine, and the effect may be to increase the wear on the inner part of the rod. In the early days rods were made of a more or less ordinary class of mild steel. Then the carbon content rose as high as ·5. Nickel rods were also tried—all with a view to securing a stiff rod with a hard wearing face.

In one particular engine (duplex) two rods were used—one case hardened and one oil hardened. It was found that the case-hardened rod gradually developed circumferential cracks and ultimately failed, whilst the other rod ran for a long period. Generally, the rods had to be ground to a fine finish, etc., and it is essential that the method of attachment to the crosshead is such as to allow ready adjustment to enable the rod to be always kept running dead true. Carelessness in this respect means early failure of the packing and wear on the rod. Further, to steady and support the piston, tail rods were fitted (for horizontal engines); these were standard practice on four-stroke engines, and were also fitted to the two stroke, but the Author's experience was that the long piston of the two-stroke fitted with an anti-friction pad did not require such rods, and ultimately they were dropped. They are fitted to the German duplex.

Metallic Packings for the Piston Rod.—The metallic packing for the piston rod could hardly, after 10 years, be said to have arrived at a finality. Probably there is no part of the engine which has been so many times altered in design. The failure of the packings may be said to be due largely to the failure of the rod to remain parallel after a certain amount of running. In nearly every case after a few months of running, it was found that the rod had worn (often very considerably) at the explosion end whilst it had retained its original

diameter at the crosshead end. The problem, therefore, has been to pack a taper rod.

The wear at the explosion end of the stroke is no doubt primarily due to the heavy maximum pressure and higher temperature. It is influenced by the design of the packing—the lubrication of it, the cleanliness of the rod, the quality of the material, and the amount of intelligent attention it receives in cleaning and adjustment from the attendants in charge. Dirty gas or products (from incompletely consumed fuel oil) will cause wear of the packing and of the piston rod. On the other hand, considerable wear has taken place on the piston rods after comparatively short intervals of full-load running, when no dirt has been present. Lubrication is very important, but need not necessarily be made of multiple effect. For instance, in one case, Fig.

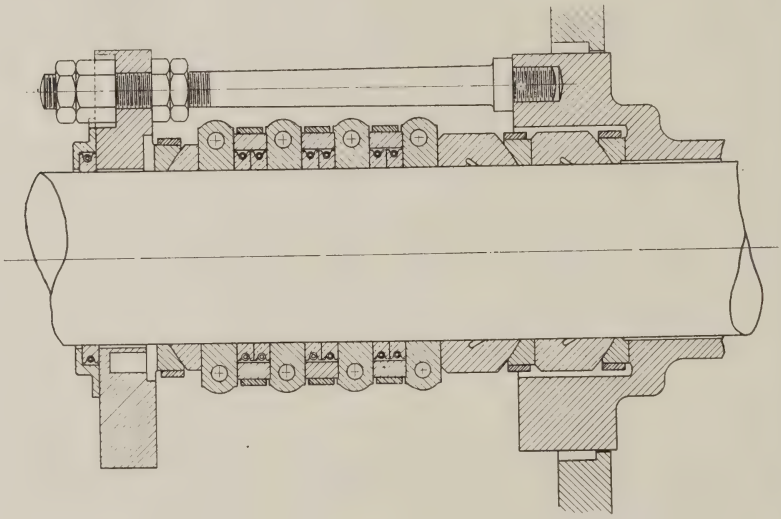


Fig. 45.

45, it was found possible to run with only one oil feed placed at the crosshead end, and as far away as possible from the explosion rings, without wearing the rod. This, however, is the only case in which this method of lubrication proved satisfactory, for all the weight of experience went to show that the back rings, which receive the explosion, should be lubricated. Still the one oil feed packing was probably as successful as any.

The following factors are fundamental.

(1) Cleanliness of the gas. (For the oil engine this is secured.)

(2) Adjustable crosshead guides to permit of the rod being kept dead true.

(3) Conscientious attention and adjustment from the engineer in charge.

(4) A hard and truly ground piston rod.

The attention that the packings receive at the hands of the engine attendants is undoubtedly a very great factor for successful running. If there is one point certainly established by experience, it is that piston rod packings will not run indefinitely, without cleaning out and adjustment, and that regular and conscientious attention is of the first importance if satisfactory results are to be obtained.

When the packing is water cooled as well as the piston rods, carbonization is probably minimized and the necessity of cleaning out becomes less frequent; but this type cannot be considered as essential if everything runs dead true, and complete combustion of the fuel takes place.

It is, of course, usual to design the packings so that the rings will float, and allow for any slight want of alignment that may be caused by

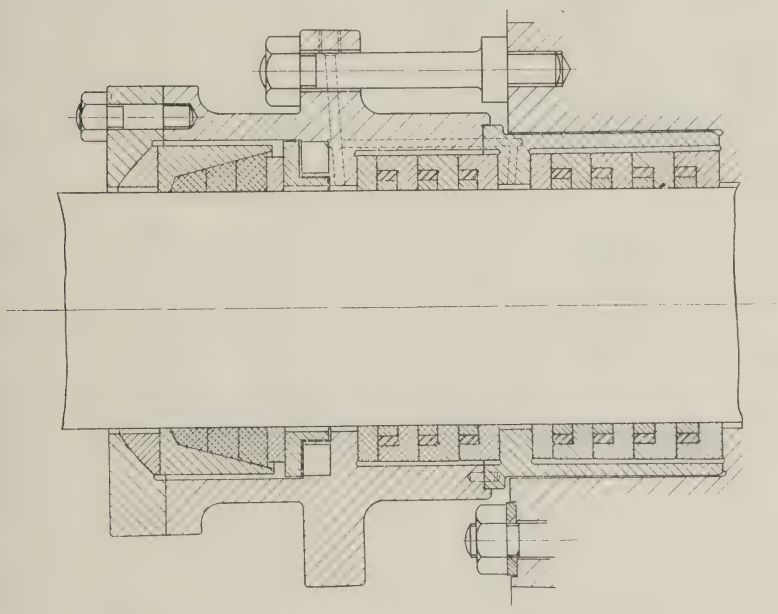


Fig. 46.

wear either of the piston or of the slides. How far this floating action—which necessarily implies the sliding upwards and downwards (or sideways if the connecting rod is not lined up properly) of each ring upon the next, affects the life and efficiency of the packing, is a matter of opinion, but it is true, that if the piston rod were in perfect alignment with the cylinder the motion of the rings upon their housings would not take place, and the end wear that is so detrimental, occasioned by this floating action would be eliminated. To minimize the end wear is very important, for, once the rings become slack in their cups due to it, the packing will begin to blow, the lubrication will be impaired by the passage of the hot, and often dirty, gases and the wear of both the packing and the rod, will rapidly increase. This point is borne out by experience, where if any end wear is immediately taken up, and pro-

vision is made in the crosshead slides for adjusting any wear that may take place which will cause the rod to run out of truth. If this adjustment were made a standard practice and promptly carried out, a packing of the type shown at Fig. 45, and which has the advantage of being easily taken apart without disconnecting the rod from the crosshead, might be the best solution of the packing question. Without such adjustment in the crosshead guides, this packing may not be so successful, since no provision is made for floating on the back rings, and therefore any want of alignment in the piston rod must necessarily re-act in a manner detrimental to the packing. This was probably the cause of failure in one case.

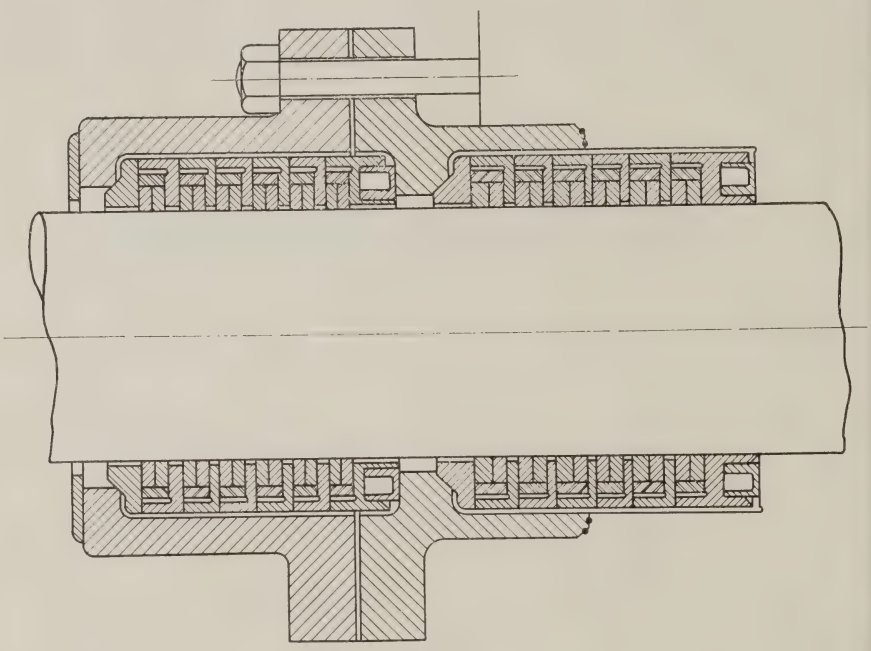


Fig. 47.

In Fig. 46 a form of oiling to the back rings was used, and phosphor bronze rings substituted for cast iron throughout the packing. This packing was successful. The rod had not worn at the explosion end after about three months of continuous running. In this case it was not possible to say definitely that the superior lubrication, or the phosphor bronze rings were, separately or together, responsible for the improvement, for the quality of the piston rods was superior to previous ones, in that they were of nickel steel and had been ground.

Generally speaking, a type consisting of solid rings at the back to break the force of the explosion, a spring ring in the middle, and white-metal rings at the front, and the end play to be automatically taken up on springs placed at different points in the packing, involves sets of rings of at least three different shapes and of several different sizes. This type is also more expensive to maintain than one having a similar ring

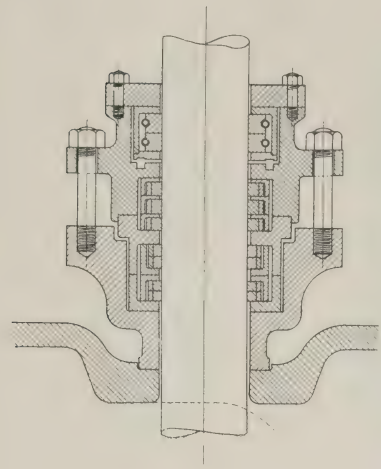


Fig. 48 (*b*).

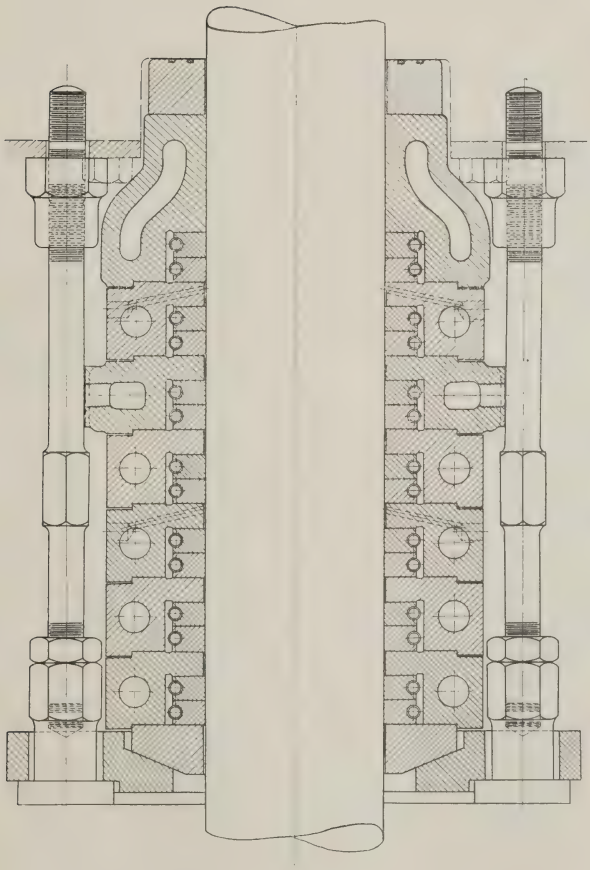


Fig. 48 (*a*).

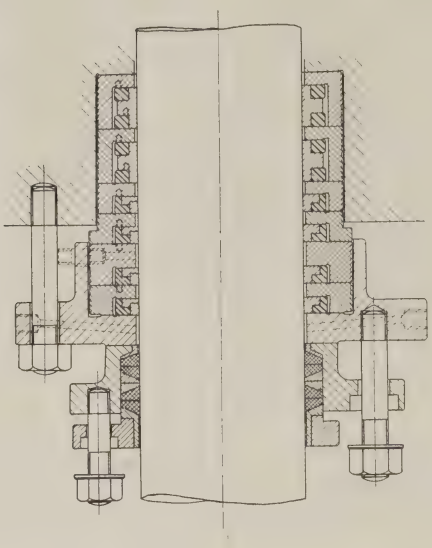


Fig. 48 (*d*).

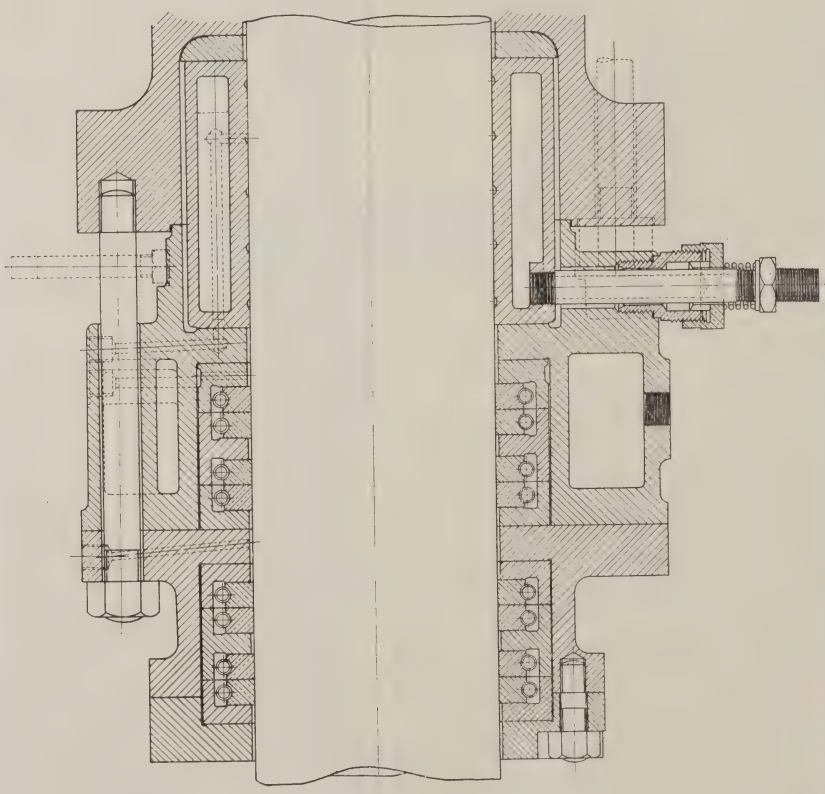


Fig. 48 (*c*).

throughout its entire length and of the same metal. The advantages of such a type are :—

(1) That a uniform pressure would be exerted upon the rod by each ring.

(2) That as each ring would be interchangeable and standard, new ones could be fitted easily to any part of the packing that required them.

(3) The rings could be made cheaply to the same gauge so that a reasonable number could be kept in stock.

A packing on the lines above mentioned (Fig. 47) was installed, phosphor bronze and clip rings being used throughout. The faces were not ground, as is usual, nor was special lubrication adopted. The packing, however, proved successful, and worked three months under continual night and day load (equivalent to over six months of ordinary mill load running) without blowing. Isolated rings have required to be

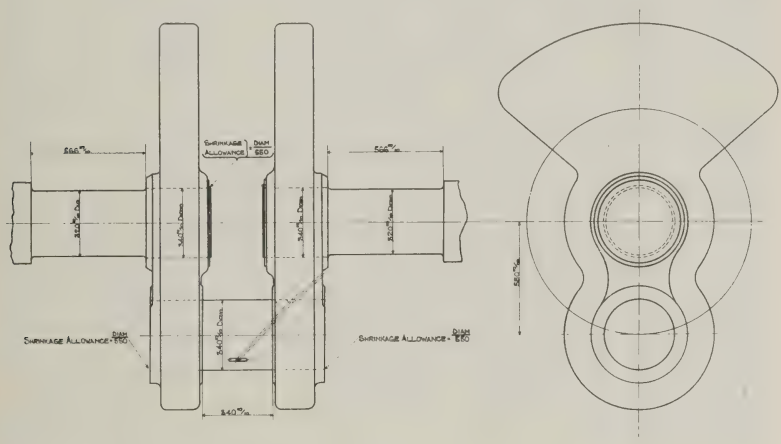


Fig. 49.

renewed from time to time, but being plain clip rings and all alike, they are cheap and easy to install.

A further point that has yet to be determined with certainty, is the relative merits of cast iron, steel, and phosphor bronze for packing rings. Cast-iron rings have been more or less the standard in the past, and some German engineers have expressed the opinion that cast iron is the only suitable metal for an internal-combustion engine packing and this continues to the present day, the metal being described as one that will not smear. This is important as the chief trouble that has been experienced with cast iron rings has been that no matter how soft the cast iron when first put in, after a short interval of running the surface has become glass hard. The result has been that the rods have worn, and the rings have not.

Phosphor bronze appeared to have much in its favour over either cast iron or steel, and as described previously it has been tried. It has, however, been stated, that phosphor bronze will not stand the temperature

of explosion at the back end, and consequently is unsuitable for packing purposes, but it was the experience that, at anyrate for reasonable periods, this metal was satisfactory, and even superior to cast iron. The packings shown in diagrams given and in Fig. 48, may be grouped into 3 types—sectional rings with circumferential garter springs, single- and double-clip rings, and cone and wedge rings with axial springs.

Crankshafts.—A short note on the practice in built-up crankshafts may be of interest. Fig. 49 illustrates a crankshaft for a 650 mm. cylinder, the custom in shrinkage allowances is given. (L. & Y. Rly. 1/458 part of the diameter).*

The Author thinks he was the first to try built-up cranks on large two-stroke engines; the results were fully successful. Built-up cranks

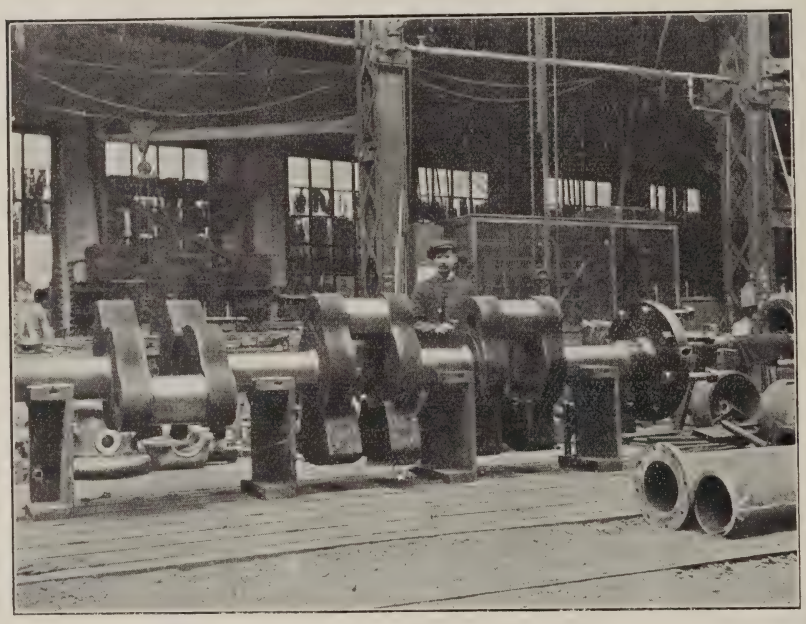


Fig. 50.

have now become standard practice. In the cranks referred to, great care was taken in the shrinking together as the shaft was not trued up after it was assembled on V blocks. Fig. 50 shows a shaft after completion, true to two thousandths of an inch. As no large lathes were used these shafts were very economical in cost.

Lubrication.—Efficient lubrication was one of the main factors in the successful running of large double-acting engines in the past. Until this problem was solved, the engines were unreliable and considerable wear took place in the cylinder and on the pistons. Engines which

* See "Locomotives Designed and Built at Horwich, with some Results" read by Geo. Hughes before the Institution of Mechanical Engineers, July 27th, 1909.

had given bad service, gave, when fitted with a good lubricating system, results similar to those obtained with a steam engine, regularly running the full week without stop. One such engine which does this to-day was installed in 1904 (500 b.h.p. in one cylinder at 94 r.p.m.).

A point in the double-acting engine is that all holes of ingress to the cylinder are uncovered by the piston and exposed to the heat of combustion; this, in consequence, means more holes of ingress and less feed per hole, *i.e.*, no excessive quantity exposed on the cylinder surface at any one point to carbonize. It is standard practice in single-acting engines to avoid this by not uncovering the hole.

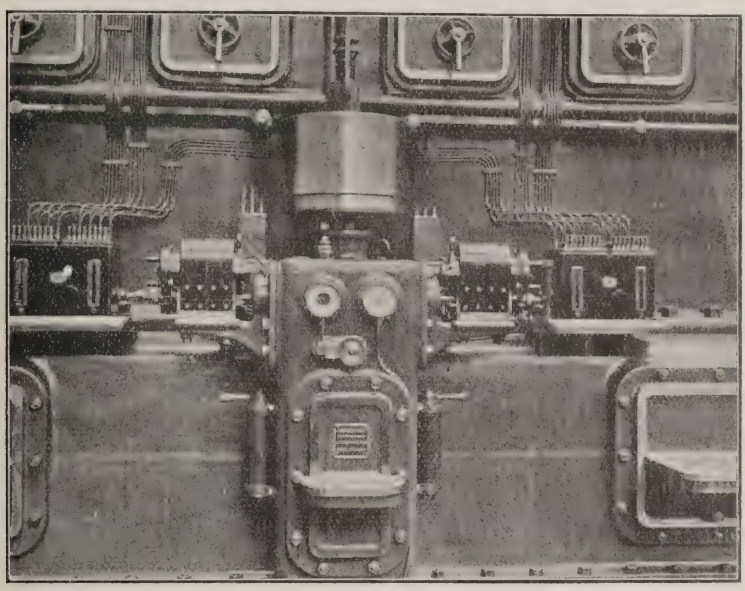


Fig. 51.

A good lubricating system was found to require:—

(1) A good lubricator.
(2) Multiple feeds into the cylinder and one plunger and pipe per feed.
(3) Small piping (of steel).
(4) Special back-pressure valve and cleaning out fitting to each hole on the cylinder.
(5) Good oil.

The last is not the least important, but it is curious to note that at the beginning very expensive heavy oil was used, whilst finally it was quite cheap. The fundamental idea is to put in oil at as many places in the cylinder as you can afford and as little as possible at each place; then there is no excess to carbonize and a cheap oil can be used (possibly 2s. a gallon).

This system when finally carried out in complete detail, resulted in an astonishing change in the running of the engines, greatly reduced the wear, brought down oil costs and increased the reliability of the engine. Fig. 51 shows an installation, and Fig. 52 a back pressure and cleaning valve. Double plunger lubricators were most frequently used. There must be a sight feed to each plunger, which for preference should be of the measuring type, so that the sight is not under pressure.

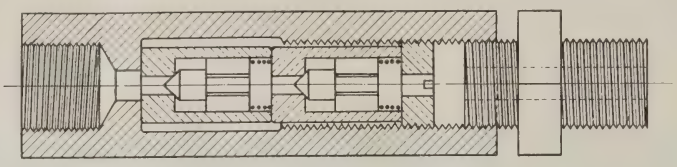


Fig. 52.

Lubricators are well dealt with in a paper on "Mechanical Lubricators" read by Mr. A. B. Smith before the Diesel Engine Users' Association on February 10th, 1922.

Materials.—The materials of construction, particularly those used for the parts which receive and transfer heat, are of very great importance. They have, however, been dealt with several times during the last few years, notably by this Institution, so that it is only proposed to deal with them briefly.

MIXTURES OF CAST IRON.

Mixture.	A	B.	C.
Silicon.	1·37 ± 0·37.	1·10 ± 0·10.	2·05 ± 0·25.
Manganese.	0·70 ± 0·10.	0·5 max.	0·55 ± 0·05.
Phosphorus.	0·30 ± 0·10.	1·0 max.	0·04 ± 0·01.
Sulphur.	0·08 max.	0·12.	0·014 ± 0·001.
Graphite.	2·45 ± 0·25.		2·80 ± 0·30.
Carbon. Combined.	0·70 ± 0·10.		1·35 ± 0·05.
Carbon Total.	3·15 ± 0·35	3·10 ± 0·10.	4·15 ± 0·35
Note:- The Thermal Conductivity Factor for Cast Iron is from 0·10 to 0·12.			

Table I.

Various mixtures of cast iron are given in Table 1. In this table, the mixture (*a*) was used for the cylinders and pistons of the later gas engines built under the Author's supervision; mixture (*b*) is the one recommended by Mr. Cook in his paper on "Diesel Engine Castings" read before this Institution, and mixture (*c*) was used by the Author for cylinder covers and exhaust belts.

For the cylinder, cylinder cover and piston, heat conductivity is a very important factor in large engines, and internal strain in the metal due to temperature will obviously decrease if the heat gradient can be reduced. They may lead to the greater use of alloys of higher conductivity.

The chemical constituents and physical properties of some of these materials are shown in Table 2. The difference in the thermal conductivity factors of " Y " metal and cast iron should be specially noted.

Another aluminium alloy, used by the Admiralty, for the pistons of Diesel engines, contains $2\frac{1}{2}$ to 3 per cent. of copper, 1 to $1\frac{1}{2}$ per cent. of nickel, and $\frac{1}{2}$ to 1 per cent. of magnesium.

For cylinder liners, forged steel may be preferred by some designers, using alloy pistons in conjunction with them.

HIGH CONDUCTIVITY ALLOYS.

ALLOY		ELECTRON	"Y" METAL	SILUMIN	ADMIRALTY ALLOY	AIR BOARD 2.L.8.
ALUMINIUM	%	TRACE	92·5	86·06	95·0	86·3
COPPER	%	0·62	4·0	NIL	3·0	12·0
IRON	%	TRACE	NIL	0·54		0·8
LEAD	%	TRACE	NIL	NIL		0·1
MAGNESIUM	%	98·01	1·5	0·52	0·75	NIL
NICKEL	%	NIL	2·0	NIL	1·25	NIL
SILICON	%	NIL	NIL	12·88		0·7
ZINC	%	4·37	NIL	NIL		0·1
SPECIFIC GRAVITY	S.G.	1·79	2·80	2·69		2·88
YIELD POINT	TONS/□"	12·0	20·8	13·5		
ULTIMATE TENSILE STRENGTH	TONS/□"	18·0	26·9	14·75		9·0
ELONGATION	%	13·0	17·0	7·5		
THERMAL CONDUCTIVITY FACTOR		0·32	0·385	0·39		

NOTE :- THE THERMAL CONDUCTIVITY FACTOR FOR CAST IRON IS 0·10 TO 0·12

TABLE II.

Exhaust Boilers.—Though exhaust-heat boilers do not bear any obvious relation to large two-stroke engines, still, for the reason that they may in the future become an important element in marine installations, and that the Author has had considerable experience with their use in as much as they were fitted to almost half the engines installed, it occurred to him that some notes regarding them might be of profit.

The first installation of size which the Author remembers, was one near Birmingham twenty years ago. The type of boiler fitted was the " Wilson." This was of interest because of its special design, with some approximation to contra-flow heat transfer. More interesting for marine work is one of the double-back type, Fig. 53, which allows separate heating other than the engine exhaust. For marine work, the boiler must be available for service when the main engines are stopped; a separate oil-fuel burner is, therefore, necessary. A modern double-purpose boiler by Spencer-Bonecourt is shown in Fig. 54.

For gas engines the boilers have an output of 2 lbs. of steam per brake horsepower; for an oil engine, even with feed heaters, it would be less. If we take a consumption of 8,350 British Thermal Units per brake horse power and 30 per cent. of this rejected in exhaust gases, then for 100 lb. pressure and assuming a boiler efficiency of 75 per cent., the quantity of steam generated would be 1·58 lb. Is this sufficient for needs during the voyage, the other, the main oil-fuel burner, being used for port duty for the winches, etc.?

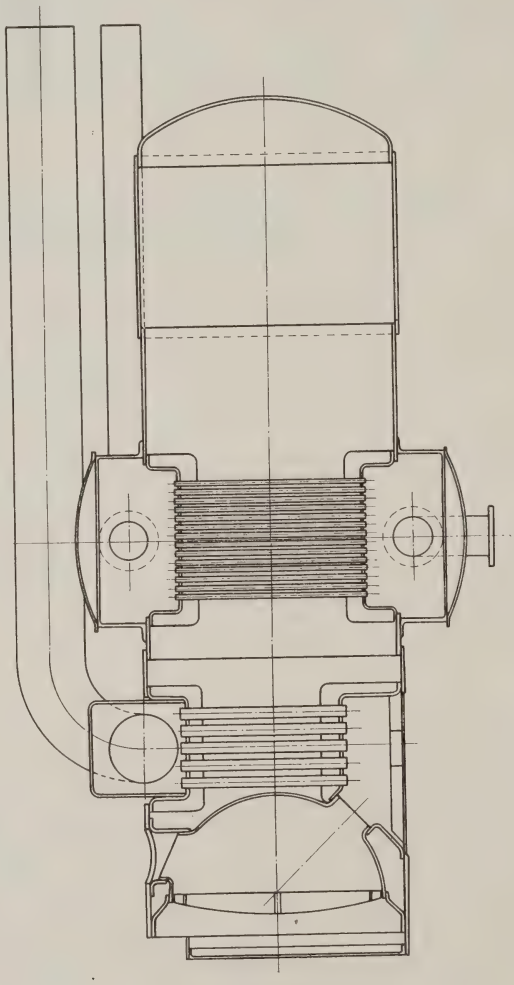


Fig. 53.

The whole question has to be very closely surveyed before any decision can be arrived at. At the present stage of development, it is most difficult to say whether it is worth while unless adopted with the elaboration of the Still engine, when the gain brings so many additions to the mechanism of the engine. The view of the Author is rather that if the amount of steam required for the steering engine, heating, etc.,

during the voyage can be met, whilst the port duties of unloading cargo are looked after by the oil-fuel burner, the balance is in favour; further, if, due to the use of lower compression pressures (as seems likely), and air-less injection, the main engine is arranged to be started by steam (there appears to be no fundamental reason why it should not), compressed air can be done away with, and the proposition is more favourable still. An exhaust-heat boiler for this purpose should be simple, small in size, with probably forced circulation and arranged to lie close up to the engine; in effect. a tubular heater with a steam

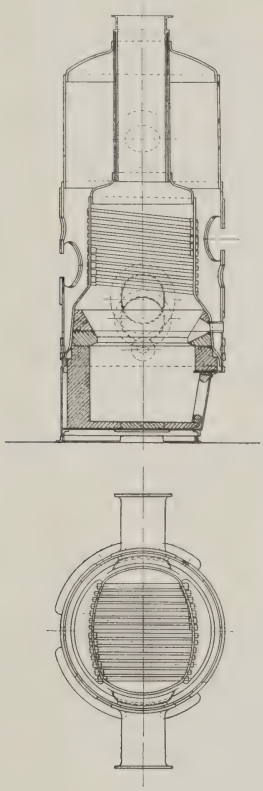


Fig. 54.

collecting drum above, contra-flow and fed with hot water from the cylinder heads. Such a type of installation is lower in first cost, and nearer in general to the usual steam installation, than the present high-pressure compressed-air Diesel engines with large auxiliaries and electric winches.

Finally, I take this opportunity of expressing my very great thanks to Messrs. Mather & Platt for making available to me all the data without which this paper would probably not have been written.

I would also like to refer to the work done by your Institution in the technical and practical development of the marine oil engine, and

if you would allow me, not only to express my great admiration of the excellence of the various Papers given, but of their correlation to each other, all apparently forming parts of a very sound plan for covering the whole ground. It places the Institution, I can very safely say, quite in the front of any other in this field of engineering.

DISCUSSION.

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E.: Under the inspiration of Mr. Chorlton's Paper there is hardly anyone present who does not want to ask him something or tell him something, for he has adopted the unusual line of asking many questions—and there is something refreshing in that. I will not, therefore, detain the meeting very long, although there are one or two points I would like to refer to briefly.

First of all, I wish to thank Mr. Chorlton for the very kind words he has said about the Institution. I know that some people think that a large percentage of the work of the Institution has been devoted to the question of internal-combustion engines, but what Mr. Chorlton has said makes me feel sure that we are on the right track.

We are so accustomed to talk about "Diesel" engines that what Mr. Chorlton has said about Akroyd Stuart has come home to me very much, and I think in future we will try to talk about the "oil" engine, more especially as it is associated with a good British name. As Mr. Chorlton has said—and he used a very apt aphorism—success in practice proceeds chiefly with small steps. The Author of the Paper has taken us through the various steps of the two-stroke engine, but in regard to the interesting photograph of an engine made by Mather and Platt he said it was run on gas and thought it would be successful if run on oil. I am sorry it was not run on oil, and hope Mr. Chorlton will say why.

Quite a number of communications have been received from various gentlemen on the subject of the Paper, and one of them from the Director of Civil Aviation,* General Sir Sefton Brancker—who lectured before the Institution last session—is of such interest that I am going to take the opportunity of reading it to you. (The President here read Sir Sefton Brancker's letter.) Letters have also been received from Admiral Sir George Goodwin, from Admiral Dixon (Admiral Goodwin's successor as Engineer in Chief of the Navy) and one from the Air Ministry from Vice-Marshal Sir Geoffrey Salmond. These are sufficient warrant of the importance of the subject and of the excellence of the Paper we have heard.

The President then called upon Engr.-Commdr. C. J. Hawkes to open the discussion.

Eng.-Commdr. Hawkes

ENG.-COMMDR. C. J. HAWKES, R.N. (ret.), *Member of Council*: I have been extremely interested in the valuable Paper that has just been read. The Author, as is generally known, has had a wide and varied

* See Correspondence, page 114.

experience with internal combustion engines, both gas and oil, and consequently he is able to speak with authority on the subject of the Paper. He deals with the difficulties encountered with gas engines of comparatively large power per cylinder and with the modifications in design that were carried out in order to overcome these difficulties, and, although the gas engine differs in some respects from the oil engine, I am sure that the information given in the Paper, some of which has been published for the first time, should prove of value to those interested in the further development of the oil engine. Eng.-Commdr Hawkes

The Author raises a number of very interesting points. Some time ago I had occasion to go into the question of the double-acting two-stroke oil engine and from the available information it appeared that the principal difficulties in the past had been in connexion with scavenging, fuel injection and piston-rod packing. From the point of view of efficiency, and also on account of the packing, it was essential that the combustion on the underside of the piston should be satisfactory and consequently in considering the double-acting proposal an attempt was made to design the engine round a combustion space of good shape. This, however, necessitated the adoption of at least two piston rods per cylinder with, as the Author points out, two sets of packing. Two sets of packing per cylinder certainly has objections but the difficulties should not prove insuperable. In the design of the double-acting engine special attention has to be given, as mentioned by the Author, to the design of the top and bottom covers, to the liners and to the provision made for the expansion of the liners, to the method of introducing the scavenging air, &c., but with careful design it would appear quite possible to produce a satisfactory engine. In the case of merchant ships, where there is generally plenty of head room in the engine room, there is something to be gained by the adoption of the double-acting engine where comparatively large powers are required. On the other hand, in the case of submarines, where the head room is limited, the double-acting engine is not so advantageous from the power-space point of view as perhaps one would expect, unless the stroke-bore ratio is kept down.

I had an opportunity of seeing at work one of the triple-expansion steam engines that had been converted to a Duplex gas engine, and it ran very well. The Duplex engine, which was introduced by the Author, certainly has some good points, although a unit involves two sets of packing, as in the case of the engine shown in Fig. 3. The combustion space in the Duplex engine, as a self-ignition oil engine, is not so satisfactory as in the engine shown in Fig. 3, although this difficulty might be overcome in the case of low-duty engines. I agree with the Author that it is desirable to have a valveless engine, and there is no doubt that some of the difficulties in regard to efficient scavenging, the disposition of scavenging and exhaust ports, &c., are overcome in the Duplex engine. It is essential, in my opinion, that separate liners should be fitted. Some years ago insufficient attention was paid in the design stage to the question of whether or not

Eng.-Commdr. Hawkes

it was possible to get satisfactory castings of the various parts as designed, and in at least some instances the foundry was expected to produce a satisfactory casting from any drawing furnished by the drawing office. It is not surprising, therefore, that difficulties were encountered and in these circumstances it was not unusual for the foundry or the material to be blamed. The bars in exhaust and scavenging ports should be cooled, and no difficulty has been experienced in doing this in the case of large engines. Experience has also shown, that a satisfactory joint can be made between the liner and the exhaust and scavenge belt or belts by providing a comparatively small shrinkage allowance, heating up the belt, or jacket casting as the case may be, by means of hot water and drawing in the liner with bolts. The history of the size and shape of exhaust ports, referred to on page 70, is most interesting. In this connexion it might not be out of place to refer to a high-speed two-stroke engine which was constructed some years ago, the height of the exhaust ports being made in accordance with the practice usual at that time. The designed power could not be reached and eventually the height of the exhaust ports was reduced with beneficial results. It is true that in this engine valve scavenging was adopted, but this instance emphasizes the fact that the height of the exhaust ports is of importance, although in some designs there is not the freedom that is possible in such an engine as the Duplex.

A piston on the lines indicated in Fig. 38 was designed a few years ago but so far as I know it has not yet been tried. I agree with the Author that alloy pistons are undoubtedly superior to the ordinary type, although they are at present more expensive and the experience with them on service is limited. In engines of the double-acting type the pistons, as is clearly indicated in the Paper, have to be carefully designed and in future it may be that alloy pistons will prove of value in the development of the large oil engine.

I agree with the Author that ground piston rings are not advisable in the case of large engines, and for some years I have advocated something on the lines of the proposal indicated in Fig. 42. In support of this, I would mention the experience gained some years ago with metallic piston-rod packing in a reciprocating-engined ship. It was usual for the metallic packing to be carefully scraped up to the rod—and some days were spent in doing this. It was also not unusual to find that in spite of the time and care taken in endeavouring to get a good job the packing was not satisfactory when under steam. I came to the conclusion that this was probably due to a slight distortion of the packing rings when heated and the scraped surface prevented them from bedding satisfactorily. Consequently I thought it worth while trying to get a serrated surface, as indicated in Fig. 38, and this was done by bedding the packing to the rod by means of a half-round file instead of a scraper. When steaming, after treating the packing in this manner, the packing leaked slightly, but in a short time it was found to be quite tight, which indicated that the serrations had allowed the rings quickly to find a bed under running

conditions. As the result of this experience no scraper was used for metallic packing whilst I was in the ship, and we obtained very satisfactory results. It seems to me, therefore, that in the large oil engine the same principle might with advantage be adopted for piston rings and piston-rod packing. The Author refers to the size and position of piston rings. Satisfactory piston rings are necessary in the case of high-compression oil engines and I agree with the Author that it is undesirable to make them too wide. In some of the early heavy-oil engines it was the practice to place the first ring close to the face of the piston and after a short period of running it was not unusual to find that this ring had gummed up and was of little value as a packing ring—although it had, perhaps, some value as a protection for the remaining rings. Subsequent to this the first packing ring was placed lower down the piston so that there was more chance of cooling it, and, in some instances, a protecting ring of comparatively stout section, and exerting a small radial pressure, was fitted above the first packing ring. In my opinion, a protection ring is an advantage so long as it does not exert too great a pressure on the liner, and this might be met by fitting restricted rings on the lines indicated by the Author—which reminds one of the restricted rings used in steam practice. Experiments with an obturator ring fitted with a protection ring were referred to in a paper which I read at Liverpool in 1920.* Eng.-Commdr. Hawkes

The Author rightly calls attention to the importance of efficient cylinder lubrication. Some years ago, in the design stage of a large engine, I suggested that it was desirable that a separate lubricator should be provided for each cylinder lubricating connexion, but the engine was made with only one lubricator per cylinder, provided with the necessary branch pipes to the various points to be lubricated. There was considerable trouble and the trunk of the piston was always running warm. At a later date separate lubricators were fitted on the lines previously proposed and no further difficulty was experienced. I agree with the Author, therefore, that the cylinder should be provided with multiple feeds, and one plunger and pipe per feed (page 97). By this means it is certain that, barring leaks, if the lubricator is working satisfactorily, which can be seen at a glance from the engine-room platform, the lubricating oil is finding its way to the point intended. In the case of multiple feeds to the cylinder with lubricators supplying more than one feed it is apparent that a partial choke will interfere with the correct distribution of the lubricating oil. The Author indicates, in some instances, where the lubricating points are placed. Would he state whether the introduction of the lubricating oil was timed in any way, or whether it was allowed to enter the cylinder when the piston was in any position.

The details of the various types of piston-rod packings used are exceedingly interesting and valuable. The Author calls attention to the

* *Proceedings*, Inst. Naval Architects, July 1920.

Eng.-Commdr. Hawkes

necessity for correct alignment in the case of such an engine as that shown in Fig. 4. Would he say whether these engines were lined up hot, as used to be done in the case of large reciprocating steam engines with such beneficial results.

I was very pleased to hear the Author refer to the use of the word "Diesel" and the claims of Mr. Akroyd Stuart in regard to the airless-injection heavy-oil engine. As a tribute to the work of a British engineer I should like to associate myself with the Author's remarks in this connexion.

There is insufficient time to deal with other points raised. In conclusion I should like to thank the Author for his most interesting and valuable Paper.

Mr. Bruce

MR. A. D. BRUCE, *Member*: The Paper is of special value because it deals with both the gas engine and the heavy-oil engine of the same type. The difficult problems of the one are also those of the other and since much experience has been gained during the development of the gas engine it is natural that information of importance should be available for the use of oil-engine designers. It is a fact, however, that in spite of the literature dealing with the gas engine, there is a lack of concise experimental data of the kind that makes for success in manufacture and such Papers as Mr. Chorlton's ought to be welcomed from this point of view.

Turning to page 62, the Author says "Will there be a reversion to the valve type (of scavenging) when the double-acting marine oil engine is evolved? The Author very much doubts it, as the valveless engine is very attractive." Valve scavenging is very efficient, but as the valves are placed in the cylinder head, the latter becomes a complicated casting and is subject to heat cracks. In certain covers of this type cracks 5 inches deep and 1/32 inch wide, when cold, made their appearance and when the engine was started up exhaust gases escaped through the cover; but later due to expansion of the metal, the cracks closed and the gases ceased to escape. The cracks took place usually between the valve apertures where there were no water spaces, and these cylinder heads, in many cases, remained in service for a long time after fracture.

Port scavenging, although in some cases less efficient, is safer, and the success met with is sufficient to enable an opinion to be given in favour of its final adoption. The opposed-port system is open to the objection that one side of the cylinder is hot and the other cold, which leads to some distortion, and uneven wear in the neighbourhood of the ports where lubrication of the surfaces is difficult, especially the bars between the exhaust ports which naturally are very hot.

The opposed-piston engine has a complete circle of exhaust and scavenge ports at opposite ends of the cylinder which arrangement gives very good results. An objection, which Mr. Chorlton attributes to two-stroke engine manufacturers, to building double-acting two-stroke

engines, is that, with the single-acting engine, trouble is already experienced with the ports; therefore, in the double-acting engine, the trouble will be greatly accentuated, on account of the extra amount of heat that the exhaust ports will have to pass. But Fig. 6 shows a type of engine where a set of ports is provided for each end of the cylinder. The engine is quite novel and has many good points. The figure is merely diagrammatic and cannot be taken as representing the proper proportions of the various parts. The connecting rod is quite sound and the movement of the cylinder not really formidable. Also, there is no stuffing box required and Mr. Chorlton has told the meeting how troublesome that part can be; even in the best designs it is subject to clogging from carbonized oil and has to be overhauled frequently. The combustion chamber is of simple form and the scavenging and distribution of the fuel good. Mr. Bruce

Referring to the Duplex engine, Fig. 16, with which Mr. Chorlton has been closely associated, the Author said certain modifications would be necessary to convert it into an oil engine. No doubt this engine has some good features which make it suitable for large powers, such as the pipe-like form of the internal castings which are unrestricted so far as expansion is concerned; but there are features which might prove troublesome and in some cases difficult to overcome. These are the presence of two stuffing boxes, and the difficulty of placing the fuel valves in a central position in each cylinder, on account of the position of the piston rods; and if the fuel valves were placed elsewhere, more serious local heating would take place due to the impingement of the flame on the cylinder parts. Perhaps the greatest difficulty would be that of the transfer passages between each pair of cylinders which must be kept large for the passage of the exhaust gases and the scavenging air, yet must be reduced in volume together with the main combustion spaces, to give the high compression pressure necessary for the ignition temperature when working on the Diesel cycle.

With reference to the question of piston rings, Mr. Chorlton has given a wall pressure of 7 lb. per square inch as being usual in designing and also given an instance of 2 lb. per square inch having been used. In my experience with fairly large cylinders the pressure usually adopted was about 5 to 6 lb. per square inch, and this has been found to be too great as the piston rings very frequently took a permanent set after being pressed over the piston body, and did not then fit the cylinder because they were no longer circular. For this reason the wall pressure was reduced to about 2 lb. per square inch which worked satisfactorily. Of course, as the Author carefully pointed out, a certain wall pressure is necessary just on starting up, but 2 lb. per square inch would be found enough if the rings really fitted the cylinder, and as for the working conditions, this pressure, which is obtained from the spring of the material, is of no consequence as the pressure behind the rings due to gas leakage is very much in excess of these values.

Mr. Bruce

There is little doubt that by removing the top ring away from the piston crown the wear on the liner would be slightly reduced due to the wire-drawing effect of the clearance between the piston and the cylinder which reduces the pressure of the gas behind the rings; but a more important reason is that the further the top ring is removed from the hot zone, the longer the ring remains free to perform its functions without the oil about it becoming carbonized.

The question of crank-shaft proportions and the shrinkage allowance is very important. For a time 0·001 of an inch per inch of diameter of the shaft was regarded as sufficient, being the accepted practice for steam engines; but unfortunate instances with oil engines have occurred where movement between the journals and the webs has taken place shearing the dowel pins. The shrinkage allowance has recently been increased to 0·00175 of an inch per inch of diameter of shaft with satisfactory results. When the conditions of high pressure and large torque are considered, the differences between the problem of the steam engine and that of the oil engine become apparent.

It is understood that Messrs. Mather & Platt did not fit dowel pins at all, which means that they relied entirely on the shrinkage allowance which, I believe, used to be about 0·00175 inches per inch of diameter of shaft. This is interesting, as Lloyd's insist on large dowel pins—no doubt with the idea that they may bring a ship into port in the event of a failure in the crank shaft occurring.

Mr. Swinnerton-Dyer

Mr. L. S. SWINNERTON-DYER (*Graduate*): The conversion of a successful gas-engine design to oil will necessitate considerable alterations in the cylinder owing to the difference in compression ratio. For marine purposes the Duplex type (Fig. 4) has the advantage of reducing the length of the engine, but at first sight it would appear that the design of the transfer passage between the cylinders would be a matter of difficulty owing to the higher compression. In order to obtain the requisite cross-sectional area of passage, either the height could be kept as shown which would give a rather awkward shape of combustion space, or else the height could be lessened and the breadth increased which might assist the injection, but at the same time appreciably increase the area of walls exposed to the gases at point of maximum temperature. Since about 80 per cent. of the heat loss occurs in the first 45° after t.d.c., this point would appear to be of importance, and applies even more to the bottom half of the cylinder as, owing to the obliquity of the connecting rod, the piston is moving slightly slower in the neighbourhood of b.d.c. than it is near the t.d.c.; consequently the heat loss is greater. The efficiency of injection is also lessened by the obstruction of the piston rod. The increased mechanical efficiency of the double-acting engine does not appear to compensate for these losses, and, at present, it would seem that the fuel consumption would be probably 10 per cent. greater than in the single-acting.

Mr. Swinnerton-Dyer.

Some time ago I got out the rough preliminary designs for three two-stroke engines to develop the same power at the same revolutions, three-cylinder and six-cylinder double-acting (type as in Fig. 2) and six-cylinder single-acting. The comparative weights were 100, 105 and 138. Are these "drawing board" figures at all in accordance with Mr. Chorlton's practical experience?

The reasons for the comparatively small saving in weight in the double-acting engines were: (1) increased height in terms of stroke; (2) small saving in cylinder weight; (3) the weight saved on the crank-shaft was rather less than was at first hoped owing to the relative unevenness of the resultant twisting-moment curve for double-acting engines. This is due to: (*a*) different connecting-rod angle for piston position at top and bottom sides; (*b*) smaller area of piston on bottom side due to rod—about 11 per cent.; (*c*) weight of reciprocating parts acting downwards—about 14 per cent. of m.i.p. per square inch of piston area. (*a*), (*b*) and (*c*), together make a difference of over 40 per cent. in mean twisting moment for upward and downward strokes. The maximum value of the ratio of maximum to mean twisting moment came out for the three engines as 1·81, 1·36 and 1·51.

The saving in cost of the double-acting engine will be rather less than the saving in weight as, while the cost of fitting and machining per b.h.p. will be nearly the same as for the single-acting, the cost per ton should be somewhat greater. It does not seem, therefore, that in first cost the double-acting engine will show a great saving over the single-acting; in both running costs and accessibility it will be at a disadvantage. I am fully aware that I am in the unpopular position of being an advocate of the single-acting engine, but I cannot help feeling that, at present, it will be a matter of very great difficulty to convince a shipowner that the double-acting oil engine is a dividend-earning proposition.

Finally I hope Mr. Chorlton will consider these comments on his very valuable Paper to be more in the nature of questions than assertions.

Mr. H. Hunter

Mr. HARRY HUNTER, *Associate Member* (of Messrs. Alfred Holt & Co.): It is interesting to learn from Mr. Chorlton that more trouble has been experienced with piston-rod packing of double-acting gas engines than has been experienced with the piston rings. In the case of the marine steam engine, particularly when using high-pressure superheated steam, the rings as a rule require more attention than the rod packing, nor does the type of ring fitted appear to be quite so standardized as is the rod packing. For instance, one may find two ships in a port, the superintendent of one removing, say, the "X" type piston rings, and fitting Ramsbottom in lieu, and the superintendent of the other doing exactly the converse. Perhaps Mr. Chorlton will say whether it is considered advisable to reduce the indicated pressure

Mr. H. Hunter

on the cross-head side, as compared with that of the top side, out of consideration for the rod packing.

With regard to the liners in the double-acting engine illustrated, it appears necessary when new liners are required, to bore them in place, or alternatively, to remove cylinders to machine for boring. This would appear to be a disadvantage of this design of engine. It would be of interest to know what amount of expansion is allowed for between the two portions of the liner, and whether in practice there is any tendency for the corners of liners at expansion joint to burn off.

Is it possible to detect when gas is leaking from one side of the piston to the other? In the case of the single-acting engine it is, of course, very readily detected, and the necessary action taken, or the piston withdrawn at the first opportunity, before any more serious results have developed. This question would appear to be of greater importance with the Diesel engine, than with the gas engine. In the double-acting engine it is to be anticipated that a greater mechanical efficiency will be obtained than with the single-acting engine, but perhaps Mr. Chorlton may be able to give some information on the point.

The question of auxiliaries in motor-engined ships has, as Mr. Chorlton says, to be closely considered before a decision is arrived at. For instance, in the case of the ship now being fitted with Scott-Still engines, although there is a complete steam installation down below, the winches on deck are electrically driven. In this case it was estimated that with electrically-driven winches we should burn a quarter to a fifth of the quantity of fuel oil required with steam-driven winches. Further, we were in any case tied to a considerable dynamo power by reason of the turbo blowers being electrically driven. The cabin heating in this ship is also by electricity.

I was recently aboard a motor-ship fitted with a range in the galley through which the main-engine exhaust was passed. In this case, however, the heat available, although sufficient to boil water, was not sufficient for baking bread. It would thus appear that when Kipling comes to write MacAndrew's Hymn for the Chief Engineer of a Motor-Ship he will have a more prosaic reason for Ferguson putting up the revolutions in the middle watch.

I would like to join with the previous speakers in thanking Mr. Chorlton for his most valuable and interesting Paper.

VOTE OF THANKS.

Mr. Waldie Cairns

Mr. C. WALDIE CAIRNS, *Vice-President*, said there was no need for him to say anything except to express the meeting's hearty thanks to Mr. Chorlton for his Paper. Mr. Chorlton had paid the Institution a delicate little compliment by his remark as to the reputation which it had attained for Papers on the internal-combustion engine. Mr. Cairns thought they might fairly return the compliment, assuring Mr. Chorlton that his own paper would certainly increase the prestige which he had stated the Institution had already earned.

CORRESPONDENCE.

ENG. VICE-ADMIRAL R. B. DIXON, *Engineer in Chief of the Fleet*: Mr. Chorlton is to be congratulated on the care taken in assembling important information respecting the past experience and present position of the double-acting internal-combustion engine; in giving special emphasis to some of the difficulties yet to be overcome he has opened up fruitful matter for discussion. Eng. Vice-Adm. Dixon

The double-acting two-stroke cycle engine using oil fuel and the possibilities of its successful application for high powers, are matters that have been seriously considered for naval purposes. The disadvantage for naval purposes of the single-acting heavy-oil engine, including both the four-stroke and two-stroke cycle types, is the excessive weight and size, which, even after allowance is made for the smaller amount of fuel required for the same radius of action as compared with the oil-fired-boiler-steam-turbine combination (quite apart from their present inability to develop the high powers required) in general render their application out of the question, except in the case of submarines. On these grounds the Admiralty are closely interested in any development of the double-acting two-stroke engine in which increased power per unit weight is promised. There is a real need for a successful engine, capable of developing high powers in each individual cylinder—at least until the long awaited internal-combustion turbine develops into an economical and practicable proposition, and supplants it.

In the struggle for existence which must ensue between the various types of heavy-oil engines for commercial purposes, of many of which Mr. Chorlton gives us descriptions, the most efficient will no doubt survive; but in this connexion it is observed that Mr. Chorlton has not referred to the mechanical efficiency. It is well known that the mechanical efficiency of the different types of heavy-oil engine varies considerably, and an engine with a low figure in this respect would necessarily compare unfavourably in respect to the important aspect of durability. The question of accessibility must also be kept well in view by the designers of these engines, and the availability of internal parts for cleaning is of the very first importance if its durability is to be maintained.

Eng. Vice-Admiral Sir George Goodwin, in a paper recently read in Glasgow, intimated that the Admiralty have under construction a double-acting experimental engine of large power for installation and test in the Admiralty Engineering Laboratory, in the design of which the experience of the past, coupled with that obtained with other experimental work at that establishment, has been incorporated. Without being unduly optimistic, it is hoped that some of the difficulties indicated by Mr. Chorlton have been foreseen and will be overcome in the design of this engine, although restricted space, especially in the direction of head room, compels this engine to be of the high-speed type, and somewhat increases the difficulties.

Eng. Vice-Adm. Dixon

Referring to one important difficulty in the double-acting engine, viz., that of obtaining a satisfactory combustion chamber on the rod side of the piston, it is considered that, as the questions of efficient injection and combustion are of outstanding importance, especially for engines of high speed and power, any disadvantages obtaining from fitting multiple rods which enable the space to approach the ideal must be accepted. These disadvantages, however, are not necessarily so great as may appear at first sight. By fitting two rods and suitably disposing the same with respect to scavenge and exhaust ports so that these rods are exposed to the same temperature conditions, the possibility of uneven relative expansion largely vanishes. Also, if these piston rods are used as the oil ducts for the piston cooling, their variations in diameter at different portions of their lengths should be such as can be easily absorbed in a comparatively simple design of metallic packing; nor should their alignment with respect to the cover be appreciably affected. Although additional glands are introduced, still two smaller glands should offer no greater difficulties as regards maintaining gas-tightness than one large one.

As regards the relative importance of the principal engine parts enumerated, it is still considered in the light of present experience that the cylinder covers are probably the most important and the design with separate thin cooled combustion plates will probably be found most satisfactory, at least for moderate and high powers. Certainly the question of arranging for cooling the bars of the exhaust ports would appear also to present very great difficulty, but it is possible to arrange for a design which is comparable to that already fitted in some well-known and successful makes of single-acting two-stroke engines. Cooling of the exhaust bars is an essential: otherwise cracking must always be expected, and moreover carbonization of lubricating oil in the neighbourhood of these bars will be a fruitful cause of wear of the liner at this position.

The cooling of the head of the liner is also recognized to be an important feature in high-powered internal-combustion engines; this device has been definitely established as a practicable proposition in one of the Admiralty Laboratory engines, in which a mean effective pressure up to 150 lbs. per square inch has been obtained.

It is agreed that ready adjustment of the cross-head must be provided in a double-acting engine to maintain correct alignment of the piston rods.

There is one point to which an exception to the Author's remarks is taken, and that is, as to the possible adoption of 24-inch high-conductivity-alloy pistons without water or oil cooling of any kind. It is considered that it would be practically impossible to run any piston of 24-inches diameter, even on the four-stroke system, without water or oil cooling. The higher conductivity of the alloy piston warrants the assumption that heat will flow to it more readily than is the case with the ordinary piston. As the greater portion of the heat flowing to an

uncooled piston has eventually to pass to the liner by way of the rings, these will be the principal factors in regulating the flow. They will most probably be of cast iron, although it is noted the Author suggests a possible use of " Y " alloy, and it follows they will have to run at a higher temperature to give the necessary gradient to deal with the increased heat flow. As a consequence, greater wear might be expected and a greater liability to carbonize the lubricating oil and gum up. It is not unreasonable to assume on these premises, that the high-conductivity-alloy piston would require more cooling than the ordinary cast-iron piston. Eng. Vice-Adm. Dixon

The foregoing constitutes no argument against the high-conductivity-alloy pistons, and in view of their superior qualities in this particular direction, combined with the large reduction in the weight of the reciprocating parts obtainable, they are looked upon as almost a necessity for high-speed engines. The Admiralty has now sufficient experience to believe that the special alloys in use are suitable for the highest powers that can be foreseen at present. Certainly great care has to be given to their design, but it can be stated that the design of a piston for a double-acting engine with external cross-head, is if anything simpler than that of a trunk piston of the same diameter.

So far as piston rings are concerned, it is considered these should preferably be of thin section but at the same time provide for an adequate radial depth in the piston. This gives a light ring with a good bearing surface and reduces the inertia load of the ring on the piston. It is necessary in two-stroke engines to locate these rings radially to permit easy running over the port bars. The trial of the piston ring of " Y " alloy referred to by the Author is interesting, but before adopting such rings in continuous-running engines, it will be necessary to know the life of these rings—at present they are in the purely experimental stage.

The statement as to the trial of the piston rods, one case-hardened and one oil-hardened, is of great interest. Special care was taken in the selection and treatment of the material for the rods of the Admiralty experimental engine, and case-hardening was rejected for the very reason given in the paper, viz., it was considered that surface cracking would occur. The material now chosen is $3\frac{1}{2}$ per cent. nichel steel, ground to size. Of all the comparatively heavy parts of the engine these rods require the most careful treatment and handling if a good surface is to be maintained.

Of the large number of metallic packings, that shown in Fig. 47 was considered the best design, and it is noted that the Author's experience with this type has been satisfactory.

As regards the lubrication of the piston and liner, it would appear preferable so to design the parts that the lubricating-oil holes and any oil-ways are never uncovered by the piston, even if this can only be achieved at the expense of increasing the height of piston and cylinder.

Eng. Vice-Adm. Dixon

In conclusion, this paper should be of much interest to designers and builders of heavy-oil engines, as the Author has brought out many points which, from his experience, require careful consideration in the design and manufacture of large double-acting two-stroke engines.

Maj.-General Brancker

MAJ.-GENERAL SIR W. SEFTON BRANCKER, K.C.B., A.F.C., *Director of Civil Aviation*: The development of the type of engine that Mr. Chorlton describes is of intense interest to me as Director of Civil Aviation. If we can get an engine that can be kept running for 600 or even 1,000 hours without serious overhaul, and which will burn heavy oil, without increasing the total weight of the present petrol engine and its fuel to any serious extent, we shall have made a really serious stride towards making Air Transport into a commercial proposition without any artificial assistance. Such an engine would enormously reduce the cost of maintenance, would bring the cost of fuel down to about one-fifth of what it is to-day, would eliminate the magneto, which is one of our weak points at present, would very much reduce the danger of fire in case of accident, and would make flying just as safe as any other means of transport.

We have got to a point in aviation now where we can sacrifice a certain measure of lightness in order to obtain reliability, and in the design of such an engine I would urge that springs, valves, etc., should be made thoroughly robust. One weakness in the present engine is that its revolutions are of necessity extremely high to produce the horsepower for which it is designed. In the interests of reliability and cheapness of maintenance, it is of urgent importance to reduce these revolutions, if it is possible, without losing efficiency; I understand that this will be possible in the type of engine about which Mr. Chorlton is going to speak.

Brig.-General Bagnall-Wild

BRIG.-GENERAL R. S. BAGNALL-WILD: The problems with which marine engineers are predominately concerned are correlated with those appertaining to internal-combustion engines for aircraft.

The chief of these problems, namely:—

(*a*) the ability to utilize economically the cheap and heavy grade fuels, and

(*b*) obtaining the maximum volumetric efficiency, with its consequent influence on the weight b.h.p. ratio.

are considerations of extreme importance.

Whilst the Author confines himself to the historical review of the practical difficulties encountered in the development of double-acting two-stroke engines, the Paper in no degree loses its importance on that account. The sequence of changes in design to meet the conditions peculiar to this type of engine, are of basic interest, and it is the candid exposition of difficulties and the subsequent steps to overcome these difficulties that add greatly to the value of such a contribution.

Brig.-General Bagnall-Wild

The question of materials used in the construction is one of the important factors upon which the success of a design is dependent. The problem does not end with the selection of suitable materials, for the various component parts, but should be extended to the source, and represent the results of close co-operation with the steel manufacturers and founders. There has been a tendency to stand still in regard to forms in which materials may be employed. Undoubtedly, the most profitable lines of development are to regard light alloys generally and high tensile steels as the basic materials for internal-combustion engines, and to adapt these materials to those forms in which they may be used to the greatest profit and security.

An example may be offered in the use of light alloys in the forged state for the pistons. The Author has emphasized the possibility in this direction, and it will probably be of interest to relate that a unit constructed for the Air Ministry employs a forged aluminium piston, 12 inches diameter, the power output of the cylinder unit at normal speed, *i.e.*, 1,000 r.p.m., being 200 b.h.p. I would also like to refer to the consideration of forgings in various directions in place of castings as offering a greater degree of security.

The selection and satisfactory application of lubricating oils, to which the Author also briefly refers, is a diversion and a subject of extreme importance in itself. His reference to the alternative trials with an expensive and a cheap oil, suggests that the satisfactory results obtained with the cheap oil are due not so much to the characteristics of the lubricant used as to the fact that the cheaper oil was applied in the correct manner and in the correct quantity. With certain grades of lubricating oils, the acid content of the oil is such as to produce corrosion on component parts of the engine during storage, and is a factor that also requires consideration in the selection of lubricating oils. This is particularly pronounced in vegetable oils and animal fats, and is further accentuated in that certain oils which originally appear free from acid develop these properties in storage.

The satisfactory production of internal-combustion engines employing a heavy-grade fuel whilst maintaining a reasonable weight ratio is of extreme importance and urgency to the aircraft industry. I am hopeful that good results will be attained before long in these developments, and economy ultimately obtained with lower compression pressures than those generally now in use in the true Diesel.

The progressive stage of aircraft engines calls for the searching of every channel to obtain efficiency, and I should like to call attention to the fact that a four-stroke double-acting engine was designed and a unit produced in 1916 by my experimental establishment. The design, Figs. 55A and 55B, if analysed, will be found to possess interesting basic features as affecting the internal stresses and bearing pressures when compared with the normal type of four-stroke engine. The experiments were interesting, and indicate that the problems met were surmountable.

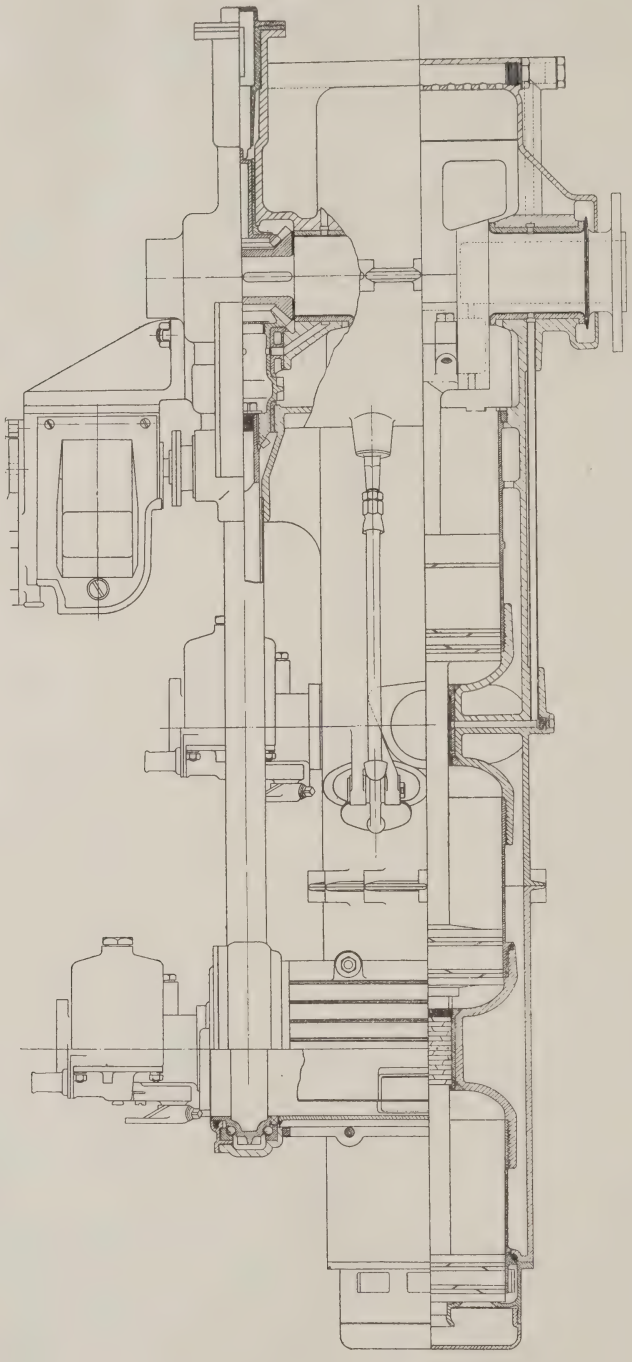


Fig. 55A.—Arrangement of Double-Acting Tandem Engine, R.A.F. 12T.

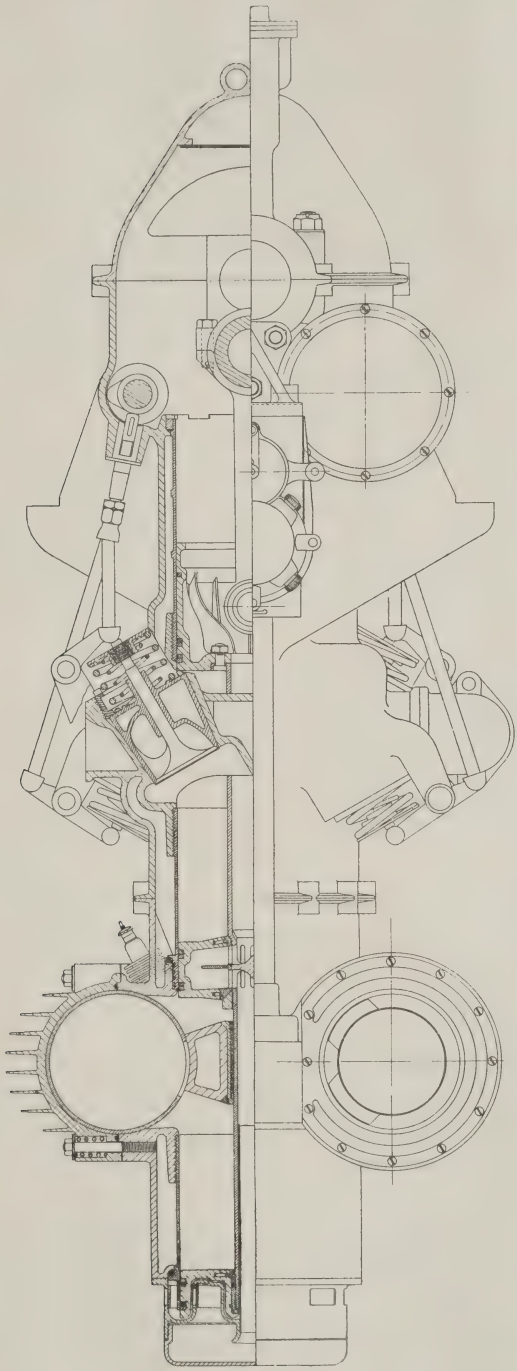


Fig. 55B.—Arrangement of Double-Acting Tandem Engine, R.A.F. 12T.

Mr. Rennie

MR. ARCHIBALD RENNIE (of Messrs. Scott's Shipbuilding and Engineering Co., Ltd.): Mr. Chorlton deals with matters of great interest and importance to all who are engaged in the design and working of internal-combustion engines, and it is evident that he has drawn upon a large and varied experience in presenting his Paper to the Institution. The last section of the Paper—that dealing with exhaust boilers—is in some respects the most interesting, for in this section the Author indicates certain lines along which the rational development and improvement of the internal-combustion engine might be expected to take place, all of which have already found expression in the Still engine. And, as in this section also, Mr. Chorlton conceives the Still engine to be an elaborate piece of mechanism, it would appear that his knowledge of that engine is not up to date, and I venture, therefore, to call attention to these matters.

In the section above referred to, Mr. Chorlton points to the desirability of recovering the waste heat in the exhaust gases, and suggests that this heat might be used to generate steam which could be used to work some of the auxiliary engines at sea; and if there should be sufficient steam available he sees no reason why it should not be used in the main engine itself for starting and manœuvring purposes, thereby simplifying the engine by eliminating the starting-air compressors and all their gear. There can be no question that these are desirable ends to aim at, but the surprising thing is that Mr. Chorlton does not seem to know they are all embodied in the Still system; even his suggestion of a boiler with separate paths for the gases from the engines and those from the burners, is to be found in the Scott-Still engine.

In connexion with the weight of steam per brake-horsepower generated from waste heat, to be expected in certain cases, I agree with Mr. Chorlton's figure for gas engines but consider his figure for oil engines requires some qualification. If he is considering the waste heat in the exhaust gases alone, it would not exceed 1 lb. per b.h.p. at the very best; if, however, he is including some heat recovered from the cooling water, as his later reference to hot feed from the cylinder head would suggest, then his figure is probably about right. But why limit the recovery of heat from the cooling water to the mere heating of feed water? Why not recover all the waste heat in this water for the generation of steam as is done in the Still engine? In this case the quantity would be from $2\frac{1}{2}$ to 3 lb. per b.h.p. These figures are based upon actual results obtained from the Scott-Still engine. But the point I wish to call attention to is that here again Mr. Chorlton visualizes the Still system.

In passing, it might be said that it is a much more difficult matter to recover effectively the heat that is lost to the jacket and piston cooling water than it is to recover the heat carried off by the exhaust gases, and so far as I know, the Still system is the only system which satisfactorily solves the problem. In Still engines the heat lost to the cooling water, which is in ordinary Diesel engines practically equal in quantity to that

carried off by the exhaust gases, is recovered and used in the generation of steam at practically 100 per cent. efficiency; hence the reason for the greater weight of steam generated. Mr. Rennie

It will be apparent that if steam generated from waste heat is used for starting purposes, as suggested by Mr. Chorlton, considerable simplification of the engine can be made; but there is no reason why the steam should not be used for propulsive purposes also, and in the Still engine it is so used continuously with much benefit to the economy of the engine.

Elsewhere in his Paper Mr. Chorlton suggests the use of a compression pressure of 300 lb. per square inch, or even lower; and further on he would like to abolish the water- or oil-cooling services to the piston, with all their gear; whilst still further on he deals with the troubles of gland packings in double-acting engines. It is not without interest in these connexions to note that the normal compression pressure of the Still engine is 300 lb. per square inch; that no water or oil-cooling service is required for the pistons; and that, although the Still engine is a double-acting engine in principle, the piston-rod packing is required only to stand up to steam conditions and is, in fact, an ordinary type of metallic packing such as is met with in any good design of reciprocating steam engine.

I feel sure that a study of the Still engine, as it is to-day, will convince Mr. Chorlton that, apparently without knowing it, he has laid his imprimatur on the Still engine both in principle and in practice. How else can he reconcile these advantages and simplifications which he desiderates, and which are found in the Still engine, with his view that that engine is elaborate?

In view of what I have said, it will be apparent that I thoroughly endorse Mr. Chorlton's suggestions to make use of the waste heat of combustion for the generation of steam for use in the main engine itself and in its auxiliaries, and I agree with him that one of the lines of development of the internal-combustion engine lies in this direction. I have, however, tried to show that much has already been accomplished along this line of development, which finds realization in the Scott-Still engine, and indeed at the present time it would seem that this engine marks the furthest point of advance in the marine internal-combustion engineering world.

Mr. P. BELYAVIN, *Member*: It is very seldom that one can find so much useful practical information and data as are given in Mr. Chorlton's Paper. It is a very interesting and important Paper indeed and I do not intend to criticize it, but simply to add some comments on certain points raised by the Author. Mr. Belyavin

Mr. Chorlton says: "Up to now the use of the single-acting engine has been almost universal, but the time has arrived when the high first cost and heavy weights have practically compelled engineers to turn to

Mr. Belyavin

the double-acting type as an avenue of relief." This statement is, of course, perfectly true; but is it right to follow this direction for relief? I would very much like to know what makes Mr. Chorlton think so. All investigations and calculations indicate that no double-acting or opposed piston engine of any known type can possibly be smaller or lighter, and, of course, simpler than a single-acting two-stroke engine: this is not merely a matter of opinion, for a comparison of existing engines of both types will prove it. If Mr. Chorlton is basing this assumption on experience with the stationary gas engine, I would emphasize that there is a great difference between a marine Diesel engine and a stationary gas engine.

Cost, weight and size are the greatest handicaps of the marine oil engine when compared with the steam. I have tried many times to solve the problem without impairing the engine's simplicity. Many years ago I thought the opposed-piston engine was the solution; one of my early attempts may be found in an article which I contributed about ten years ago to *Der Oelmotor*, *Rivista Marittima* and some other papers, giving a sketch of an opposed-piston engine of large power, designed as a propelling unit for a battleship. Since then I have been continuously investigating this problem, treating the question with quite an open mind.

I would not say that some type of double-acting engine could not be invented, which might have certain advantages if compared with the single-acting engine; but most certainly this type is not yet invented, and all the existing types are very much inferior with regard to simplicity to a conventional two-stroke single-acting type, and cannot be built cheaper, lighter and smaller than the latter.

This statement everybody can easily verify by sketching engines of various types. Taking the parts subject to tension, compression or bending stresses, in a single-acting engine, when running, in all these parts the load is changing from nothing to maximum; in a double-acting engine from + maximum to − maximum. For the same margin of safety, the stress in the second case should equal only half the stress in the first case; thus the weight of all parts, subjected to maximum loads on the piston, will be twice as much for the double-acting engine with the same cylinder diameter as that of a single-acting engine.

Again, the weight of the double-acting engine and the width of the base will be considerably increased, whereas the power will be only about 50 or 60 per cent. greater, not taking into account difficulties in the design; thus, it is evident that the weight and size in this case must be greater than those of a single-acting engine for the same power, and, as a matter of fact, it is so in the existing engine. All the other types of double-acting or opposed piston engines are in no better position, being, in addition, much more complicated.

The question arises: Why build larger, heavier and more complicated and more expensive engines? It would seem that the shipowners are misinformed, misled or simply lost amongst numerous types of engines.

Even the old controversy of two- or four-stroke engine does not appear to be clear enough for many of them. The old argument in favour of four-stroke engines was, that it is more reliable than the two-stroke engine; and it was quite true for the old days. However, with numerous two-stroke engines running now in ships with perfect reliability, this old argument has lost all its grounds. Mr. Belyavin

As there can be no doubt of the well-designed two-stroke engine being smaller, lighter and practically as economical and reliable as a four-stroke engine, it appears that the days of the four-stroke engine are numbered; it has rendered splendid services in the past, when a two-stroke engine was not studied enough, but before long it will gradually disappear from the market.

More light is necessary on all the above-mentioned questions, and more endeavours should be made to cheapen and simplify, and not to complicate, the marine oil engine.

Mr. J. L. CHALONER, *Member*: The Paper is a very valuable contribution to the technical literature on internal-combustion engines. In view of the great interest which is taken at the present moment in the development of the high-power oil engine, the information in the Paper is of specific interest, and the Author has by this very timely contribution rendered great service to all engineers and designers who are studying and investigating the many problems connected with internal combustion. The contents of the Paper make it quite clear that much resourcefulness and initiative were displayed during the early development of this type of engine to find practical solutions. Moreover, the frank and open mind with which the Author has approached this subject has produced much instructive and interesting material beneficial to the whole industry. Mr. Chaloner

Whilst the problems of design and construction are dealt with specifically in the light of gas-engine practice, they are applicable also to a very large extent to oil-engine construction. One cannot help, therefore, but feel that by dealing in such great detail with the magnitude of the pioneer work of the double-acting design the Author has strengthened the hand of the advocates of the opposed-piston engine. In fact engineers who look upon the opposed-piston type as a purely temporary measure will find much food for reflection in the Author's discourse. Pistons, cylinder covers and piston-rod packings have required much experimental work, for which the reward has not been proportional.

The Author lays stress on the fact that the service of the double acting engine has hardly been such as must be expected for everyday use, and on several occasions he refers to the main difficulty being not so much one of complications of design and construction as of the fuel. Yet it does not appear quite clear why a dirty gas should be made responsible for the apparent failure of any engine unless the Author's remarks imply that the additional expenditure incurred in dealing with

Mr. Chaloner

the gas problem would be sufficient to impair the commercial aspect of this type of engine. At the present moment, when the minds of engineers are preoccupied with the introduction of oil engines in large sizes, this point is of more than general interest, and one might be led to the conclusion that as regards the flexibility of an engine with specific reference to a wide range of fuels, the double-acting engine brings forth a definite set of new problems to be solved as regards atomization and combustion efficiency. The Author compels one to conclude that with large double-acting engines—whether gas or oil—the two-stroke principle will predominate. Moreover, it is more or less clear that port scavenging must become universal practice.

The experimental work on the various shapes of the scavenging ports is very interesting although it is doubtful whether by accommodating the requirements of piston rings in such a considerate way as advocated by the Author one may not be compelled to sacrifice some valuable percentage of the overall efficiency. It would be instructive to hear a little more from the Author regarding this point, and with specific reference to the scavenging efficiency and power output as a result of his suggested port design—the more so because, whilst the Author takes a favourable view of the efficiency of the round holes as scavenging ports, he does not retain them with later designs, neither is this design of scavenging ports adopted in the German Klein engine. In this connexion, would the Author be good enough to give the leading dimensions of the 7,000 h.p. engine referred to, and any comments on the quality of gas used and the fuel consumption obtained?

The reference to a compression pressure of 300 lb. and even lower also touches on a very vital factor in conjunction with future oil-engine design. A low compression and easy rating may be a simple means of producing cheap castings, but it does not necessarily bring such an engine on a competitive base as regards cost per h.p. developed. Whilst low-running charges are a decided advantage, the initial capital expenditure of any power plant must not be overlooked, and hence the engine builder will be faced with the necessity of a compromise between these factors. One doubts very much in the face of present knowledge whether the oil engine will be able to run with a lower compression than 380 lb. to accommodate the conditions which are set by the commercial range of fuel oils.

The Author indicates clearly how very important a part the metallurgist will play in connexion with large-oil-engine construction, and his reference to the utilization of alloys or similar material high conductivity for such vital parts as pistons is very encouraging. His reference to the gradual evolution of practical designs with specific reference to cylinders and pistons indicates that symmetry as an aid to producing castings or parts which are subject to temperature fluctuations can do much to produce satisfactory results.

Mr. C. ANDERTON BROWN: *Cylinders and Belts*—The designs shown in Figs. 17 and 22 should be taken in combination and represent a thoroughly well cooled cylinder and exhaust belt the joints being made on narrow faces. The principal reason for the water-cooled bars between the ports was to secure better lubrication and in this matter the design was particularly effective. The packing bushes at the lower ends of the cylinder are water-cooled also. Mr. Brown

The design of cylinder shown in Fig. 15 was only produced shortly before the war and though more expensive than previous types in the first instance, it makes a very useful type of cylinder in which no complicated coring is required, and in the case of a continuous-running engine renders it possible very easily to renew the liner and thoroughly scale the exhaust belt. Many of the early cracked exhaust belts and head troubles were undoubtedly due to the use of hard water which it is difficult to avoid in colliery districts.

Difficulty was experienced in cylinders of the type shown in Fig. 16 in sufficiently raising the compression, and the piston had to be very much domed whereas in the somewhat later type, where a centre bearing of the crank-shaft is dispensed with, the piston could be made fairly flat for compression up to 140 lb. per square inch (see Fig. 20).

The type of sleeve valve (Fig. 20), in spite of the fact that it carries three pump covers, gives remarkably large port and passage areas and enables the proportion of pump work to be reduced to an extent previously unapproached with other types of sliding valves. In spite of its considerable weight and large rubbing surfaces this type of air pump, particularly in the case of an oil engine where a single piston only is required, is very attractive.

Cylinder Covers.—With reference to Figs. 24 and 28, it is obvious that 28 relies on a single thickness of metal for strength and a later design was produced wherein the flange of the inlet pocket was also cut adrift from any rigid connexion with the rest of the shell. This design is easily accessible for cleaning.

Pistons.—The pistons illustrated represent a rather miscellaneous collection and, judging from the number of rings, Fig. 30 represents a very early type. Four rings at each end were found to give the best results. It will be noted that the rings shown are crowded towards the ends of the piston by reason of the desire for a long white-metal slipper in the case of horizontal engines; also to avoid wearing a ridge at the end of the cylinder the rings were made to match or slightly to overrun the bell mouth in certain cases. Cod pieces, stop pegs, etc., of ordinary types were successfully used on horizontal engines where the piston-ring joints could be sealed by being put at the bottom; but in the case of vertical engines where the piston speeds were 900 to 1,050 feet per minute, steel pistons were produced from solid forgings and excessive trouble was at first experienced with broken pistons due to leakage past the ring joints and cracking commencing as from the stop pegs. In certain cases considerable corrosive effect takes place within steel pistons

Mr. Brown

due to the corrosive action of the water in the presence of snifted air required to reduce shocks.

The use of milled grooves and round-ended rings completely overcame these troubles and no difficulty was experienced with steel pistons in any cast-iron cylinders when the lubrication was maintained.

Piston Rings.—The tendency in the light of later experience was to use narrow rings of fair radial depth, but, with stop pegs of the ordinary type, running over ports was apt to damage the somewhat thin ends. This trouble was also overcome by making the stops integral with the piston, but part of the rings ends could be made to cover the stops.

Some of the methods used in this gas-engine practice may perhaps appear rather drastic and expensive but for long runs on heavy duty first cost must not be the only controlling feature.

It may be mentioned in comparing large gas engines with oil engines that the ever-present fear of pocketing, lingering flame and resultant pre-ignition, introduce features that one would gladly avoid. Thus the long unringed end of the oil-engine type of piston might by the time a cylinder re-bore was due, for a horizontal gas engine, become a very fruitful cause of pre-ignition.

Mr. Myles

Mr. DAVID MYLES, *Fellow*: The Paper is a straightforward account of practical difficulties attending the development of a new type of prime-mover and as such is a very valuable guide to those engaged on work of this sort.

I very cordially agree with the Author and previous speakers in objecting to the credit of originating the ignition of the fuel by raising the temperature of the air in the cylinder, being given to a foreigner instead of to Mr. Akroyd Stuart who was the first to suggest this arrangement. It is rather late now to substitute the real name but if so, I hope the name of Diesel will be dropped and "internal-combustion" adopted.

At the beginning of his Paper and illustrations, the Author refers to different types of double-acting engines, tried and in use. In addition to obtaining greater power in a given space, a double-acting engine has other advantages. In a two-stroke double-acting internal-combustion engine, the pressure on one side of the piston is partly used in compressing the air on the other side, thus relieving the framing of the engine, the crank-shaft, connecting rod and piston rod and bearing of the load due to transmitting the power to do this work, as has to be done in single-acting engines. At the beginning of the stroke, the power required to compress the air is small and at this time the pressure on the other side of the piston is at its maximum, so that the maximum load is not materially reduced, but the average load on the bearings is lower and this should result in less wear and tear and more efficient lubrication.

The Author shows several arrangements of double-piston duplex-cylinder engines which are very interesting. He also shows in Figs 6 and 7 designs which, having no piston rods, require no piston-rod

packing. The same advantage is obtained by the opposed-piston design and in the Fullagar engine, in addition to this, there is the further advantage that the engine is in effect double-acting and eliminates several other causes of trouble. Mr. Myles

Mr. A. F. AINSLIE, *Member*: I wish to congratulate Mr. Chorlton on a most stimulating Paper. With reference to the piston ring shown on Fig. 42, some years ago a superintendent engineer put that form on each of the h.p. pistons of a twin-screw cross-channel steamer in place of the restricted expansion rings then in general favour, with this difference that it was a solid ring and not split for expansion but having many deep V grooves round it. As the pistons were deep he found the method very satisfactory, and we now recognize it as a form of labyrinth packing. Mr. Ainslie

With regard to Fig. 49, it used to be common practice in marine work to build up crank-shafts in this form. It was not considered necessary to true them up in the lathe after shrinking and they gave almost universal satisfaction. The shrinkage allowance should be determined from the elastic properties of the materials—Young's modulus, Poisson's ratio, and the safe hoop stress—together with the ratio of radial thickness of web to diameter of hole. To make the rule for all cases as proportional only to the diameter, though certainly the diameter is the most important factor, is somewhat defective. If Mr. Chorlton knows of any case where slipping has taken place, it would be interesting to learn what the materials and proportions were, how the shaft had been manufactured, and what the torque had been.

Mr. H. G. KIMBER, *Associate Member*: Regarding the crosshead-end combustion chamber as obtained with Duplex-type cylinders and its suitability for a high-compression oil engine, it would be of interest to hear how the Author proposes to inject the fuel into such a combustion chamber. The clearance volume on a Diesel-engine cylinder is, say, 1/13th or 7·7 per cent. of the piston displacement. If we assume that the injection period occupies 8 per cent. of the stroke, we see that the clearance volume will be more than doubled at the end of injection, the increased volume being created, of course, by receding pistons. Mr. Kimber

Figures 4 and 5 show one fuel valve placed centrally: such an arrangement does not appear satisfactory unless combustion were completed before the pistons had moved materially from the end position. During injection there would be dead zones behind the piston rods and around the circumference of the piston crowns.

Aluminium-alloy pistons.—The Author mentions that alloy uncooled pistons of over 20-inches diameter are being used successfully. It would be interesting to hear something of the conditions under which such pistons operate. Are they working as Diesel-engine pistons? Are they of the cross-head type or of the trunk piston type? Twenty-four-inch-diameter aluminium-alloy pistons, uncooled and of the cross-head type,

Mr. Kimber

are proposed. Such pistons would represent a considerable advance on present practice, and would be a likely source of trouble.

Aluminium-alloy pistons of 20-inches diameter, uncooled, would, no doubt, work satisfactorily as trunk-type pistons, but I should not trust them as cross-head-type pistons. The reason for the difference lies in the fact that the trunk-type piston is in close contact with the cylinder wall because it has to take the connecting-rod side thrust and is also usually operated with a smaller diametrical clearance. Consequently it is better able to give up its heat to the cylinder wall.

I should like to ask the Author what is the largest uncooled aluminium-alloy piston of the cross-head type that has run satisfactorily on an indicated mean pressure of, say, 95 lb. per square inch?

Piston rings.—The various piston-ring joints are interesting, notably the one of the limit type, although in practice I should expect the stop surfaces to wear rapidly in the case of an oil engine and thus destroy the object of the device.

Inertia of piston rings and wear resulting therefrom.—In the case of Diesel engines with cast-iron pistons there is practically no side wear due to inertia, but with aluminium-alloy pistons the side wear becomes a more serious matter. I should not expect channel-section rings to be very efficient until they had filled themselves at the back with carbon The usual practice is to keep the clearance behind the ring to a minimum.

With regard to the pressure exerted by piston rings, a lower pressure seems to suffice with trunk pistons than with crosshead-type pistons of the same cylinder bore, probably because lubrication is more efficient on the former type.

Regarding the difference in size of section of piston rings for gas and oil engines as shown in Fig. 41, the Author has explained that the increased size of the gas-engine ring is due to its having to pass over ports in the cylinder without breakage, but there seems to be very little information available regarding the most efficient width of piston ring to use.

Piston-rod glands.—Have aluminium-alloy rings been tried for piston-rod glands?

Mr. Darling

Mr. C. S. DARLING, *Associate Member*: The Paper deals with the qualitative side of construction and is one which should be useful to those making divergences from orthodox oil-engine practice parallel with those which Mr. Chorlton so successfully carried out.

As the Author states the demand for more power per unit mass of material is becoming more pressing daily and is forcing us to adopt the two-stroke double-acting engine. The two-stroke engine is more favourable than the four-stroke to double-acting working, in spite of the defects mentioned by the Author. For example a double-acting four-stroke engine would not decrease the cyclical irregularity of turning moment as much as would a double-acting two-stroke, and in order to gain equal t.m. curves it would be necessary to put in two-pistons per crank in tandem so that the idle strokes would have no effect upon the cross-head

forces. As a matter of fact, horizontal double-acting four-stroke engines have a large mass of reciprocating material which, together with the heavy flywheels, bring the t.m. to something approaching a working curve. However the Paper not being a defence of the two-stroke principle this point will not be laboured here, as much has yet to be accomplished by the advocates of both systems. Mr. Darling

Coming to the gas engine as viewed from the heavy-oil engine side, one essential difference is at once apparent, being due to the physical and chemical characteristics of the comparative fuels, *i.e.*, the compression pressures, ratios of fuel to air; this difference is the variation between the clearance volume of the engines, which, as shown in the Duplex engine is much larger than in any high-compression engine. The effect of this is, of course, to increase the inefficiency of scavenging since more air has to be supplied for a given piston displacement, but more particularly than this it increases the cooling surface exposed to the explosion pressure and so increases the jacket losses. The heat flow and heat stresses are also increased. The large or comparatively large surfaces exposed to heat flow and pressure at the time of explosion must cause high stresses in the areas which are far from supporting ribs and walls.

The writer would be interested to know whether the cover shown in Fig. 25 was designed with a view to causing a vortex ring and stratification in the entering charge, and, if so, to what extent the arrangement was successful. The disadvantage of a coned head with a deflecting piece like that shown was that the scavenging charge travelled with greater velocity at the centre than at the portion next the cylinder walls resulting in a cone formation, whose peak on striking the piston was diverted and short-circuited itself out through the ports without removing all the exhaust products, some of which were left trapped in between the outgoing and central scavenging flux. In Diesel or heavy-oil engine working, stratification was not to any advantage as regards either economy or flexibility, but turbulence might play a great part, if the path of the injected fuel was through the concentrated mass of compressed air and in the hottest zone of this air.

The writer has utilized a cylinder head of approximately this form in his design of cylinder for a double-acting direct-coupled internal-combustion locomotive, the features being port exhaust, valve scavenging, twin piston rods, conical combustion space with central valve to damp out peak velocity of charge. The use of twin rods obviates the interference of the rod with combustion, the loss of power from this cause being considerable since immediately combustion has begun the piston's movement causes exposure of cooling area to take place more rapidly than at the top end, these areas being composed of lower temperature surfaces which retard combustion; also the rod's and piston's speed are less at the inner end than at the top or front end for a given angular displacement from the centre and the time of exposal to the cooling surface is thus greater.

Mr. Darling

Twin piston rods permit of a simple arrangement since water can be led up one and down the other and a floating joint arranged with the piston to prevent differential expansion distorting it.

It would be of interest to have the Author's idea as to the efficiency of tank cooling which he mentions. Was the cooling water guided in its path through the tank? Did any natural thermo-syphon action threaten to take charge of the flow? Why have the engines shown in Fig. 10 and Fig. 23 incorporated the semi-cylindrical cast jackets with their comparative small water spaces? A somewhat similar idea was proposed to the writer some years ago with respect to air compressors for Diesel engines, and he believes it was successfully adopted, in conjunction with safety diaphragms in the jacket walls. In the case of the air compressors, all inter-cylinder piping intercoolers, etc., were enclosed in the one cast-iron tank and warning of a leaky joint or coil was given by the diaphragm. For adjustment the water was run off and the tank lifted clear.

Pistons.—High-conductivity-alloy pistons, both of the Admiralty composition, "Y" metal and Electron metal, but particularly the second, have certainly given good results in Naval service up to diameters of 20 inches, but their use could not always be looked upon with favour as it depended greatly upon circumstances. One great point in their favour was that of doing away with the necessity for artificial cooling provided always that there was some heat sink behind the piston at the sides or back to absorb the heat transmitted by this piston; another was that of weight reduction which was very profitable inasmuch as inertia reduction was identical with reduction of bearing losses and increase in mechanical efficiency up to a point since beyond a certain point we lost the reciprocating equivalent of the fly-wheel and want of inertia may at times be worse than too much. On the other hand, the scantlings of an aluminium-alloy member must be larger and this may mean increase of weight elsewhere again counteracting the advantage. High-conductivity materials have been used for connecting rods in high-speed engines, and have solved the problem of relieving the big end of its heat and load, thus taking away one of the limiting features of present-day high-speed aero engines as regards output and speed.

Perhaps some day we shall see connecting rods, piston rods, etc., of locomotives and 3,000 h.p. Diesel engines of aluminium-silicon alloy, but until it becomes a product of the local founder its use will, in the writer's opinion, be very limited.

Lubrication.—It gives the writer much pleasure to find Mr. Chorlton in agreement with what he has regarded as perhaps a fad of his own, namely, the use of separate pumps for each feed to cylinder barrel multiple feeds with check valves. His reason for its advocacy is this: assume a multiple-feed ring supplying oil to the cylinder barrel from one pump and that owing to cross-head thrust in a trunk engine or deflected piston rod in a cross-head engine, the piston is deflected from its concentric position and the clearance at one side is reduced to a

minimum and that at the other side increased to a maximum, then lubrication takes place at that side which does not require it. Further, any lubricant that did exist on the one side is displaced to the wasteful side, and finally no oil can reach the point of minimum clearance since the pressure is too high and the resistances are decreased on the other side. Thus, it is essential to safe running to have independent pumps of reliable type. Also, it is advisable not to make the piston circular but to turn it to a diameter of say $1\frac{1}{2}$ to 3 thousandths less than the cylinder barrel and carefully to grind off the side subject to thrust to a radius equal to that of the cylinder bore. This provides for more than line contact on that portion subject to thrust. Mr. Darling

The use of check valves in the lubricating oil line is a good point and should be extended to fuel, air injection and other lines, since the passage of heated air along a pipe line is detrimental to the function of the pipe, if not absolutely dangerous as it is in certain cases.

Ignition.—As one who has for the time being gone from constant-pressure engines to constant-volume cycle, the system of ignition used in the Author's engine is interesting; it would be helpful to the writer if he could obtain information as to the type, *i.e.*, high or low tension. If high tension was used how many plugs per cylinder head, and what type of plug? If low tension make-and-break was used what size of igniter and how many of these were used? In a recent description of a coke-oven-gas engine, the writer noticed that four make-and-break igniters were used at each end. Would this be better than a single or dual-plug ignition? When the magnetos were used was the deterioration of the magnetism rapid? The writer has had such trouble with magnetos that coil ignition of a modern type has been resorted to with benefit, both on cylinders of 5-inch and 10-inch bore. Make-and-break ignition certainly gives a bigger, more illuminating spark, but its incendivity is not to the writer's knowledge as great as a small "hot" spark from a high-tension "jump" spark. The great trouble of large multi-cylinder engines, with multiple igniters per cylinder, is the synchronism of the spark, which may be perfect at the beginning, but due to unequal wear causes much trouble, loss of power, incomplete combustion, high exhaust temperatures and other defects indirectly or directly traceable to faulty ignition. Ignition is the crux of the constant-volume engine problem, whilst injection is the crux of the constant-pressure engine problem.

Waste-heat Boilers.—The use of waste-heat boilers is always justifiable in high powers, whether gas or heavy-oil engines are used, and even if the Still system is not used it will be commercially profitable in many cases. The vertical cross-tube boiler shown with the Cochran fire-box is suitable for confined space, but the writer prefers a horizontal multi-tubular boiler with multiple-tube plates the cold feed passing at the bottom of the end nearest the gas outlet and gradually finding its way through perforations in the tube plates to the hot end of the boiler, steam only being generated and collected at this the hottest end. The Contra-

Mr. Darling

flow principle must be followed if any gain is to be anticipated from a gas boiler. Although higher figures have been claimed, an equivalent evaporation of from 1·5 to 1·8 lb. per b.h.p. is very good and cannot be much exceeded. A point in the installation of waste-heat boilers is that auxiliary silencers should be fitted so that the uncooled gases can be silenced in case it becomes necessary to take the boilers out of the system for repairs.

Mr. Arthur

MR. J. C. ARTHUR, *Chairman of the Graduate Section*: Having regard to the very much smaller clearance volume required by the heavy-oil engine as compared with the gas engine, and to the differences in the nature of the combustion in the two engines, the Duplex design of cylinder employed by Mr. Chorlton for the latter does not appear to be altogether suitable for the former. Even assuming a low ratio of compression it would probably be difficult to obtain a form of combustion space which would allow of sufficiently good distribution of the fuel to secure good consumption. In the case of the double-acting two-stroke of the ordinary type with opposed-port scavenging, the scavenge, as well as the exhaust ports would probably have to be duplicated in order to obtain the inclination necessary for good scavenging.

It is questionable whether aluminium pistons of as large a diameter as 24 inches would stand up to their work in double-acting engines without some form of liquid cooling. The resistance to heat transference from the piston crown to the cylinder wall is twofold—that due to the imperfect conductivity of the metal and that of the "joint" between the piston and the liner. With aluminium pistons it should not be difficult to provide an ample section to render the first resistance sufficiently low but a distinct limitation is imposed by the second since the amount of heat that can be transmitted by the piston at a given temperature, will vary as D^2 (where D is the cylinder diameter) while the amount absorbed by the piston crown will vary as D^3. An air-cooled piston in a double-acting two-stroke engine will be operating under conditions about twice as arduous as will a similar piston in a two-stroke single-acting engine since the area of the rubbing surface is approximately the same in the two cases, though the transmissive efficiency will be higher in the case of the double-acting engine owing to the greater heat transfer from the upper as compared with the lower part of the piston skirt, while the amount of heat to be transferred is about twice as great in the former as in the latter. The single-acting air-cooled piston is also assisted to some extent by a certain amount of internal air circulation. In oil engines of relatively small size, running with air-cooled cast-iron pistons, a certain amount of trouble may be experienced due to burning of the pistons where they are struck by the injected oil spray. Aluminium might or might not be superior to cast-iron in this respect. It would be interesting to hear if air-cooled aluminium pistons of 20 inches diameter or over have been used in single-acting two-stroke oil engines. Mr. Chorlton states that the cast-

iron rings employed in piston-rod packings became too hard and caused wear on the rods; would it not have been possible to overcome this difficulty by fitting the rod with a cast-iron liner which would wear as "hard" as the rings and which would be easily replaceable if badly worn? Mr. Arthur

Timing of lubricators seems especially desirable with double-acting engines. At present no lubricator on the market will meet the requirements of the case. In order to supply oil to various points on the cylinder with a separate pump for each point, the pump unit employed must be capable of consistently delivering a very small quantity of oil. A pump having a differential plunger running at $\frac{1}{2}$ to $\frac{1}{4}$ engine revolution must therefore be used, the speed reduction being necessary in the case of the smaller sizes of engine to prevent the amount of oil delivered per stroke being so small as to cause the working of the pump to be unreliable. Several types of lubricator are available which will meet requirements as stated so far, but none will achieve what is required of a timed lubricator under the given conditions; *e.g.*, they will not, when geared to run at a reduced speed relative to the engine, make a full delivery stroke in less than 60 degrees of engine revolution. It seems possible, however, that a lubricator with a differential plunger positively operated by a cam driven from the engine on the delivery stroke and by a spring on the suction stroke would meet all requirements; with engines running at moderate speeds time lag in the pipes should not cause any trouble. Such a lubricator, when arranged to inject oil between the piston rings would seem to be ideal for a double-acting engine. It would be interesting to hear if Mr. Chorlton has tried such a device and if so, with what success.

Mr. J. CALDERWOOD, *Graduate*: The question of the exhaust-gas boiler raised by Mr. Chorlton is one of great interest, as, if oil-engine auxiliaries can be driven by steam produced in this way, a considerable saving of fuel will be obtained. The figure he gives of 1·6 lb. of steam per b.h.p. per hour could only be obtained with the use of a Contraflow-type of boiler and feed heater, and it would be of great interest if Mr. Chorlton could give us more information of the design of this type of boiler. Mr. Calderwood

In spite of the low gas temperatures available in the exhaust boiler, it would not appear that a much greater heating surface is needed than in other boilers, as higher gas velocities could be adopted. Taking values of heat transference based on data given by Mr. Gibb in his recent Paper before the Institution, and correcting these to the gas velocity that could be used, a value for the evaporation of 4 lb. per hour per square foot of heating surface is obtained. It would be interesting to learn whether this figure agrees with the results obtained from the exhaust-gas boilers with which Mr. Chorlton has had experience.

If electric auxiliaries with turbo-generators were used, about 40 h.p. per 1,000 h.p. of the main engines could be obtained from the

Mr. Calderwood exhaust gases, and this should be sufficient to drive all auxiliaries that need to be run when a ship is at sea.

Was any trouble experienced in double-acting engines with uncooled pistons, due to excessive heating of the part of the piston rod exposed to the combustion temperature? Such trouble, if it exists, would be worse in a Diesel engine due to the long period of combustion and consequently greater length of piston rod subjected to the highest temperature attained by the gases.

AUTHOR'S REPLY.

The Author THE AUTHOR (Mr. A. E. L. Chorlton, C.B.E.): It has been a great pleasure to me to listen to the discussion. There is one thing in which the reader of a paper has the advantage, and that is because, as he is the last to speak, the other speakers cannot hit him back. Generally, I have not been on large gas engines for some nine or ten years, my later work having been all on oil engines. This earlier experience has been a great help and I hope the Paper has proved this, and that it will be equally useful to others. I would also say this: I have no particular "fish to fry": that is, I am not advocating any special type of engine in any way; so that all the remarks I have had to make should be taken as my personal opinions, or as experience presented from an oil-engine point of view.

Reply to the President.—I very much appreciate the President's remarks, and only wish I had been able to try the converted engine on oil instead of gas; at the time, however, the oil-engine experiment could not be tried. After running gas engines personally for years, and subsequently having run engines using oil, I know which is the easier to run; the engine working with a compression of over 300 lb., that is, the solid-injection type, is the easiest engine to run, and a gas engine far the worst. The marine people do not at all realize this, but I assure them that it is a fact. I am also glad to note the interest the President has taken in my remarks with reference to the origination of the British type of oil engine, now familiarly known as the Direct-Injection type. This engine has very frequently worked at compression pressures but little above 300 lb. and is really quite distinct from the Diesel.

Reply to Eng.-Commdr. Hawkes.—I always listen to Commander Hawkes with the idea of picking something up, and I have never been disappointed. We have worked together and agreed on many questions; whenever we differ, we usually keep our dispute for some other place. Generally, I have been much interested in all that Commander Hawkes had said, perhaps the more so because, as has so frequently happened in the past, I am in complete agreement with much of it. With regard to his point about the two rods, I think he was quite right. The two rods in one piston probably gave a better combustion space, and a

higher mean pressure can be worked to. Commander Hawkes's opinion as to the limitations of the double-acting engine is interesting, and I agree that the whole problem is based upon the need for a slow-running large-powered engine, and here, I think, we are bound ultimately to adopt double acting. In Commander Hawkes's further remarks, *re* liners, I note these were all in keeping with my own experience, including those with water-cooled bars between the ports. His remarks *re* the effects of scavenging are most interesting, and also those in which he describes his own experience in the use of serrated piston-rod packing rings. The Author

With reference to cylinder lubrication and Commander Hawkes's enquiry as to timing the feed, for long I tried to do this, unsuccessfully; ultimately, I found it was not necessary, and a lubricator was used running at engine speed with a feed to each point, and efficient non-return valves at the cylinder entrance. In this method the cylinder oil feed seemed to be quite efficient, and the consumptions of engines running to-day are quite as low, if not lower, than the best Diesel. The oil entrance holes were all exposed to the combustion. With regard to Commander Hawkes's other point, it is certainly necessary to line up hot.

Reply to Mr. Bruce.—I have read with interest Mr. Bruce's remarks *re* cracking of cylinder covers, and their remaining in service even after a fracture.

The question of the type of port to be adopted in the valveless engine is, of course, a constantly recurring one, but it is not correct to say that the exhaust port is subject to clogging from carbonized oil, with the latest experience of the engines described. The cooling of the ports and the proper lubrication of the engine ensure absolutely clear ports.

Mr. Bruce's remark on the special double-acting engine described in Fig. 6, is interesting. No doubt the governing factor is the mean pressure to be used, and as he does not advocate a high one, this type could be readily adopted. It is not necessary to have the large transfer passages indicated. Such engines would, of course, have very long strokes, and the combustion space resulting is adequate. I do not suggest the engine working on the Diesel cycle, but rather with compression pressure of about 300 lb. (solid injection) with the first part of the diagram more or less on the constant-volume cycle.

Mr. Bruce's remarks on piston rings are very interesting, particularly where he has reduced the wall pressure to 2 lb. per square inch, as his experience agrees with mine. His further remarks with reference to the carbonizing up of the top ring should be taken in conjunction with the type of lubricator adopted in single-acting engines. In my experience with the lubrication that I have described for double acting, we got over the trouble with the top ring.

The Author

With reference to the built-up crank-shaft, and Mr. Bruce's remarks on dowel pins, I do not agree with the dowel-pin policy; it is an admission, in my opinion, that the shrinking principle is not to be trusted, and, therefore, one must fortify it with something else: if the shrinking is sufficient, no dowel pins are needed. The insistence upon them is rather like suggesting that there might be some risk of bad workmanship or incorrect shrinkage allowance occurring.

Reply to Mr. Swinnerton-Dyer.—Although Mr. Swinnerton-Dyer's remarks may have an important bearing in principle, in actual practice I do not think that they need be taken so seriously. The combustion space available with the Duplex type is sufficient for the mean pressure for long and reliable running, experience having decided that this should not be exceeded. Of course, very high mean pressures could not be obtained.

I do not follow why Mr. Swinnerton-Dyer stretches the heat losses in the way that he does, and wish he had divided it up in a way more usual with thermo-dynamic work. There is no reason for the figure that he gives of a greater fuel consumption than the single-acting engine: the opposite would be the case, that is to say—less. The point to be observed, however, is that the Paper does not actually advocate double acting; the Author presented his working experience to assist, if possible, in the design of such engines, believing that natural causes will bring them about, in spite of all the single-acting disputants.

I regret that I cannot check Mr. Swinnerton-Dyer's figures of relative weights of the engines he mentions, but the slow-speed double-acting engine comes out at about 150 lb. per b.h.p. Referring to his further remarks, he may have overlooked the fact that the proper proportion of the ports and exhaust angle brings in a much shorter piston for the double-acting engine than for the single-acting one.

Sizes of crank-shafts in almost all engines are governed largely by bearing areas.

I cannot understand on what basis Mr. Swinnerton-Dyer's costs have been taken; that is to say, the cost rate per ton for one compared with the other, as regards labour, etc.

As has been pointed out to another member, years ago all large slow-running gas engines were single acting: they are non-existent to-day.

Reply to Mr. Harry Hunter.—It is certainly difficult to explain why a piston should be easier to pack than a piston rod. This undoubtedly seems to be the case, because packings have been introduced on to the rods in the form of a piston; that is to say, there were rings on the rod that moved with it, as against the fixed type, and it might be said that the results obtained were better. With reference to Mr. Hunter's remark with regard to reduction of the pressure at the crosshead end,

one would say that this is not usually done, unless the packing is giving trouble and blowing; with a tight packing it is not necessary. The Author

Cylinder liners are usually bored in position after being pushed in. On the other hand, with the accuracy of work now done, it does not seem that this will always be essential. There is no burning on the joint in the liners, and the clearance is about $\frac{1}{8}$ of an inch.

So far as regards leaky pistons, that is to say, gas passing from one side to the other, this is more dangerous with a gas engine than a Diesel—contrary to what Mr. Hunter says—the reason being that it may possibly start an ignition on the other side during the compression stroke. The temperature of the water discharge from the piston would indicate this abnormal running.

A greater mechanical efficiency will be obtained for a double-acting than a single-acting one, but figures for an oil engine are not available.

Mr. Hunter's remarks *re* auxiliaries, are interesting. He does not, however, deal with first cost. This seems, undeniably, an extremely important consideration, particularly at the present time. Other methods than the standard ones for oil-engine auxiliaries must be considered—either steam starting and deck gear or mechanical driving of the deck gear.

Mr. Hunter's remarks *re* the engine exhaust and the galley is amusing. There is only one way of usefully abstracting exhaust heat, and that is right against the cylinder itself. The instance given, I think, was from an installation which Mr. Kloos described to me some while ago.

Reply to Eng. Vice-Admiral R. B. Dixon.—I feel how much to the point are the remarks of Admiral Dixon, and therefore, will endeavour to reply to them in some detail.

The special conditions attached to oil engines for naval use render high rotational speed practically a necessity and owing to the restriction of weight make the use of light alloys for the beds, cylinder covers, etc., almost equally so.

My experience with the double-acting two-stroke was with speeds up to about 400 r.p.m. With these speeds scavenging was less efficient and consequently, lower mean pressures resulted. Additional work on this type of engine is necessary if it is to have any future for this class of work. The increased port areas which will be required together with the ample scavenge supply, will bring down the mechanical efficiency; the Duplex type may suit this work. The alternative to this type is the supercharged four-stroke cycle engine, and it is really interesting to examine the claims of the two for the special purpose of Admiral Dixon.

So far, the experience with aircraft engines indicates that higher powers can be developed on the weight basis with a single-acting four-stroke than with the two-stroke. In both cycles the double-acting type is, of course, higher, requiring more head room. In such a comparison

The Author

it may be taken that a supercharged four-stroke can give 1,000 h.p. per crank at 400 r.p.m. with a weight of 25 lb. per b.h.p. (alloy bed and crankcase).

With further reference to mechanical efficiency, while it is undoubtedly less with the two-stroke cycle if a large scavenge is used, one must point out that this does not affect the aspect of durability, because as is remarked, the lost power goes in the excess of air to be supplied. In accessibility and simplicity the two-stroke is undoubtedly a good type, whether duplex or single cylinder.

I note with interest the experimental engine under construction and quite expect with the increased experience now available, that successful running will be achieved.

The Admiral's further description of a type of piston with double rods and their functions is most interesting, and illustrates how carefully the problem has been attacked. I doubt, however, whether Admiral Dixon does really appreciate the difficulties of the metallic packing, judging by his remarks.

I note his agreement with the views expressed in the Paper as regards cooling the cylinder and the exhaust port bars.

With reference to Admiral Dixon's disagreement with my views regarding the uncooled 24-inch diameter piston, I would point out that I did not suggest that this would work with a high mean pressure. The Duplex engine mentioned gives 1,000 h.p. which means 250 for each side of each piston. As General Bagnall-Wild points out over 200 h.p. has been successfully carried by a 12-inch piston; by another piston still larger, which Admiral Dixon is aware of, over 200 has been carried, the temperature of the piston being so low that no oil was carbonized on its face. I think, therefore, that instead of being on the high side I was on the low side in suggesting that 24-inch diameter could be used uncooled.

The argument with reference to the rings is a good one, but on the other hand, curiously enough, it does not seem to apply in practice. Still, "Y" metal rings seem to be quite a good proposition. Hence I entirely retain the opinion that I expressed on these pistons. I would, however, say that the design that I showed is not a trunk piston but two separate pistons, and that they are forgings.

The piston rod material that Admiral Dixon is using seems good; in fact, although oil-hardened rods have been used, it is doubtful whether the extra cost was justified. On the whole, however, to minimize trouble with packing, I would prefer for quick running engines to consider the use of such rods.

With reference to the question of lubrication, I have not found it necessary to avoid the uncovered oil hole suggested, getting quite satisfactory results with the designs shown, a suitable back pressure valve and lubricator being fitted.

Reply to Brig.-General Bagnall-Wild.—I have read the communication from General Bagnall-Wild with very great interest. It is quite true that the Paper as presented was historical but the whole of it bore direct relation to what is now being developed for double-acting oil engines. I entirely agree with General Bagnall-Wild that for the internal-combustion engine the question of the materials used is of paramount importance, and that these in the future will be high tensile steel and light alloys, although I would prefer, particularly on thinking of the larger engines, to refer to the light alloy more particularly as the alloy of high conductivity, for it is with materials having this property that we shall replace some of those at present in use, in the parts where heat transfer is high. The Author

With reference to his remarks *re* the use of oil, what he suggests is correct, namely, that the method in force when the cheaper oil was used was a superior one to that in use for the dearer oil. In the large oil engine, the main difficulty is not so much the acid in the oil as the carbonizing up at the oil entrance which often takes place.

My direct concern with the production of an engine using heavy oil for aircraft, enables me to agree entirely with the views expressed by General Bagnall-Wild. The illustration and description of the special four-stroke double-acting engine (Fig. 55) reveals an engine that must have in its experimental trials afforded extremely interesting work and probably valuable data. Without a more detailed description of it, I do not think I can comment further. The tandem arrangement was constructed for large powers by the M.A.N. (four-stroke Diesel type).

Reply to Mr. Anderton-Brown.—I observe the comments of Mr. Anderton-Brown, with more than usual interest for the reason that Mr. Anderton-Brown was associated with me through many years, in which the two-stroke cycle double-acting engine was being developed. I am very glad of his remarks as they so frequently explain in further detail the experiences made at the time. I note with interest his remarks on the sleeve valve and pump arrangements, and certainly think that this design was a much better one than critics were at first inclined to admit. It certainly would be very suitable for oil-engine work where the question of tarry gas does not arise.

With reference to his remarks on pistons, originally seven piston rings were used at each end. The number was eventually reduced to four by the Author and this was found to be quite sufficient.

Mr. Anderton-Brown's concluding remarks on pre-ignition are very important. There is, in the opinion of the Author, from his own personal experience, no doubt that the large gas engine, due to this liability coupled with that of dirt in the gas, is a much more difficult engine to run successfully than the large oil engine.

Reply to Mr. Belyavin.—I fail to follow Mr. Belyavin's belief in any figures that indicate that a single-acting engine is lighter than a double-

The Author

acting engine for slow speeds of rotation. It is for the reason of such speeds that I consider the double-acting engine will have to be adopted. It is extremely dangerous to take up the attitude of Mr. Belyavin in saying that there is a great difference between a marine Diesel engine and a stationary gas engine. This attitude invariably ends in a lot of very useful information and experience being disregarded. There is no more difference between a marine Diesel engine and a stationary gas engine than a trained engineer not tied to either school can make allowance for. The Author has been ten years on oil engines and ten years on gas engines and so speaks from some experience. Originally all large low-speed gas engines were single acting, whilst to-day they are practically not to be found.

Mr Belyavin then goes into more detail as to his reasons for maintaining his view of the single-acting type. I confess I cannot follow him in this argument.

Though the Paper is on the subject of two-stroke cycle engines for the reason of its Author's experience of that type, I do not agree with Mr. Belyavin when he compares the two-stroke with the four-stroke.

The four-stroke engine is a very good type, probably more economical in fuel, and I cannot think that this type which has existed so long will drop out. On the other hand, I am inclined to the belief that the two-stroke, double-acting, oil engine will ultimately prove cheaper in first cost than the four. Furthermore, being valveless, it has another attraction.

Reply to Mr. Chaloner.—The remarks of Mr. Chaloner are interesting. He writes that the Author has indicated that the double-acting engine described was not good enough for practical daily service. This is not the deduction that the Author intended should be drawn from his Paper: the engine was very much so. What the Author was concerned in was to shew by what evolution of parts this had been brought about. The reason that dirty gas was allowed to affect so much the engine's running was not a question of cost, but first, failure to realize its very deleterious action and second the difficulty in really cleaning a producer gas containing tar—a difficult problem, at any rate, in the early days. Again as to the comparison of cycles, it is nowhere laid down in the Paper that a two-stroke cycle is any better than a four-stroke; actually, more double-acting four-stroke engines existed than two-stroke.

With regard to size, shape and area of ports, this falls under the section of fluid factors, requiring a paper in itself, which the Author might present at some future date.

The 7,000 h.p. engine has two pairs of "Duplex" cylinders the bore and stroke of which are 1,000 mm. and 1,400 mm. respectively; the normal speed is 94 r.p.m.

I have no records of gas consumption. I do not follow Mr Chaloner in his remarks about 300 lb. compression; the cost of an engine depends

largely on its maximum pressure, which may be over three times this and therefore for 300 lb. it must be much less than 480 or 500. A compression pressure of 300 lb. will burn most, if not all, the oil I have come across. The Author

High-conductivity alloys of high strength may be used for cylinder covers as well as pistons. Their use for bridges, due to the specific gravity being one-third that of steel, has been only lately drawn attention to by Dr. Maw in his Presidential Address to the Institution of Civil Engineers.

Reply to Mr. Darling.—I note Mr. Darling's view regarding the two-stroke double-acting engine, but as pointed out in the Paper, I took up no position of preference or of priority between this and the four-stroke.

I do not quite understand Mr. Darling's reference to the differences of fuel and where he goes on to speak of the variation in clearance volume etc. The clearance volume is that set up by the compression pressure used, and will, therefore, be the same for all types using the same compression. The heat flow is not altogether dependent in the way that he describes, or a small engine would be much less economical than a large one, whereas it is not, sometimes even the other way.

Mr. Darling's reference to surfaces as being supported by ribs, would rather indicate an unusual experience in this way.

The scavenging with the special head shown and as described at the Meeting was very good; an actual test proving this was made but not mentioned in the Paper. All such vortex-type heads were tried with smoke. The stratification in two-stroke engines is indicative of good scavenging. The engine that Mr. Darling describes seems a remarkably interesting one, and one must look forward to the publication of his experiences with it.

The cooling and piston-speed questions mentioned as heat losses by Mr. Darling, are stretched altogether out of proportion to their value. The tank cooling for the Duplex type engine, needed no internal pipes. These, however, were used in the other type to direct the flow.

With reference to the ignition, two plugs were used per cylinder end; either a high-tension Lodge, or low tension make and break was used. Very little trouble occurred with the ignition gear in the later periods. Therefore, as the make and break was the simplest it was used most frequently. Periods of six months with one stop were run by engines without any change whatsoever.

I note Mr. Darling's views on exhaust boilers.

Reply to Mr. Archibald Rennie.—With reference to Mr. Archibald Rennie's remarks, I regret that I evidently did not make it quite clear that I was fully aware of the nature of the Still engine, though very probably not of its latest detail. In my remarks I made no reflection on the Still engine whatsoever, and am very glad that it is going ahead. What I did say was that, in the question of reduction of the cost of an

The Author

installation, the consideration of the use of superheated steam for starting direct on the oil side of the piston in place of air might be considered. I was not aware that the Still people contemplated doing this.

With reference to the exhaust boilers, my practice has been to use stage heating, using jacket-water as a feed. This might be done with the oil engine to a greater extent, when the figure stated in the Paper may be obtained. Of course, with the type of generator used, the figures obtained in the Still engine could not be attained.

I would like to assure Mr. Rennie that my remarks did not visualize the Still engine, as my experience and practice with exhaust boilers began before that engine, so far as it was known to me, existed. An illustration from a Patent taken out by the Author in 1904 may be of interest to Mr. Rennie (Fig. 56).

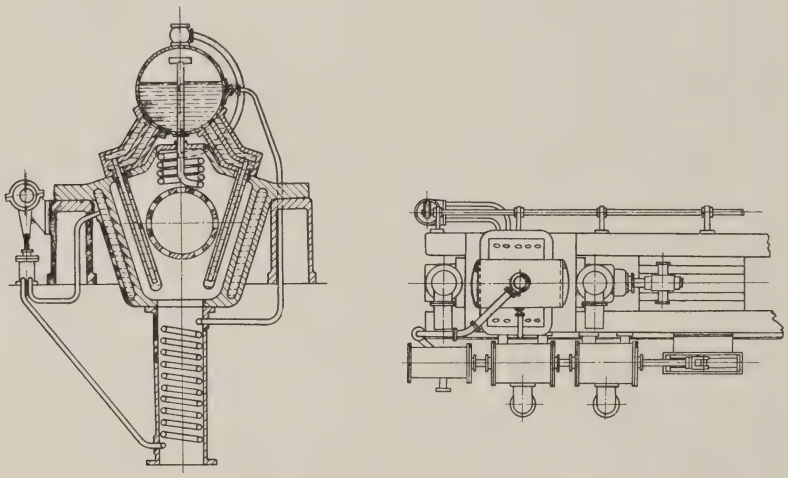


Fig. 56.

The further details which Mr. Rennie gives of the Still engine are very interesting and all to the good in the way of reduction of pressures.

Reply to Mr. Kimber.—With reference to Mr. Kimber's remarks on the clearance volume and its effect, these depend very largely on the mean pressure that the engine is to work at. I believe that low mean pressures will ultimately be preferred to obtain constant and long-period service, and with such pressures, the duplex form, with a long stroke, can be adopted. Mr. Kimber's further remarks on alloy pistons have been noted, but I can only say that from my experience the 24-inch diameter piston can be used uncooled. I have seen alloy pistons running under indicated mean pressures (130 lb.) much above that stated, though I do not suggest that so high a figure as this should be used for constant marine service.

I note Mr. Kimber's reference to piston rings, and rather agree with Mr. Kimber's views *re* the channel type filling up. I never used this

type in my own practice. I note Mr. Kimber's experiences with the pressure of rings for trunk pistons, as compared with those for the cross-head type; this should not be so, but it appears that the rings do not pack completely by themselves in either case. The Author

With reference to piston-rod glands. I have no experience of aluminium-alloy rings being used; usually it was cast-iron or phosphor bronze.

Reply to Mr. Arthur.—Despite Mr. Arthur's remarks with reference to the 24-inch alloy piston, I still hold the opinion that this size could be effectively run if the mean pressures were not too high. I have, in my own personal experience, obtained 250 h.p. from one side of the piston, and the pistons had actually run much cooler than was expected. I would point out to Mr. Arthur that the suggested double-acting piston was really two pistons separated from each other each giving 250 h.p. So far, I understand, 20-inch pistons are the largest alloy pistons in use at the present time.

With reference to the suggestion of a cast-iron liner on the piston rod, I think this has been tried, but cannot at the moment lay my hands on the record. The result was fairly successful; the difficulty was the very large rod due to the steel core or tension member inside.

I do not agree with Mr. Arthur's remarks *re* lubrication. I would say bluntly that it indicates to me a want of experience with large engines in this respect. It is not necessary to time the oil supply. I tried to do this for a very long period but ultimately gave it up. I was fully successful with the feed delivered in small quantities from a lubricator running at engine speed.

Reply to Mr. Calderwood.—The essential feature of an exhaust boiler is to be right against the engine cylinder itself. There are no such boilers in use that I am aware of. The types that are in use are such as were described at the meeting; there is also the locomotive type. It is obvious that with the limited amount of heat, contra-flow or series heating must be used, and because of the nature of the dispositions of the heating surfaces, forced circulation may have to be adopted. Steam auxiliaries and steam starting are attractive, but more work requires to be done on this method before we can definitely state that it will do all that is required of it.

With regard to the piston heating with uncooled alloy pistons, no overheating occurs, as it has done with cast-iron. It is a mistake to think it would be worse in a Diesel engine. In my experience, oil engines are easier to run than gas engines.

I do not understand the statement with reference to time of combustion. The most economical engines have usually the shortest periods of combustion, and the high-compression type is the most economical.

Reply to Major-General Sir W. Sefton Brancker.—The Author has

The Author read the letter from Sir W. Sefton Brancker with great interest but does not think he can usefully comment on it from the slow-speed-engine point of view.

Reply to Mr. Ainslie.—With regard to Mr. Ainslie's remark *re* the piston rings, his piece of history is certainly very interesting. His further remarks with reference to built up shafts, are correct in the sense of the method adopted in designing such shafts. The difference of opinion is usually between the shaft with dowel pieces, and the one without, and this controversy seems to be an unending one.

Reply to Mr. David Myles.—I do not quite follow why Mr. Myles should suggest the change from Diesel to "internal combustion"; possibly this is a misprint for "high compression."

Doubtless the explanation which Mr. Myles gives of the action of a double-acting engine with relation to the bearings and lubrication is correct. On the other hand, for the double-acting two-stroke engine, I did not find that the bearing areas could be cut down if satisfactory running was to be obtained.

NOTES ON TORSIONAL OSCILLATIONS WITH SPECIAL REFERENCE TO MARINE REDUCTION GEARING.

A. T. THORNE, B.Sc., *Assoc. Memb.*: J. CALDERWOOD, M.Sc., *Graduate.*

[Read in Newcastle-upon-Tyne on the 17th November, 1922.]

The importance of torsional vibrations has recently been emphasized by the fact that trouble experienced in connexi n with reduction gearing on board ship has, in one or two cases, been attributed to the existence of torsional vibrations in the system, and the result has been that in some quarters the possible effects of such vibrations have been exaggerated. The authors have been engaged on an investigation of the problem during the past two years which has been directed towards obtaining confirmation or otherwise by direct observation of the natural frequencies calculated and determining if possible the magnitude and character of the vibrations actually experienced; and while the results obtained are as yet comparatively meagre it is hoped they are already of sufficient interest to justify the authors in placing them before the Institution.

The problem has been considered entirely from the point of view of marine installations involving reduction gearing, and all calculations etc., have been confined to that particular case of the general problem.

Any elastic system will, in general, have a fundamental vibration involving only one node, and higher frequencies, involving a multiplicity of nodes, will occur up to an unlimited number. Ample published information is available on the general theory of oscillations, but little of it is in a form that is readily applicable to the calculation of the natural frequencies of vibration of a complex elastic system. Eventually the authors found that the easiest method of solution of the problem was based on the following fundamental equation :—

I = the moment of inertia of a mass about the centre of the shaft to which it is attached.

$K = 2\pi \times$ frequency of vibration.

Then the maximum torque acting on the mass when it is given a vibration of two radians amplitude $= K^2I$.

C = torsional rigidity of the shaft.

Then angle of twist in the shaft $= \dfrac{K^2I}{C}$

By the progressive application of the above equation to consecutive sections of a system containing a number of masses connected by flexible shafts it is comparatively simple to obtain a formula that will give the natural period of vibration of the system as a whole. The method of

procedure is given in some detail in the appendix and a formula so derived (equation 3) is given in a form which covers most of the usual arrangements of marine reduction gearing.

If, in the application of this method, the mass of the shafts is neglected, the formula obtained is of a similar form to that given by Dr. Smith in the paper which he read before the Institution of Naval Architects last March; such a formula gives the single node frequency accurately and, if very flexible shafts are used to connect turbines and gears, will give approximate values to some of the higher frequencies, but the solution is incomplete. Any elastic system consisting of a number of masses, each concentrated at a point, connected by massless but elastic shafts, has a finite number of critical frequencies. This is not the case with a system such as we are considering, in which the mass of the shafts cannot be neglected. It is owing to this error that Dr. Smith arrives at the conclusion that he has limited the critical frequencies in the modified "Melmore Head" arrangement to two. Actually even the modified system has an infinite number of critical speeds. In general it may be said that for all frequencies which are likely to be of practical importance it is sufficient to take into consideration the moment of inertia of the mass of the propeller shaft. Only in very exceptional cases need the mass of the intermediate and high-speed shafts be considered, as, unless these are 30 feet or more in length, their mass does not materially affect the frequency of any of the types of vibration which are likely to coincide with forced vibrations in the turbine, gearing or propeller.

There has been some criticism of the assumption that a system including gearing may be regarded as an unbroken elastic system. That it is a broken system when at rest is obvious but when transmitting a torque sufficient to ensure that the gear teeth are continuously in contact such tooth contact is undoubtedly equivalent to a rigid connexion.

A graphical method of solution of vibration problems has recently been evolved by Herr Gümbel and was described by him in Zeitschrift des Vereines Deutscher Ingenieure No. II., 1922. By his method the actual amplitude at any vibration frequency is obtained without previous calculation of the critical speed and by it the effect of damping forces can easily be considered. It is not, however, more simple than direct calculation of the natural frequency in the case of a marine-turbine installation and is intended by the author to be applied chiefly to the effects of the introduction of damping devices into a system which has experienced trouble from torsional vibration.

If an elastic system is subject to the action of any periodic disturbing force, forced vibrations will be set up of a frequency equal to the frequency of the disturbing force. In the system we are considering such disturbing forces may be due to external causes, of which the most prominent is the variation in propeller resistance, or may be due to what may be called "internal causes" such as slight lack of balance in any of the rotating masses of the system. When a forced vibration is impressed on the system the character of the vibration set up

will depend upon the point in the system at which the disturbing force is applied. Nodes will occur in positions such that the natural frequencies of the free ends of the system about those nodes (each free end being regarded as a fixed free system fixed at the node) are equal to the frequency of the impressed forced vibration. If the disturbing force is due to either of the causes mentioned above, this frequency will be directly proportional to the r.p.m. of the propeller shaft and, consequently, as the r.p.m. rises the frequency of the forced vibration will increase. As this frequency increases the nodes will move outwards towards the free ends of the system and additional intermediate nodes will occur.

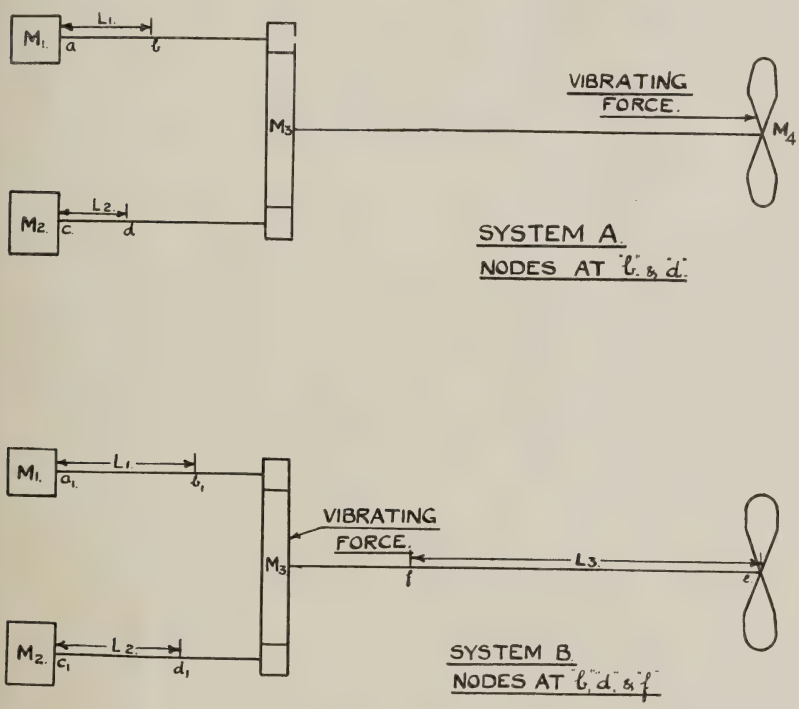


FIG. 1.

The whole problem is rendered more clear if the manner in which the system vibrates can be actually visualized.

In the diagram Fig. 1, two systems are shown: A, which is subject to a forced vibration due to the uneven resisting moment of the propeller, and B, which is subject to a forced vibration due to lack of balance or periodic inequalities of the teeth in the gearing. Nodes are indicated at *b*, *d* and *f*. In A the lengths L_1 and L_2 are such that the natural frequency of the masses M_1 and M_2 and the shafts *ab cd* about *b* and *d* are equal to the frequency of the forced vibration. In B the lengths L_1 L_2 L_3 are similarly determined.

The amplitude of the forced vibration produced by any disturbing force in a given system depends primarily on the magnitude of the

disturbing force and the ratio which the frequency of this disturbing force bears to the natural frequencies of the system. As the frequency of the disturbing force approaches a natural frequency of the system the phenomenon of resonance occurs, resulting in an increase of the amplitude of the vibration. When the two frequencies coincide the amplitude of vibration would become infinite but for the damping influences operating, such as the internal friction of the shaft, etc. The damping

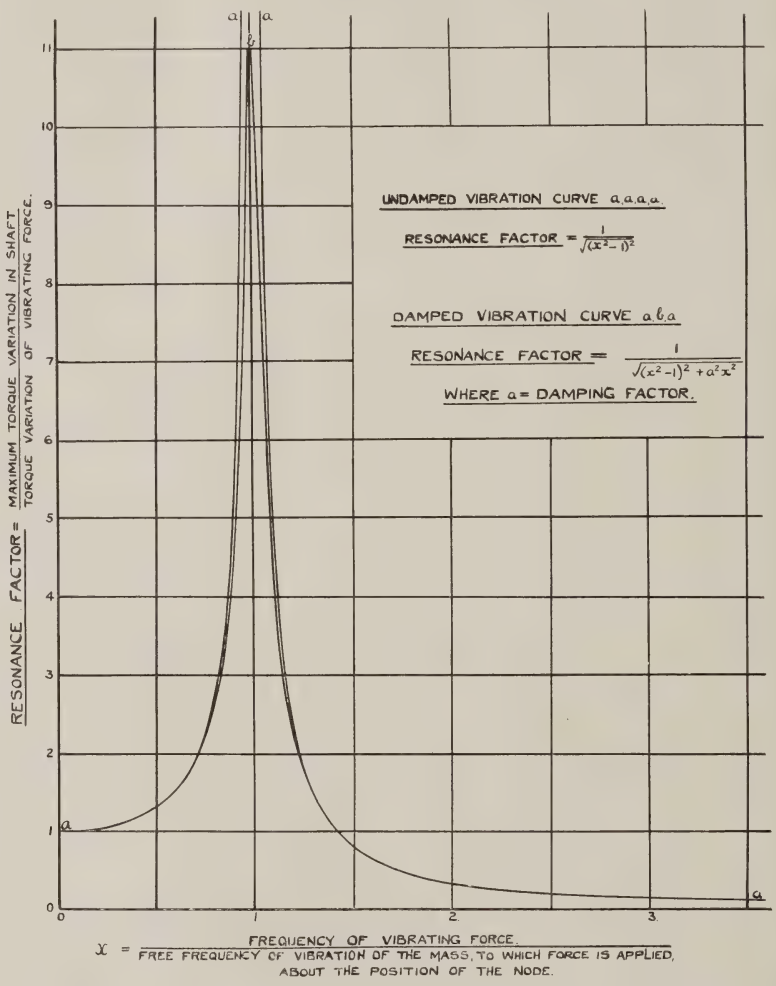


Fig 2.

force varies with the amplitude so that instead of an infinite vibration at the resonance point we only get an amplitude such that the applied force is balanced by the damping forces. The theoretical curve *aaaa* of undamped amplitude is shown in Fig. 2 and also a damped curve *aba* The actual and theoretical curves are almost coincident, except from about 0·8 to 1·2 of the critical frequency, for all values of the damping factor likely to be experienced, and outside that range the resonance

factor may be taken from the theoretical curve without introducing serious error. It is of obvious advantage to be able to determine a resonance factor without knowledge of the damping term. The multiplying factor due to resonance is of the utmost importance. For instance if the disturbing torque is ± 10 per cent. of the mean torque in a propeller shaft, a multiplying factor of 10 will produce a zero torque and any further increase will mean a negative torque. This would produce tooth separation at each vibration and the range of revolutions over which this happened would become what has been called "an unworkable range" as it is obvious that a system including gearing could not run satisfactorily for any period under such conditions. It is necessary to determine the resonance factor at the critical speed to enable the actual resonance curve to be obtained, which, combined with a knowledge of the magnitude of the disturbing torque, enables the unworkable ranges, if any, to be determined.

In a system including gearing it is necessary that consideration be given to the positions of nodes, as a dangerous condition may occur if the node falls in either train of gears. This would imply a uniform angular velocity of these gears, and any slight periodic irregularity in the teeth (whose frequency corresponded to the frequency at which the node fell in the gears) would produce a very heavy stress. These stresses become much greater if the system is so arranged that one of the nodes occurs in a gear wheel at a natural vibration frequency. The actual stresses involved can be clearly seen from examination of the expression for the equivalent inertia at the gears given in the appendix in equation 2.

Opportunities have now occurred for making actual observations on five single-screw vessels and three twin-screw vessels, all fitted with geared-turbine installations. Various difficulties were encountered in our attempts on the earlier vessels, and it was only after the adoption of precautions described later that reliable and consistent results were obtained; consequently, details are given only of two twin-screw and two single-screw vessels. The results of the earlier vessels, where intelligible, lead generally to the same conclusions. In all these cases an endeavour has been made to obtain by direct reading the variation in torque with different positions of the propeller blade. The first step taken was the adoption of Messrs. Parson's proposal to mount the torsionmeters (which were of the Hopkinson-Thring type) on a support enabling them to be moved circumferentially round the shaft, the amount of movement being sufficient to obtain readings in all positions of a propeller blade, even with a three bladed propeller. Owing to the construction of the torsionmeter two sets of readings are obtained by this method, the interval between the two being approximately 150 degrees. Difficulty was naturally experienced owing to the fact that even in fair weather and on a straight course propeller revolutions are continually varying, if only by half a revolution. When endeavouring to obtain a curve of the torque variation with different positions of the propeller

blades it is obvious that even this small variation in revolutions might completely alter the character of the curve obtained, bearing in mind that the torque varies as the revolutions squared. Finally a magnetically controlled three-pen chronograph was utilized, the movement of one pen being controlled by a clock, to the escapement of which a small extension was fitted dipping into a mercury bath and making contact every second, giving an accurate time record. The circuits of the electro magnets controlling the other two pens of the chronograph were connected to terminals making contact with every revolution of the shaft; an accurate record of shaft revolutions alongside the time record was thus obtained. This instrument was used chiefly in twin-screw vessels where it was essential that the ratio between the speeds of the two shafts must remain constant throughout a set of readings. Any reading in which one shaft had increased while the other decreased had to be left out of consideration as it was found difficult, if not impossible, to estimate the effect of interaction between the propellers.

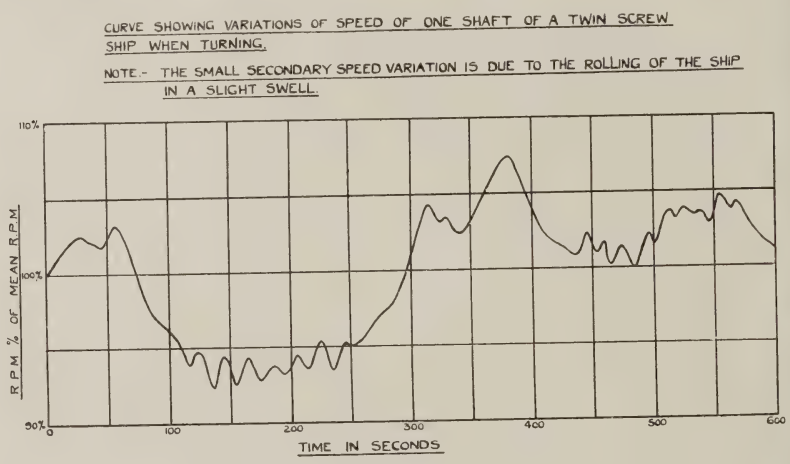


Fig. 3.

As an instance of the delicacy of the readings thus obtained it may be of interest to show the curve obtained on a twin-screw vessel showing the actual variation in revolutions of one shaft when the helm is put hard over (Fig. 3).

In the case of single-screw ships the revolution speed was counted directly, while the torsionmeter was read by taking the reading over a period such that the beam of light from the torsionmeter crossed the scale, say, 30 times, and timing this with a stop watch.

One of the chief difficulties experienced at first was that if the ship was turning during the course of a reading the reading had to be cancelled, and it was impossible to tell until the readings were being worked out which had been affected in this way. This difficulty was overcome by the use of a pendulum close to the torsionmeter by which it was possible to note after each reading whether the course had been altered and the reading could be taken again if necessary.

Torsionmeter readings were then taken throughout the available range, and finally were corrected (proportionately to the revolutions squared) from the instantaneous revolutions at the time the readings were taken to the mean revolutions over the whole set. As a result of these precautions fairly consistent results were obtained, and it became possible to draw curves giving the torque variation throughout a revolution and to obtain some indication of the relation between the extent of this torque variation and the revolutions and speed of the ship. The actual torsionmeter readings thus obtained gave the torque variation in the shaft itself.

The taking of a set of readings from the torsionmeter in the above manner occupied a considerable length of time, and it was found impossible to obtain during a trial trip curves at any considerable number of speeds. To overcome this difficulty a vibration meter was designed which drew a curve of the vibration of the shaft over a period of one revolution. The instrument is similar to ones which, we understand are used by other firms, and embodies the principle of the gauging device described by Mr. Le Mesurier in his paper on Maag Gears read before this Institution. A comparatively heavy flywheel is mounted on ball bearings on a short length of shaft and a very light aluminium pulley is similarly mounted on the same shaft. The pulley drives the flywheel through a light spring, and is itself driven by a belt off the main shafting. The pulley is so light that it follows exactly the movements of the main shaft, while the inertia of the flywheel is such that the slight variations of impulse transmitted through the spring are insufficient to disturb its uniform angular velocity. Vibrations of the shaft are thus reproduced by a relative movement of the pulley and flywheel. This relative movement is transmitted through a lever carrying a pencil which draws a circular diagram. The pencil lever was arranged to give various magnifications of the vibration so that a wide range of amplitudes of vibration could be measured. It was found with the big magnification required for small vibrations that pencil friction was so great as to

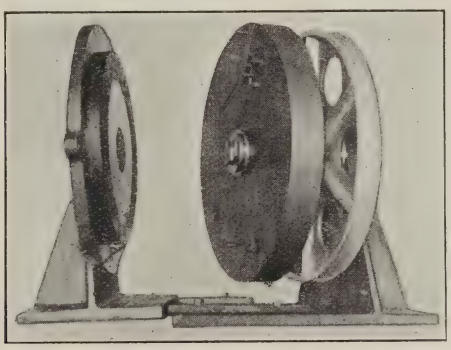


Fig. 4.—Vibration Meter.

interfere seriously with the accuracy of the readings taken, and an electrical method of recording, designed by Mr. J. C. Arthur, was used. The diagram paper was mounted on an insulated metal plate connected to one high-tension terminal of an induction coil, the other terminal of which was earthed. The pencil was replaced by a sharp metal point which was arranged to be at a distance of about 0·5 m.m. from the diagram paper. The curve of vibration was now drawn by the perforation of the diagram paper by a series of sparks passing from the sharp

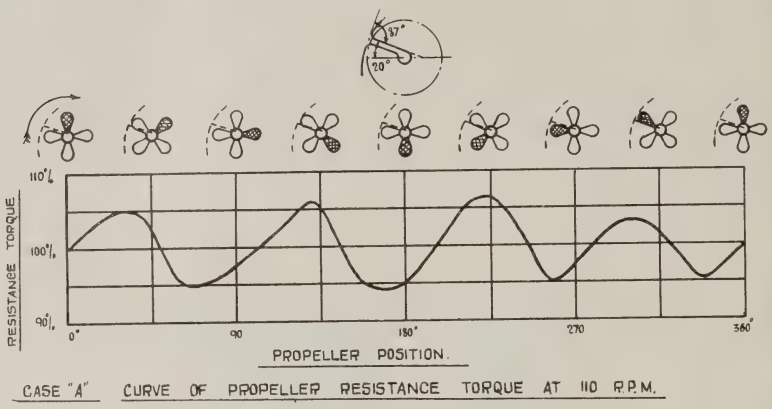


Fig. 5.

point on the pencil arm to the insulated plate behind the paper. The frequency of spark usually obtained with an induction coil is not high enough to give points sufficiently close together for a satisfactory curve from a high-speed shaft, and it was found necessary to fit a high-frequency mechanical breaker on the low-tension side of the induction coil. With the use of this instrument it was possible to get vibration readings at a large number of revolution speeds.

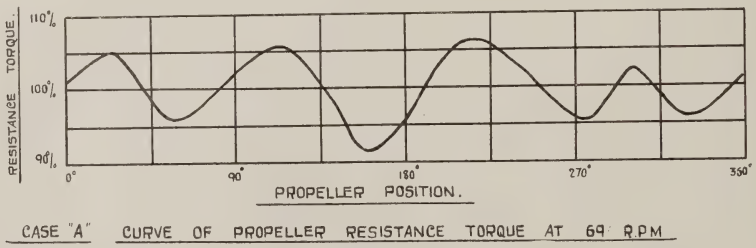


Fig. 6.

The diagrams drawn by the vibration meter show the vibrations at one position of the shaft, from which a curve of torque variation can be drawn by dividing the curve obtained into its constituent vibrations and calculating the torque variation of each of these components from the position of its node.

Case A.—This was a twin-screw vessel, 450 ft. p.p. in length, with a block co-efficient of about 0·66. The machinery consisted of double-

reduction geared turbines developing about 8,000 s.h.p. at 130 r.p.m., the speed of the vessel under these conditions being about 17 knots. The propellers were of the built type each having four manganese bronze blades.

In this case the natural frequency of the system corresponded to the vibration arising at a propeller speed of 45 r.p.m. with a four per

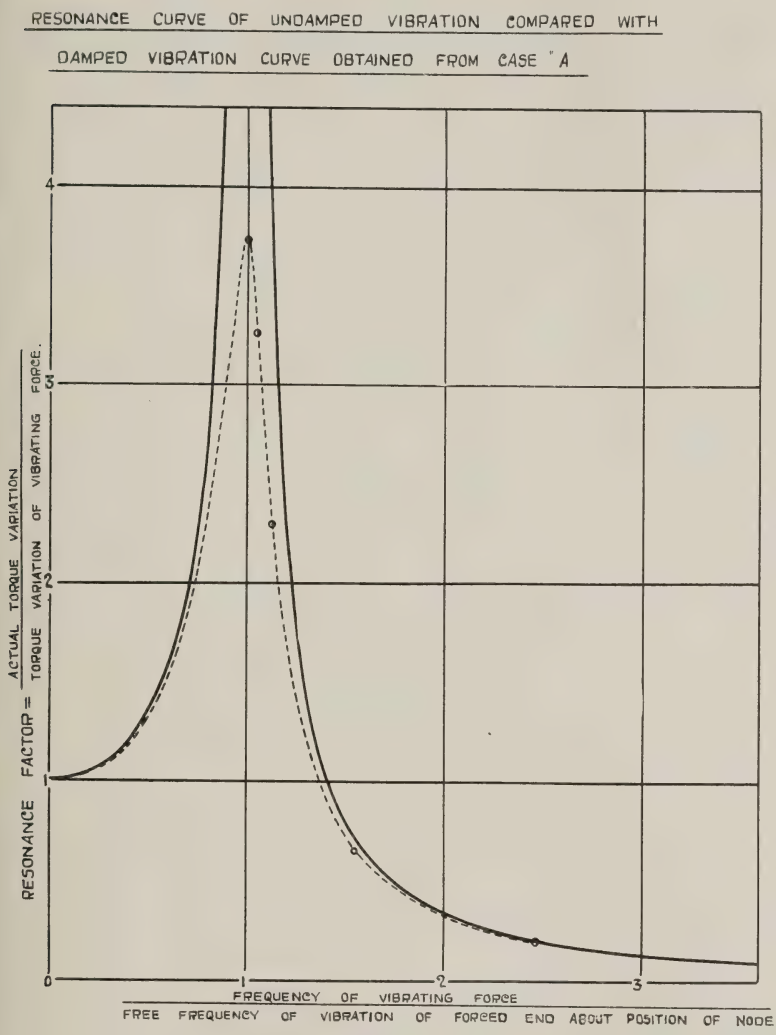


Fig. 7.

revolution variation. The amplitude of vibration was so small that accurate curves could not be obtained from torsionmeter readings. Curves were, however, obtained by the aid of the vibration meter, but while the amplitude of the four per rev. frequency could be obtained from these it was impossible to separate from the curve any of the other components of the vibration. Fig. 5 is a curve taken at 110 r.p.m. corrected for the resonance factor of the four per rev. vibration; an amplitude varying between 8 and 12 per cent. is shown by this curve. The unequal heights

of the four waves indicate the presence of another vibration of slightly different frequency, probably arising in the gearing. Fig. 6 is a curve taken at 69 r.p.m. corrected for resonance and shows the same characteristics and almost the same amplitude.

Fig. 7 is a comparison of an undamped resonance curve with a resonance curve actually obtained from vibration meter readings. This curve confirms the calculation of the critical speed as the maximum on the curve (at 44·5 r.p.m.) agrees exactly with the calculated value of the critical speed. The maximum multiplying factor in this case was only 3·7.

Case B.—This was a twin-screw vessel of 465 ft. p.p., with a block co-efficient of about 0·73. The machinery consisted of double reduction geared turbines and a power of about 4,300 s.h.p. was developed at 86 r.p.m., the speed of the vessel under these conditions being about 13½ knots. The propellers were of the built type, each having three manganese-bronze blades.

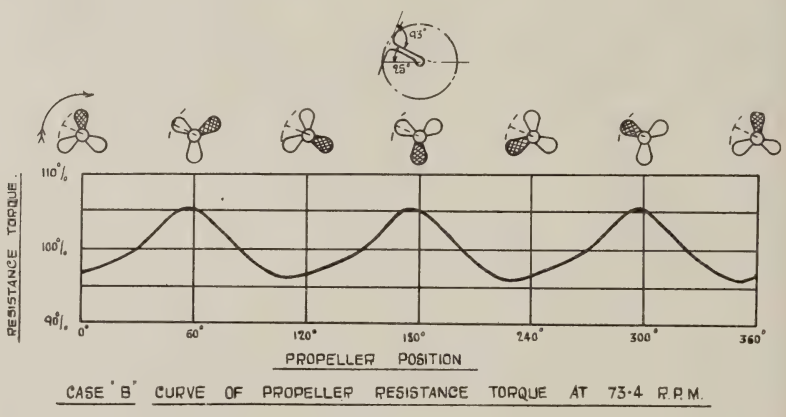


Fig 8.

In this case torsionmeter readings were unfortunately only obtained over a comparatively small angle, and as the vibration meter had not then been constructed, no very complete records were obtainable. Fig. 8 gives the curve corrected for resonance and indicates a three per rev. vibration of an amplitude of 9 per cent. with a slight indication of a six per rev. vibration whose amplitude is too small for definite measurement.

Case C.—This was a single-screw oil tanker with machinery aft, the length being 440 ft. p.p. The machinery consisted of double-reduction geared turbines and a three-bladed propeller of the built type was fitted. The power developed was about 3,100 s.h.p. at about 75 r.p.m., the speed being about 11¾ knots.

In the case of this vessel the first natural frequency of the system corresponded to revolutions of only some 10 per cent. below full-power revolutions with a four-bladed propeller. Under these circumstances the shaft diameter was increased and a three-bladed propeller fitted to remove the natural period further from the running revolutions.

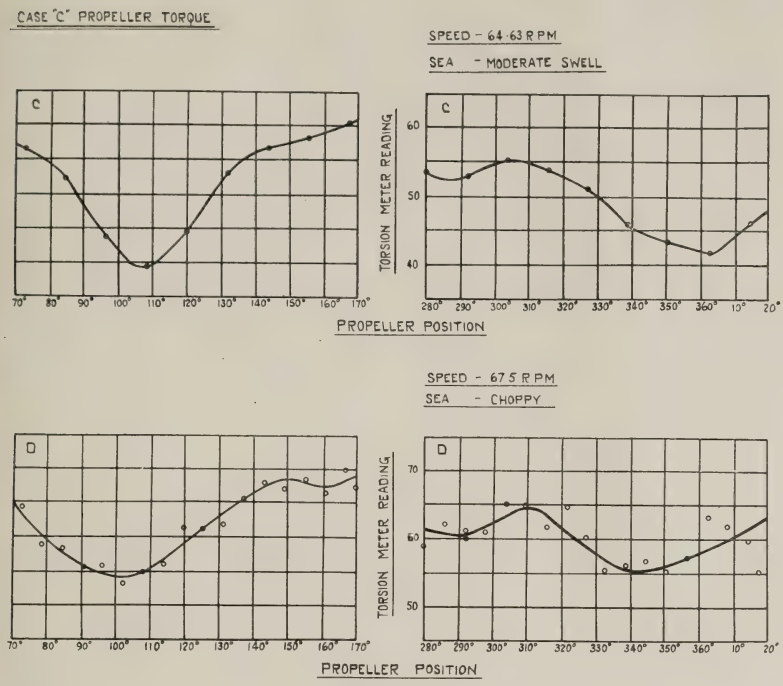

CASE "C" PROPELLER TORQUE
SPEED - 64·63 R.P.M.
SEA - MODERATE SWELL
C
TORSION METER READING
60
50
40
70° 80° 90° 100° 110° 120° 130° 140° 150° 160° 170°
280° 290° 300° 310° 320° 330° 340° 350° 360° 10° 20°
PROPELLER POSITION
SPEED - 67·5 R.P.M.
SEA - CHOPPY
D
TORSION METER READING
70
60
50
PROPELLER POSITION

FIG. 9.

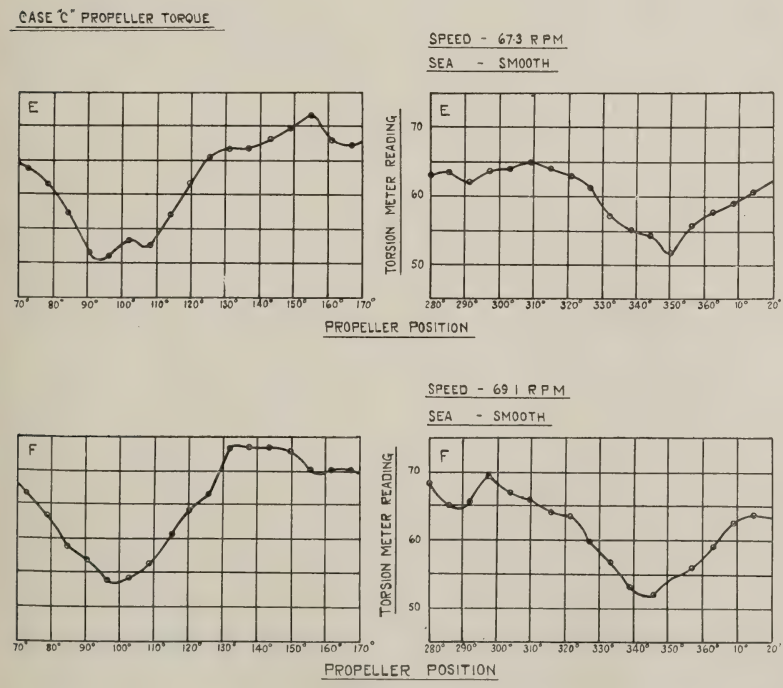

CASE "C" PROPELLER TORQUE
SPEED - 67·3 R.P.M.
SEA - SMOOTH
E
TORSION METER READING
70
60
50
70° 80° 90° 100° 110° 120° 130° 140° 150° 160° 170°
280° 290° 300° 310° 320° 330° 340° 350° 360° 10° 20°
PROPELLER POSITION
SPEED - 69·1 R.P.M.
SEA - SMOOTH
F
TORSION METER READING
70
60
50
PROPELLER POSITION

FIG. 10.

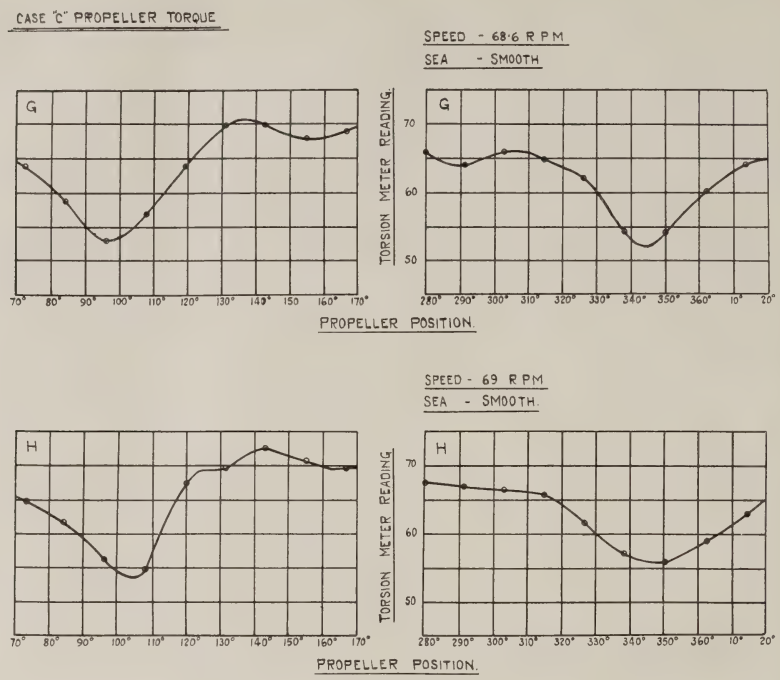

CASE "C" PROPELLER TORQUE
SPEED - 68·6 R.P.M.
SEA - SMOOTH
G
TORSION METER READING.
70
60
50
70° 80° 90° 100° 110° 120° 130° 140° 150 160° 170°
280° 290° 300° 310° 320° 330° 340° 350° 360° 10° 20°
PROPELLER POSITION.
SPEED - 69 R.P.M.
SEA - SMOOTH.
H
TORSION METER READING.
70
60
50
70° 80° 90° 100° 110° 120° 130° 140° 150° 160° 170°
280° 290° 300° 310° 320° 330° 340° 350° 360° 10° 20°
PROPELLER POSITION.

FIG. 11.

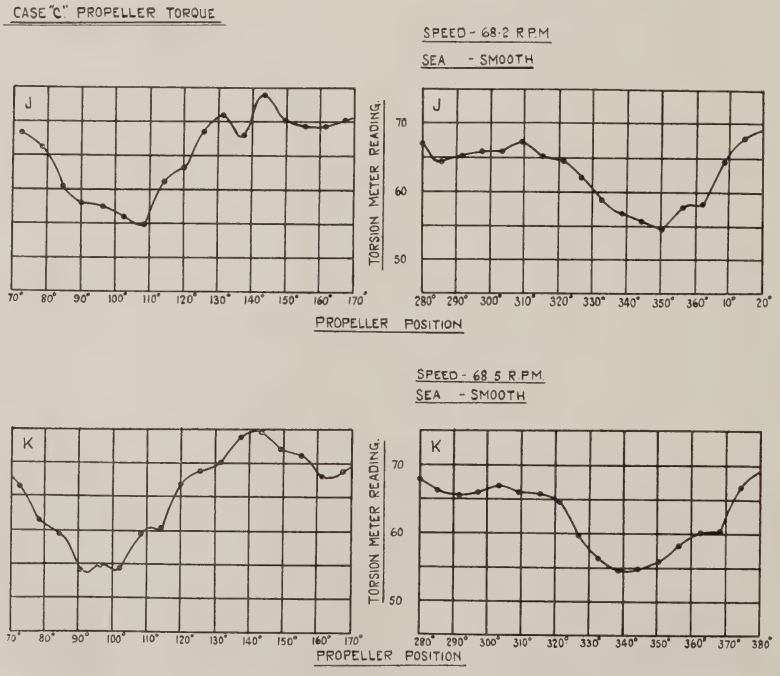

CASE "C" PROPELLER TORQUE
SPEED - 68·2 R.P.M.
SEA - SMOOTH
J
TORSION METER READING.
70
60
50
70° 80° 90° 100° 110° 120° 130° 140° 150° 160° 170°
280° 290° 300° 310° 320° 330° 340° 350° 360° 10° 20°
PROPELLER POSITION
SPEED - 68.5 R.P.M.
SEA - SMOOTH
K
TORSION METER READING.
70
60
50
70° 80° 90° 100° 110° 120° 130° 140° 150° 160° 170°
280° 290° 300° 310° 320° 330° 340° 350° 360° 370° 380°
PROPELLER POSITION

FIG. 12.

The curves given in Figs. 9-14 are plotted from readings taken with the tips of the propeller blades just immersed. The torsionmeter scale box was moved 5·8 degrees after each reading, a total angle of 94·5 degrees being possible. The readings from one mirror of the torsionmeter were separated from those of the other mirror by 153 degrees. The corrected torsionmeter readings are plotted on a base of propeller angle, a blade being vertical on top at 0, 120, 240 and 360 degrees. The torsionmeter reading to the left of the zero on the scale gives a curve from 72·8 degrees to 167·2 degrees and the right hand reading a curve from 279·2 degrees to 14·2 degrees.

The torsional vibrations caused by periodic torque variations will be due to the change in resistance caused by the passage of a blade through

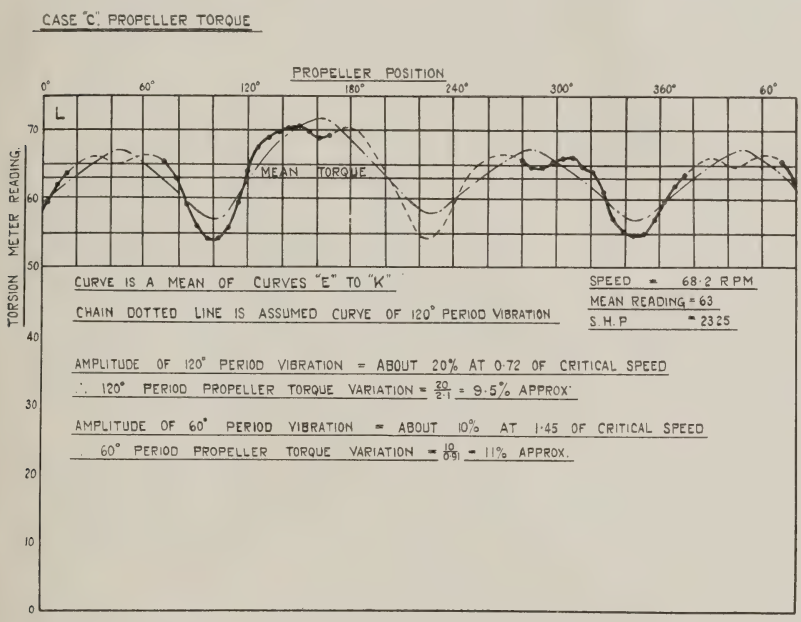


FIG. 13.

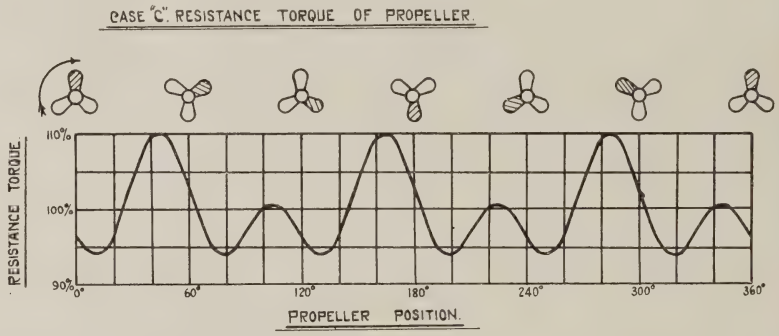
the upper portion of the aperture, and to a greater or less extent by the passage of a blade through the lower half of the aperture, considerable difference of opinion having been expressed as to the relative importance of the latter variation. If the former only is of importance, this will result in the principal vibration having a frequency of three times the revolutions; if both impulses (due to the top and bottom passage) are approximately equal, the resulting vibration will have a frequency six times the revolutions; these correspond to torque variations with periods of 120 and 60 degrees of propeller angle respectively.

Assuming a torque variation with a period of 60 degrees of propeller angle, the critical speed of the system on Ship C was between 47 and 48 r.p.m. and on the assumption of a 120 degrees periodicity, this figure is doubled, making the critical speed between 94 and 96 r.p.m.

Curves C and D (Fig. 9) were taken between London and Dover on the 20th and 21st July, 1921, and curve D illustrates the difficulty of obtaining even approximately accurate torsionmeter readings in a choppy sea. Curve C was taken when a moderate swell was running, and as will be seen, this has not interfered with the accuracy of the readings.

The readings from which curves E to K (Figs. 10, 11 and 12) were drawn were taken between Dover and Bordeaux. As the sea was calm and the turbines running at a steady speed throughout this period, the mean of these curves should give an accurate torsional vibration curve for this speed of the turbines. The curve L (Fig. 13) is plotted from the mean of the values for the curves E to K, all corrected to 68·2 r.p.m. In calculating the mean values such readings as were obviously incorrect were omitted (*e.g.*, the reading at 350·5 degrees in curve E).

In the curve L (Fig. 13) the points obtained from the mean values of readings are joined by a curve drawn in full line, and the shape of



CURVE OF RESISTANCE TORQUE OBTAINED BY TAKING THE MEAN AMPLITUDE OF VIBRATION IN CURVE "L" AND CORRECTING FOR THE RESONANCE EFFECT.

Fig. 14.

the curve between 167 and 280 degrees and between 14 and 73 degrees is assumed and shown by a dotted line.

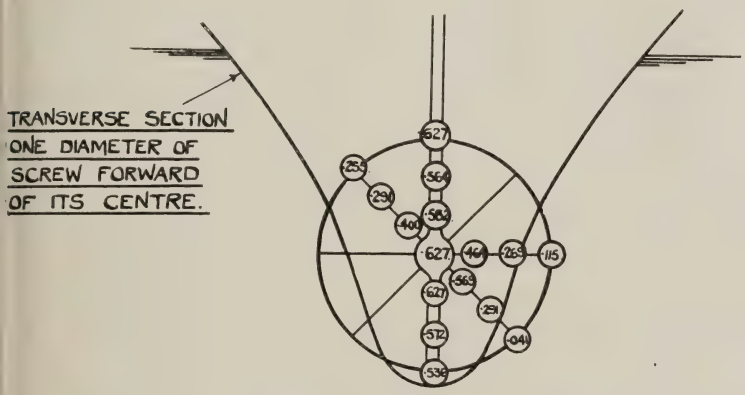
The resulting curve is evidently composed of a 60 degrees period vibration superposed on another vibration with a period of 120 degrees. The two vibrations are assumed to approximately follow a sine law and on this assumption the 120 degrees period vibration is shown in chain dotted line. The amplitude of the two vibrations can now be obtained, and dividing these amplitudes by their resonance factors, values of the torque variation at the propeller are obtained, these being 9·5 per cent. for the 120 degrees period vibration and 11 per cent for the 60 degrees period vibration.

The 120 degrees period vibration being below its critical speed, has a maximum value of shaft torque at the same time as the propeller resistance torque is a maximum, but the 60 degrees period vibration is above its critical speed, and so there is a maximum torque in the shaft when the resistance torque at the propeller is a minimum.

Curve N (Fig. 14) is drawn for the mean values of propeller resistance torque variation obtained from Curve L (Fig. 13) the 60 degrees period vibration being inverted as explained above.

Fig. 15 is the diagram of wake distribution given in the paper read by Mr. R. J. Walker, C.B.E., and Mr. S. S. Cook, B.A., before the Institution of Naval Architects on March 17th, 1921. The wake distribution for the ship in question will be similar to that shown in this diagram, and the curve of torque variation derived is quite consistent with such wake distribution. The critical speed in this ship could be determined by the vibration in the engine room and was found to be about 2 per cent. lower than the calculated value.

Although no resonance curve was obtained indicating the maximum multiplying factor it was evident from torsionmeter readings taken at



WAKE DISTRIBUTION OBTAINED ON MODEL OF "SAN FERNANDO."

FIGURES SHOWN $O = \frac{V - V_1}{V}$

WHERE V = SPEED OF MODEL.

V_1 = SPEED OF WAKE WATER THRO' SCREW.

Fig. 15.

the critical speed that tooth separation was occurring over a range of one or two revs. per min. above and below this speed. The six per rev. torque variation having an amplitude of only $\pm 5{\cdot}5$ per cent., it follows that the maximum multiplying factor must have been about 20. It must however, be remembered that this is an exceptional case, the short shafting and the further stiffening of the shafting for reasons stated above, both tending very much to decrease the damping factor and to increase the multiplying factor, and it is unlikely that under normal conditions this factor will reach a value in any way approaching 20.

Case D.—This was a ship similar to that from which Case C results were obtained but was fitted with a four-bladed propeller 18 feet 6 inches diameter, and a somewhat different arrangement of machinery. Owing to the fitting of a four-bladed propeller the critical frequency of the system again fell dangerously near to the maximum r.p.m. and the

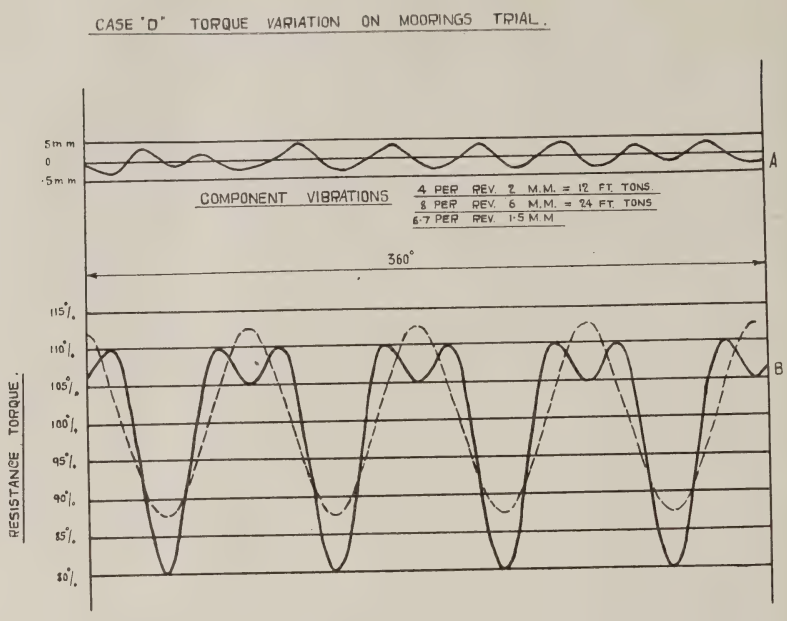

CASE "D" TORQUE VARIATION ON MOORINGS TRIAL.
5 m m
0
·5 m m
A
COMPONENT VIBRATIONS
4 PER REV. 2 M.M. = 12 FT. TONS.
8 PER REV. 6 M.M. = 24 FT. TONS
6·7 PER REV. 1·5 M.M.
360°
115%
110%
105%
100%
95%
90%
85%
80%
RESISTANCE TORQUE.
B

Fig. 16.

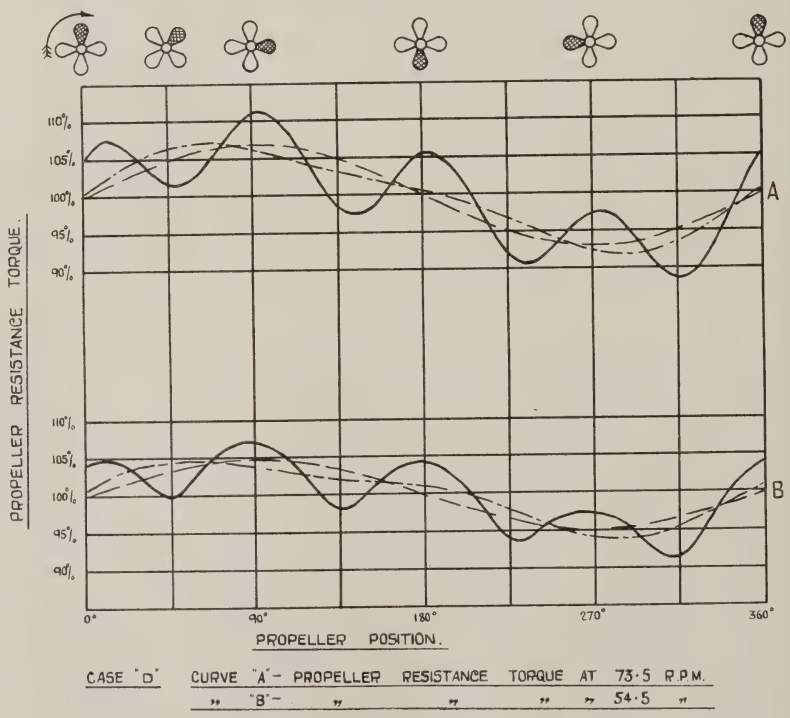

110%
105%
100%
95%
90%
A
PROPELLER RESISTANCE TORQUE.
110%
105%
100%
95%
90%
B
0°
90°
180°
270°
360°
PROPELLER POSITION.
CASE "D" CURVE "A" – PROPELLER RESISTANCE TORQUE AT 73·5 R.P.M.
" "B" – " " " " 54·5 "

Fig. 17.

shafting diameter was further increased in order to increase the critical frequency well above the maximum r.p.m. This brought the single node critical frequency up to 6·9 per second, corresponding to a critical revolution speed of about 51·5 for an 8 per rev. vibration frequency and 103 r.p.m. for a 4 per rev. frequency. Readings were taken with the vibration meter, with a propeller immersion of about 60 per cent., on the moorings trial of this vessel. The shaft was running at 45 r.p.m. *i.e.*, 0·875 of the 8 per rev. vibration critical speed and 0·437 of the 4 per rev. vibration critical speed, the multiplying factors being 4 and 1·24 respectively.

The actual uncorrected curve obtained from the vibration meter is shown at A (Fig. 16); as will be noted it shows a distinct 8 per rev. frequency of varying amplitude, no other frequency being at first apparent. On analysing the curve it was found that the variation in amplitude was due to the presence of a 4 per rev. frequency and of a 6·7 per rev. frequency: the latter will be considered later.

A combined curve of the 4 and 8 per rev. components is shown at B (Fig. 16) corrected for resonance. An amplitude of 30 per cent is indicated.

The curve shown at A (Fig 17) is from a set of torsionmeter readings taken with the propeller blade tips immersed 7 feet. The curve is corrected for resonance and consists of:—

1	per rev.	frequency	of	amplitude	13·7.
2	,,	,,	,,	,,	3·7.
4	,,	,,	,,	,,	10·5.

The shape of the uncorrected curve indicated an 8 per rev. frequency but the amplitude was so small that it could not be measured and after correction by its multiplying factor, would not amount to more than about 0·5 per cent.

In vibration-meter curves taken at about the critical speed of the 8 per rev. vibration an amplitude of about 9 per cent was apparent; this indicates that in this ship a maximum value of the multiplying factor of about 20 might be expected which agrees with the result obtained from case C.

The Curve B (Fig. 17) was taken when the propeller was immersed 90 per cent., the revolution speed being 54·5. The same components are apparent as in curve A but the amplitudes are considerably different, being as follows:—

1	per rev.	vibration	9·7	per cent.
2	,,	,,	3·2	,,
4	,,	,,	7·2	,,
8	,,	,,	0·7	,,

The value of the 8 per rev. vibration is only approximate as it is so near to its critical speed that damping has a considerable effect on the multiplying factor. The figure of 0·7 per cent. given above is calculated on the assumption of a maximum multiplying factor of 20 at the

resonance point, this giving a factor of 6·5 at the speed of 54·5 r.p.m. It must again be remembered that the possibility of so large a multiplying factor as 20 is due to the altogether abnormal circumstances existing in this vessel.

The 1 per rev. frequency is assumed, in drawing the above curves, to be due to the propeller. It is possible that it may have arisen in the low-speed gear wheel, but in view of its magnitude this is unlikely.

In all vibration-meter diagrams on this ship there occur vibrations of a frequency of 6·72 per revolution of the propeller shaft. This was the first case in which a vibration of a frequency other than an integral

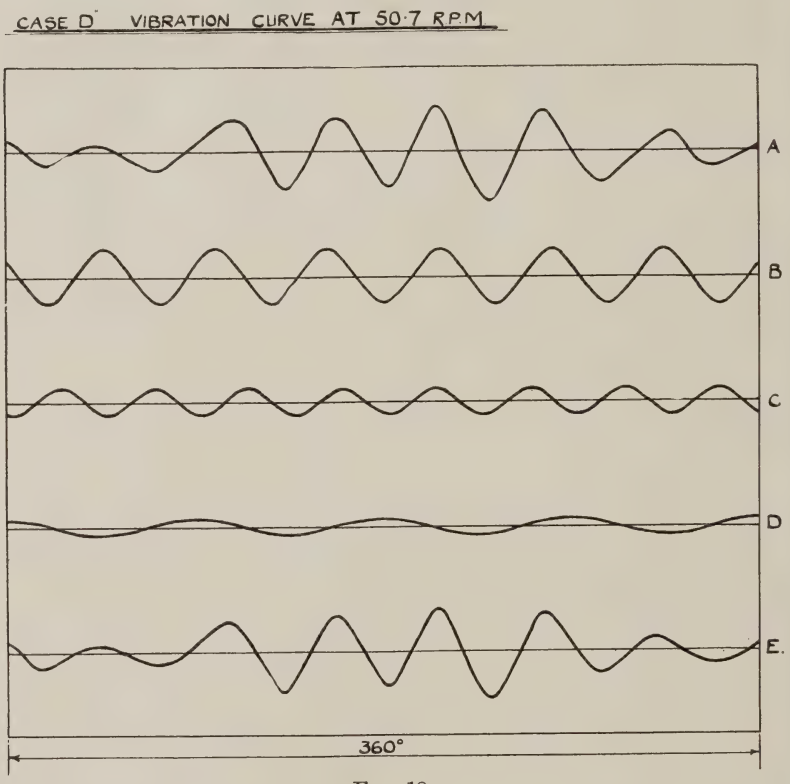


Fig. 18.

multiple of the propeller shaft revolutions was definitely observed (though indications of such were noticed in case A). In this case the second reduction gear ratio was 6·72 and it is evident that the vibration is due to a one per rev. tooth inequality in either the wheel or the pinion on the intermediate shaft. A typical curve is given in Fig. 18 and the characteristics evident there appeared in all diagrams taken about the same revolutions. The occurrence of vibrations directly traceable to gear errors is of importance and the observations made on this vessel have been fully confirmed in another vessel of the same type, in which diagrams taken while running the gearing in the shop and while running at sea show similar vibration synchronous with the speed of

the intermediate shaft, though, as might be expected, the amplitudes observed in the two cases differ considerably.

To return to the diagram given in Fig. 18 the vibration is obviously a compound vibration due to the phasing of two vibrations whose frequencies do not lie far apart. Curve A is an actual diagram taken from the vibration meter; curves B, C, and D are sine curves of frequencies 6·72, 8, and 4 per rev. respectively, and curve E is obtained by combining B, C. and D. It will be observed that the compound curve corresponds in a remarkable way with curve A.

The amplitude of a vibration arising from a gear error is directly proportional to the rigidity of the shafts on which the gear is mounted. In the case in question flexible shafts were fitted to the pinions of both the first and second reduction gears and it was the flexibility of these shafts that limited the amplitude to a magnitude such that the tooth stresses were kept within reasonable limits.

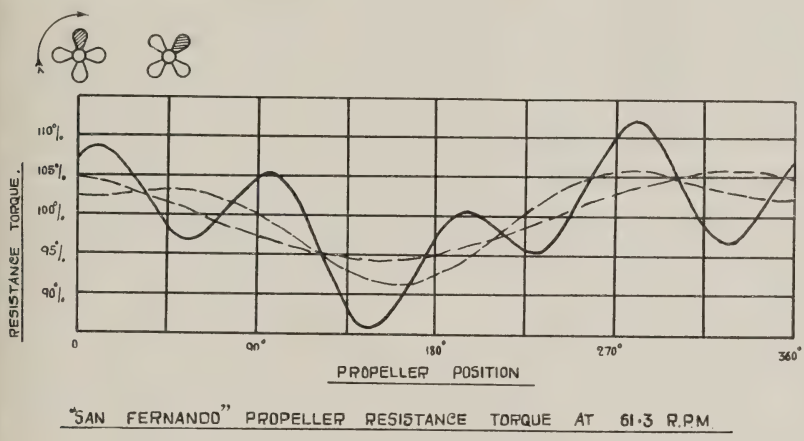


Fig. 19.

Case E.—For comparison with the above results the curve of variation of propeller torque on the S/S "San Fernando" has been calculated from the figures given by Messrs. Walker & Cook, in their paper on reduction gearing read before the Institution of Naval Architects in March, 1921. Torsionmeter readings taken at 61·3 r.p.m. were given, this speed being 0·875 of the critical speed, which is stated to be 70. The undamped resonance factor would be 4·3. The multiplying factor at the critical speed is not likely to be less than 10, giving a factor of 4·0 at 0·875 of the critical speed. This factor has been applied to the curve of torsionmeter readings given in the paper mentioned and the torque resistance curve shown in Fig. 19 thus obtained. This shows a very marked agreement with the results obtained in case D.

Conclusions.—In considering vibrations arising from the propeller the important factors which must be known with a fair degree of accuracy are the critical speeds, the actual variation in the resisting torque of the propeller and the multiplying factor due to resonance.

As regards the natural frequencies of the system; in all cases where a natural frequency could be observed it was found to be within 1 or 2 per cent. of the calculated frequency.

The data available is insufficient to make possible any general formula giving the propeller torque variation in terms of the many factors involved. On the other hand it appears that the variation is not likely seriously to exceed ± 10 per cent. under any conditions of immersion of the propeller or at any speed of the ship. (Note: It must be borne in mind that the torque variation referred to is the periodic variation occurring with a propeller working at constant immersion, etc., the frequency being determined by the revolutions. The variation in torque with a vessel pitching heavily may of course be much greater and is of an entirely different character.) The amplitude of ± 15 per cent. obtained on the mooring trial of Case D is greater than would be obtained with the same immersion at sea due to the abnormal condition of a moored vessel.

The multiplying factor due to resonance will vary very largely with different installations. In the case of vessels with machinery amidships the damping is large, as evidenced by the results obtained in Case A (Fig. 7) and the multiplying factor is small. With machinery aft and the short stiff propeller shaft resulting in such a case, the damping is very much smaller and the multiplying factor may become considerable. The damping effect will vary with the torque and revolutions and the multiplying factor will be proportionately greater at higher powers.

There appears little doubt that if resonance occurs at less than half speed there is no danger of the multiplying factor being so great as to cause tooth separation and that in such a case no unworkable range will occur. If the resonance occurs at a speed above half speed, tooth separation and a resulting unworkable range may occur, but the unworkable range will be very small and the possibility of its coincidence with normal running speed can easily be avoided if the problem is considered during the design of the installation.

The possibility of dangerous vibrations arising from causes other than the propeller is not great, but, if such occur, they are more likely to be serious, particularly in the case where the node of the forced vibration caused by periodic tooth inequalities falls in the train of gears causing the vibration. Here also it is possible in the design of an installation to avoid such a condition occurring at normal speed. It should be observed that vibrations arising from any other causes than the propeller or slow-speed wheel can only be detected by means of some such instrument as the vibration meter; only vibration of a frequency equal to or a multiple of the propeller shaft revolutions can be traced from direct torsionmeter readings.

There has been considerable discussion as to the possibility of periodic inequalities occurring in the gearing and considerable divergence of opinion as to the magnitude of such inequalities. The gearing in vessel D quoted earlier in the paper was cut with special care by a

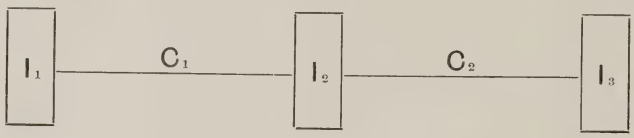
firm of high reputation, the tooth bearing on examination after trials was continuous for almost the full width of all faces and the gear was remarkably quiet when running. Yet even in such a case we find a vibration due to gear inaccuracy sufficient to cause stresses of considerable magnitude in a rigid interleaved gear, and there can be no doubt that inequalities do occur of such magnitude that it is necessary to guard against damage which they might possibly cause.

The introduction of flexible shafts between turbines and first reduction pinions and first reduction wheels and second reduction pinions appears to be the most satisfactory way of minimizing the amplitude and effect of vibrations from such tooth inequalities and by these means they can be reduced to proportions from which no danger can arise except in certain cases of coincidence with critical speeds, which cases can be avoided in the design of the gears.

The troubles which have occurred appear to be due to no inherent defect in double-reduction gearing, but the adoption of such gearing for ship propulsion has made it essential that the possibilities of torsional oscillations be thoroughly explored, and the calculations entailed, though somewhat tedious, are comparatively simple. A somewhat analogous case is that of a turbine spindle in which adequate calculations to ensure the avoidance of all critical speeds have long become a matter of routine, and it appears not unreasonable that similar precautions should be taken to protect a gearing installation.

APPENDIX.

A system has three masses of inertia, I_1 I_2 I_3 connected by weightless shafts of torsional rigidity C_1 C_2.



Let $K = 2\pi \times$ frequency of vibration of the system.

Let the system be vibrating with an amplitude of $2a$ radian at I_1.

Then when the system is at a position of maximum displacement we have :—

Torque to left of I_1 $= 0$.

Angle of twist at I_1 $= a$.

Torque to right of I_1 $= K^2 I_1 a$.

Angle of twist at I_2 $= a - \dfrac{K^2 I_1 a}{C_1}$

Torque to right of I_2 $= K^2 I_1 a + K^2 I_2\left(a - \dfrac{K^2 I_1 a}{C_1}\right)$

$$= K^2(I_1 a + I_2 a) - \frac{K^4 I_1 I_2 a}{C_1}$$

Angle of twist at I_3 $= a - \dfrac{K^2 I_1 a}{C_1} - \dfrac{K^2(I_1 a + I_2 a) - K^4 \dfrac{I_1 I_2 a}{C_1}}{C_2}$

$$= a - K^2\left(\frac{I_1 a}{C_1} + \frac{I_1 a}{C_2} + \frac{I_2 a}{C_2}\right) + K^4 \frac{I_1 I_2 a}{C_1 C_2}$$

Torque to right of I_3 $= K^2(I_1 a + I_2 a) - K^4 \dfrac{I_1 I_2 a}{C_1} + K^2 I_3 a - K^4\left(\dfrac{I_1 I_3 a}{C_1} + \dfrac{I_1 I_3 a}{C_2} + \dfrac{I_2 I_3 a}{C_2}\right) + K^6 \dfrac{I_1 I_2 I_3 a}{C_1 C_2}$

$$= K^2 a(I_1 + I_2 + I_3) - K^4 a\left(\frac{I_1 I_2}{C_1} + \frac{I_1 I_3}{C_1} + \frac{I_1 I_3}{C_2} + \frac{I_2 I_3}{C_2}\right) + K^6 a \frac{I_1 I_2 I_3}{C_1 C_2}.$$

If the system is vibrating at its natural frequency no external force is required to keep it in motion ; the torque to the right of I_3 must therefore be zero.

Dividing through the above expression by K^2a and equating to zero we get :—

$$(I_1 + I_2 + I_3) - K^2\left(\frac{I_1I_2}{C_1} + \frac{I_1I_3}{C_1} + \frac{I_1I_3}{C_2} + \frac{I_2I_3}{C_2}\right) + K^4\frac{I_1I_2I_3}{C_1C_2} = 0 \quad . \quad . \quad . \quad \text{equation 1}$$

where $K = 2\pi \times$ the natural frequency of the system.

If in place of the mass I_2 in the above system we have a gear of reduction Ratio R having a pinion of inertia I_{2A} connected to the shaft C_1 and a wheel of inertia I_{2B} connected to the shaft C_2 the equation is obtained by substituting as follows in that given for the ungeared system :—

$I_1 R^2$	for I_1
$(I_{2A}R^2 + I_{2B})$	for I_2
$C_1 R^2$	for C_1

By the above process expressions can be obtained for the natural frequencies of oscillation of any system and the effect of the moment of inertia of the shafts may be taken into account by considering them to consist of an infinite number of sections each of inertia $I_s \times \frac{dl}{l}$ connected by weightless shafts each of rigidity $C \times \frac{l}{dl}$.

The following formula, obtained by the above method, is applicable to installations having H.P. and L.P. turbines connected through gearing to a common propeller shaft :—

I_1	Moment of Inertia of propeller.
I_s	,, ,, ,, shaft.
I_0	,, ,, 2nd reduction wheel and pinions.
I_4	,, ,, H.P. 1st reduction wheel and pinion.
I_5	,, ,, L.P. ,, ,, ,,
I_2	,, ,, H.P. turbine.
I_3	,, ,, L.P. ,,
C_1	Torsional rigidity of propeller shaft.
C_2	,, ,, H.P. turbine shaft.
C_3	,, ,, L.P. ,, ,,
C_4	,, ,, H.P. Intermediate speed shaft.
C_5	,, ,, L.P ,, ,, ,,
K	$2\pi \times$ frequency of vibration.

Note.—Moments of inertia of masses and rigidities of shafts not running at propelling speed are multiplied by the square of their speed ratio.

The moments of inertia I_1 I_s and I_o and the rigidity C_1 are replaced by an equivalent moment of inertia.

$$I_E = I_O + \frac{I_1 + I_s \dfrac{\tan K\sqrt{\dfrac{I_s}{C_1}}}{K\sqrt{\dfrac{I_s}{C_1}}}}{1 - K^2 \cdot \dfrac{I_1}{C_1} \cdot \dfrac{\tan K\sqrt{\dfrac{I_s}{C_1}}}{K\sqrt{\dfrac{I_s}{C_1}}}} \quad . \quad . \quad . \quad . \quad \text{equation 2}$$

The natural frequency of vibration of the system is then given by the equation.

$$
\begin{aligned}
I_E \Big[1 &- K^2\left(\frac{I_2}{C_2} + \frac{I_2}{C_4} + \frac{I_4}{C_4} + \frac{I_5}{C_5} + \frac{I_3}{C_5} + \frac{I_3}{C_3}\right) + K^4\left(\frac{I_2 I_4}{C_2 C_4} + \frac{I_2 I_5}{C_2 C_5} + \frac{I_2 I_5}{C_4 C_5} + \frac{I_4 I_5}{C_4 C_5} + \frac{I_2 I_3}{C_2 C_5}\right. \\
&\left. + \frac{I_2 I_3}{C_4 C_5} + \frac{I_4 I_3}{C_4 C_5} + \frac{I_2 I_3}{C_2 C_3} + \frac{I_2 I_3}{C_4 C_3} + \frac{I_4 I_3}{C_4 C_3} + \frac{I_5 I_3}{C_5 C_3}\right) - K^6\left(\frac{I_2 I_4 I_5}{C_2 C_4 C_5} + \frac{I_2 I_4 I_3}{C_2 C_4 C_5} + \frac{I_2 I_4 I_3}{C_2 C_4 C_3}\right. \\
&\left. + \frac{I_2 I_5 I_3}{C_2 C_5 C_3} + \frac{I_2 I_5 I_3}{C_4 C_5 C_3} + \frac{I_4 I_5 I_3}{C_4 C_5 C_3}\right) + K^8 \frac{I_2 I_4 I_5 I_3}{C_2 C_4 C_5 C_3} \Big] \\
+ \Big[I_2 &+ I_3 + I_4 + I_5 - K^2\left(\frac{I_2 I_4}{C_2} + \frac{I_2 I_5}{C_2} + \frac{I_2 I_5}{C_4} + \frac{I_4 I_5}{C_4} + \frac{I_2 I_5}{C_5} + \frac{I_4 I_5}{C_5} + \frac{I_2 I_3}{C_2} + \frac{I_2 I_3}{C_4} + \frac{I_4 I_3}{C_4}\right. \\
&\left. + \frac{I_2 I_3}{C_5} + \frac{I_4 I_3}{C_5} + \frac{I_2 I_3}{C_3} + \frac{I_4 I_3}{C_3} + \frac{I_5 I_3}{C_3}\right) + K^4\left(\frac{I_2 I_4 I_5}{C_2 C_4} + \frac{I_2 I_4 I_5}{C_2 C_5} + \frac{I_2 I_4 I_3}{C_2 C_4} + \frac{I_2 I_4 I_3}{C_2 C_5} + \frac{I_2 I_4 I_3}{C_2 C_3} + \frac{I_2 I_5 I_3}{C_2 C_3}\right. \\
&\left. + \frac{I_2 I_5 I_3}{C_4 C_3} + \frac{I_4 I_5 I_3}{C_4 C_3} + \frac{I_2 I_5 I_3}{C_5 C_3} + \frac{I_4 I_5 I_3}{C_5 C_3}\right) - K^6\left(\frac{I_2 I_4 I_5 I_3}{C_2 C_4 C_3} + \frac{I_2 I_4 I_5 I_3}{C_2 C_5 C_3}\right) \Big] = 0 \qquad \text{equation 3}
\end{aligned}
$$

DISCUSSION.

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E. : I am not surprised to see such a large attendance. Although the Paper is one which might very rightly be called a "stiff" one, the excellent attendance reflects the interest taken in it, and the Institution is to be congratulated upon such a splendid muster this evening. In view of the presence of gentlemen of national reputation in regard to this subject, I will not myself make any remarks, but will at once call upon Dr. Smith to open the discussion.

Dr. Smith

DR. J. H. SMITH: Being particularly interested in all problems connected with gearing I welcome the Institution's invitation to join in the discussion of this Paper. The members are probably aware that I have read a paper on the gearing question at the Institution of Naval Architects. It will perhaps help those interested in the subject if I attempt to throw a little light on my own experience.

Some years ago whilst experimenting on fatigue of metals my attention was drawn to the extraordinary destructive effects on steel surfaces caused by critical speed conditions. I thought little of the matter at the time and just casually mentioned it in a short paper in *Engineering*, July 23rd, 1909. On being brought in touch with gearing troubles by Messrs. Workman, Clark & Co., of Belfast, I immediately recognized this peculiar action as playing an important part in the problems, in fact I was led to believe that this consideration was the only item of any importance apart from good workmanship, alignment and gear cutting.

The example that I had to deal with was an exceptionally bad one; Mr. Wilkie has given full details of it in the Transactions of the Institution of Naval Architects. The owners of the ship were so tired with their extraordinary experiences that they had seriously considered the replacement of turbines by reciprocating engines. Every conceivable thing had been done in the way of checking the accuracy of the work and those concerned were almost at their wits' end. I was asked to attempt to trace the cause of the trouble. An examination of the details led me to the conclusion that critical conditions of torsional oscillation were the cause of the trouble. I did not expect to find that low periodicity effects were the direct cause of the pitting, flaking and tooth smashing, but suspected that the real cause, if it could be traced, would be found to be high periodicity action. I did not suspect an actual critical condition being established such as I was able to produce in the laboratory, for if this were possible no gear system in which such a state of affairs existed would last more than a few hours or days at the most. I have spent the greater part of my life on experiments on fatigue and cyclical states of steels, and after making careful enquiries on this side of the question, I arrived at the conclusion that the quality of the steel

Dr. Smith

had not a great deal to do with the behaviour of this noisy arrangement. Having written off this item I approached the subject from the purely experimental side, working on simple models and on the actual machinery of the ships, in most cases I drew blanks.

Two rather interesting items came out in using a model, viz., the moment of inertia of the primary wheel on being increased in a disturbed gear train always led to a more noisy arrangement and elastic shafts always led to quieter running. It followed that my next step was to evolve an arrangement in which some tightening up action of the train was introduced and in which the primaries were reduced to the least possible moment of inertia. These two considerations led me directly into what I have called a nodal arrangement.

At this stage I clearly saw the difference between single and double-reduction arrangement, a point that has been very carefully avoided by all other investigators. I began moreover to realize the complexities of the problem but decided to attempt a thorough examination of the mathematics of geared drives, taking into consideration all the factors I could think of. I began by considering the disturbance produced by a propeller, the vibration of blading, the possible eddy motion effects in the turbines, the effect of the moments of inertia of propeller, turbine discs, gear wheels, couplings and shafts and set out to determine the " possible " critical conditions of the system. The only published work was that by Professor Gümbel (Verdrehungsachingunen eines Stabes mit fester Drehachse und beliebiger zur Drehachse symmetrischer Marsenverteilung unter dem Einfluss beliebiger harmonisher Krafte, Juni, 1912). This work became of such a cumbersome nature that drastic action was necessary if any useful information was to be obtained from the mathematical side.

I next tried the method of sections spoken of in the Paper which I believe is quite as good as that proposed by Prof. Gümbel. Finally I turned the whole thing down as too laborious. I have, therefore, had some experience of the sectioned method that the Authors of the Paper suggest and I can candidly say that I do not altogether agree with them. I did not find this method very helpful. I fully understand, however, that they may have pushed farther in this direction than I did; an actual example would clear the point. To see the inner meaning of my remarks one has only to examine the Author's equation 3 in the Appendix and remember at the same time that this equation is only written for single-mass shafts without all the subsidiary details. The equation is far more complex—in fact " impossible " is the only word we can use—when two, three or more masses are attached to each shaft.

The question naturally arose: What important cuts can be made in order to arrive at equations that can be manipulated? In studying vibrations one longs to obtain the predominant normal nodes and it appeared reasonable to consider usual design methods. It is useless to attempt to take into consideration every minute detail.

In the "Melmore Head" the moments of inertia are as follows:— Dr. Smith

$$I_{hp} + I_{ip} = 10{\cdot}2 \times 10^6 \; ; \; I_{ep} = 8{\cdot}9 \times 10^6 \; ; \; I_p = 0{\cdot}31 \times 10^6 \; ;$$
$$I_0 = 1{\cdot}33 \times 10^6.$$

the total moment of inertia of the propeller shaft is $0{\cdot}012 \times 10^6$. What is the effect of this ($0{\cdot}012 \times 10^6$) small moment of inertia on such an oscillatory system? Is the magnitude of mass concentration of importance or not?

I have assumed that the effect of such a small mass, roughly $\frac{1}{2000}$ of the whole, is negligible, except for the usual convenient corrections which Rayleigh and others deal with.

Two other aspects of the question might be mentioned, namely: (1) the higher nodes of a normal haphazard system can be found with accuracy when the propeller's inertia is neglected and (2) the damping which is greatest in the propeller shaft, the source of possible higher criticals.

What I would like to see is a full example worked out comparing the dissection method, Prof. Gümbel's method and the method adopted in my final equations.

I do not wish to find fault with the Paper under discussion just for the fun of doing so, for I fully appreciate the difficulties of this problem, but must say that I would have been better pleased if the Authors had attempted to obtain a solution of their own equation for a particular type of machinery upon which they were experimenting. So far as I read, they have only dealt with the fundamental vibration of the system, a vibration which I look upon as a gear slackness rather than a producer of chatter. This fundamental can with sufficient accuracy be determined by the simplest possible means in a few seconds. In short, the Authors' do not appear to have used their equations in any way whatever. My aim in dealing with the equations was to simplify them by simplifying the drive and as a result I deduced from them seven conclusions which I consider of primary importance. I particularly state of my paper that the mathematics is impossible and that being unable to move the mountain I attempt to move the prophet.

I appear to have laboured this point unduly but I have particular reasons for doing so.

The next point I wish to deal with is the statement relating to unworkable ranges. There is an error in the Paper, which I feel sure the Authors must know of, where it is stated that when there is a 10 per cent. fluctuation of propeller disturbing torque and an augmentation factor of 10, parting occurs. Parting occurs long before this even if the teeth are absolutely perfect. An augmentation factor of 10 is the limit in a perfect nodal arrangement in which neither blading, eddying, nor tooth-irregularities effects occur. For any other arrangement the augmentation factor may be as low as 6. This is rather an interesting point and I must refer the reader to a letter published in *Engineering*, October 20th, 1922. In the discussion of my paper such an idea as an unworkable

Dr. Smith

range was looked upon as ridiculous; I am pleased to see the writers of the present Paper are speaking of them. The change produced in seven months is extraordinary.

I congratulate both the Authors and the Institution on the excellent series of experimental results that they have placed on record. Only those who have carefully studied the geared-drive problem can appreciate the value of these results. When I first approached the problem I had practically no data to work upon. It was a case of groping in the dark. Had the curves recorded in this Paper been available, I should have been saved hundreds of hours of hard labour in my investigations. In taking a survey of the gearing problem there are so many pitfalls that one must proceed with care. I could make any interested, mathematically inclined reader a present of an enormous amount of information for interesting side tracks which, after many struggles, I have been determined carefully to avoid.

The Authors mention that a dangerous arrangement is got when the node of the oscillatory system falls at the gears. I will attempt to explain the real meaning of this: the word "node" has taken an important place in the literature of our subject.

In a haphazard arrangement with stiff turbine shafts the node (if we may speak of such a point in forced motion) is practically always at the gears; the moments of inertia of the turbine are so great as compared with that of the propeller that if an unworkable range is met with and hammering occurs, the teeth of the gears are practically hammering at a rigid steel wall. In a nodal arrangement owing to the elasticity of the system such a thing is impossible and damage can only occur from the rebounding action of the primaries against an anvil of very much smaller magnitude. This will seem rather an extraordinary state of affairs because most people assume that in a nodal arrangement the node is at the gears. They seem to overlook the fact that the node is only at the gears for the lower critical or fundamental free vibration. Quite a number of my critics made this blunder in the discussion of my Institution of Naval Architects Paper.

The torque arising from tooth irregularities can only be obtained by treating these irregularities as periodic and the unworkable ranges arising from them can be calculated quite readily. These unworkable ranges are always in the region of the main criticals. A paper could easily be written on this part of the subject but the Authors do little more than hint about this item although it has been suggested by many people that it is the most important one to be dealt with.

The Authors' scheme for using elasticity in the primary elements would require very careful consideration before an opinion could be passed on it. I believe that if they will examine the question more fully from the mathematical side they will arrive at the conclusion that dynamical similarity will have to be introduced to cut down the number of possible criticals; in short they will arrive at a nodal arrangement.

The effect of adding elasticity to a system (pitch lowering) makes it more and more important to cut down the number of criticals. The effect of this pitch lowering on the critical or unworkable ranges is well worth careful study and as an interesting mathematical investigation I know nothing more entertaining. Dr. Smith

I am particularly anxious that writers on this subject should get clear of this excessive-pressure idea; another error made by many of my critics. The pressure on the teeth on entering a critical range can never exceed twice the normal transmitted pressure. This is not the pressure we are looking for when damage is done; the pressure causing damage is much higher than this. Since the Authors only deal with this part of the subject very briefly I will say no more on the point. I would like to hear a new word introduced into this subject: "adaptability" appears to be the word required. The important element in any system should be the adaptability of the system for dealing with tooth irregularities and disturbances generally.

I hope that the Authors of this valuable Paper will push on with their experimental and mathematical work; on the experimental side, I would like some attention paid to the vibrations of the turbine shafts. These shafts carry the important masses of the system. If Messrs. Thorne and Calderwood will continue the development of their neat apparatus to this end of the drive, I feel sure they will obtain an ample return in the form of even more interesting results than those given in the Paper.

To me the measure of the Paper is to be found in the tail; the question naturally arises, can a person with the information given in these conclusions design a geared drive? I think not, but as the Paper is written as an unfinished product it would not be fair to say more on that point. I sincerely thank Messrs. Thorne and Calderwood for a most interesting two-hours' reading of a record of an excellent piece of conscientious work. To the North-East Coast Institution I desire also to record my thanks for the honour they confer in inviting me to join in the discussion.

Mr. STANLEY S. COOK, *Member*: This Paper should be welcomed as an important contribution to a subject of very present interest. It represents on the part of the Authors a considerable amount of careful work in experimental investigation and analytical labour necessary to interpret the results, and for this work and its able presentation in the Paper they deserve our gratitude. The mathematical treatment of the dynamics of a geared-turbine system is a fascinating study, and if this were a mathematical society or a physical society, one might say a good deal more than has yet been said on this subject. I propose, however, to adhere to the practical side of the question. Mr. Cook

The Paper confirms the view that I have always held regarding the question of torsional vibration. Under certain circumstances torsional vibration can occur in the propeller shaft under forced vibration from the varying resistance of the propeller. The general principles which suffice to determine and avoid excessive vibration of this kind were given

Mr Cook

in the Paper that Mr. R. J. Walker and I read before the I.N.A. in 1921. The only cases where it is likely to arise are those where, the engines being placed aft, the propeller shaft is short and stiff; and the remedy is to make the shaft still stiffer so as to be able to keep the working revolutions well below the critical speed. There is no difficulty in doing so, and there are numerous instances where this has been done with entire success.

Now there are no other external impulses acting on a geared-turbine system. And if the wheels are true and the teeth accurate (and I am using those words in a practical sense), there should be no other impulses capable of producing vibration. The damping effects referred to by the Authors are quite sufficient to prevent high-period vibrations in such a system. I am not advocating shutting our eyes to facts, but the calculations that have been brought forward by Dr. Smith and others of the numerous possible periods of vibration of a geared-turbine system seem

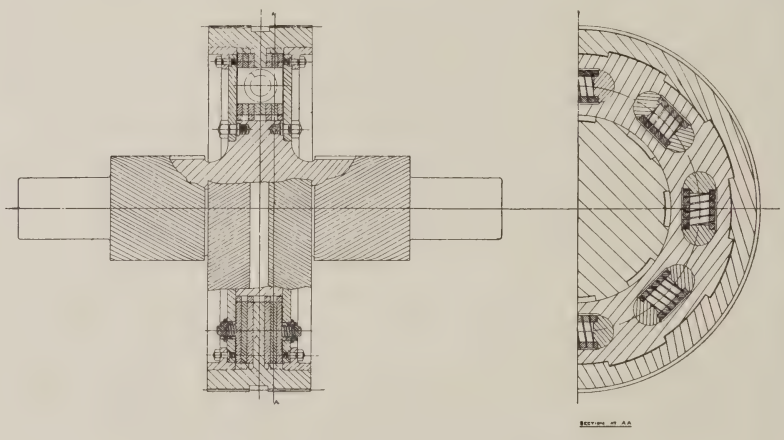


Fig. 20.—Elastic Drive Gear Wheel Shaft.

to me to magnify and distort the facts. They deal with entirely abnormal conditions and they suggest various devices for dealing with these conditions. We do not want palliatives for inaccuracy—we want accuracy—and thus to avoid the abnormal conditions they legislate for.

To come more closely to the matter, what is the effect of an inaccuracy in the gear? If we had a single turbine-driven pinion driving a single wheel, the kinetic energy of the wheel is so much smaller than that of the turbine that the speed of the latter would remain practically unaffected, and the speed of the gear wheel would vary to accommodate the error in the teeth. Even with a very considerable error, the inertia of the gear wheel is not large enough for this to give rise to any serious increase of tooth pressure; but where you have two pinions driving the same wheel, and errors in the teeth of those pinions, unless the errors synchronize—and in general they would not—the case is very different. The wheel cannot accommodate itself to both errors simultaneously and you have the turbines with their greater stored energy fighting one

another through the gear. In this case a much greater increase in tooth pressure can be produced. This I am convinced is the chief cause of noise in gearing and there is no need to invoke the theory of critical speeds to explain it. Obviously if such conditions arise the introduction of a flexible shaft in either or both drives will mollify the effects. Mr. Cook

The introduction of flexibility would, however, invite the very thing we are all anxious to avoid, torsional oscillations, and the safe way to meet such a case would be to adopt along with this increased flexibility some effective means of damping. This is quite a well-known device. I show in Fig. 20 an example of a combined elastic coupling and frictional damper as it could be applied to the primary wheel of a double-geared drive. The inner and outer peripheries are connected through a series of plates after the manner of a motor-car clutch. These plates are pressed together by axial springs whilst stiff compression springs are fitted tangentially in recesses which run through the whole series of plates. The axial springs can be adjusted to give whatever frictional grip is desired, and the torque is transmitted partly by frictional grip and partly by the tangential compression springs. One such coupling can be fitted at any part of the system, provided it is between the turbines, and it would damp the oscillations of the whole system.

The true remedy, however, for a noisy gear is to re-cut the gears and remove the error.

The Authors say that "When transmitting a torque sufficient to ensure that the gear teeth are continuously in contact such tooth contact is undoubtedly equivalent to a rigid connexion." I agree, but there is still, and no doubt the Authors are fully aware of it, a difference between the actual system and the system for which their calculations are made. I would ask you to compare Fig. 1 in the Paper with the first figure in the Appendix. The latter is a compound torsional pendulum and, supposing it to be revolving at a moderately high speed, torsional vibration could take place in such a system, if there were anything to give rise to it and it were undamped, without any external evidence of its presence. It would need careful measurement such as described in the Paper to discover that vibration was there at all. It would go on quite unseen because there would be no external forces involved. It is a question of the equilibrium between the inertia couples and elastic couples within the shaft. Such a system you have more or less in every direct drive, and it is because of this unseen operation that torsional oscillation is regarded as a dangerous phenomenon. But in the geared-turbine system this is no longer the case; there is a change of torque at the gear, the torque of the pinion being only a small fraction of the torque of the wheel, and this difference in torque is supplied by reaction at the bearings. These reactions at the bearings, therefore, faithfully copy any fluctuation there may be in the torque and any state

Mr. Cook

of torsional vibration is made obvious to the senses. Such vibration is then in exactly the same position as any kind of lateral vibration, and if you have a gear running quietly—or even only fairly quietly, for it takes very little energy to make a great noise—there need be no talk of critical speeds of torsional vibration.

There are more than a thousand successful geared turbines in service which know nothing of the formula of equation 3 of the Appendix, and run well notwithstanding, and I venture to suggest that, in the few abnormal cases the remedy is to be sought, not in the application of a formula of this description (which is an appalling one for a designer to have to use, especially in view of the uncertainty as to the exact value of a great many of its terms), but on the lines I have suggested, and chiefly in a more rigorous pursuit of the highest accuracy that it is practical to give to our gear-cutting machines.

Mr. Freeman

Mr. STERRY B. FREEMAN, C.B.E., *Member*: Our appreciative thanks are due to the Authors for their laborious work in investigating the calculations made elsewhere and comparing them with the results of experience and observation. Their solutions of the problem are evidently incomplete but all these efforts help, if only by a process of elimination, to find the cause of trouble in gearing of double-reduction.

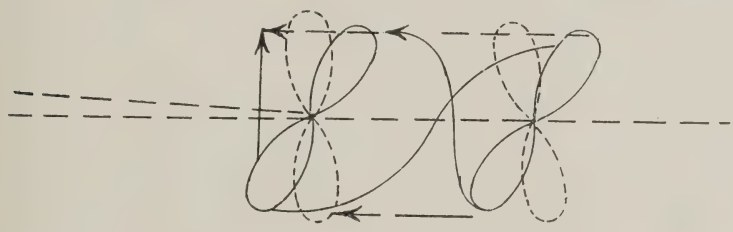
It may be useful to recall that when the further economies promised by double- over single-reduction gearing were sought, there was no published anticipation in any quarter of any difficulty arising from the further development of reduction in the gearing. In the era of single-reduction gearing the machinery, doubtless, was a "complex elastic system," "resonance" occurred when "the natural frequency coincided with the frequency of the disturbing force," but probably the applied forces were mercifully balanced by the damping forces without most of us being any the wiser.

Reciprocating machinery, especially of the fast-running naval type, has had its experiences with torsional vibration, and the cure has been found in increasing the damping effect by heavier sections and further bracing. Some years ago a certain north-east coast firm's engines had an enviable reputation for freedom from broken shafts, due, we are now told, to the rule-of-thumb addition of an additional inch to the tail-shaft diameter. It may be that had the less fortunate builders' machinery been investigated with the care that the Authors have shown in this Paper, there might have been a considerable saving effected in first cost and working expenses of the vessels of that period.

Dealing with external forces, can the Authors suggest the order of the imposed force which is applied to the blades when a vessel trims by the stern and the blades take a varying load due to the position out of the vertical?

This causes a rapidly varying load on the blades. Many vessels pass for miles through shoal water over sandy and muddy flats. The blades

must be working in different densities, but no differences are noticeable in the engine room. Would the Authors expect such conditions as these instances to set up torsional oscillations of a measurable or critical degree in the system? Dealing with internal forces, are turbine speeds regular or are they affected by the quality of the steam supplied to the engine? Superheating does not stop priming; priming, if intermittent or pulsating, might conceivably alter the torque on the pinions. Is this factor to be included in the calculations? As regards gearing teeth, it would be very interesting to have figures to show the order of the increase in load on the teeth due to an irregularity of, say, 0·005 in pitch. When observing double-reduction gear in action the intermediate shaft will be seen to be moving fore and aft in its bearings. Is this, in the Author's opinion, due to torsional oscillations or irregularities in gear cutting? If the former, what is the cause of the trouble with multi-collar thrust blocks in direct-driven turbines, which troubles did not exist, or were not so insistent, in reciprocating machinery? In other words, if an effect of torsional oscillation is to cause an alteration in torque, the lubrication of the thrust shaft should present less difficulty, since there is then an alteration in pressure. I Mr. Freeman



believe that, as a fact, there was no alteration of torque—*i.e.*, no torsional oscillation that could cause an alteration in torque. While on this point, we have nine ships at sea with Hopkinson-Thring torsion meters. The less the noise and the less the "thrill" on gear case and pipes the better the torsion meter can be read, and the longer the gearing is run the more easily the readings are obtained. As the amplitude of the oscillations of the system is not changed unless by the improvement in tooth surface—not pitch—it looks as though condition of the gearing and not the other physical properties of the elastic system is the important item.

It is encouraging to find that it is possible in the design to avoid the case where the node of the forced vibration falls in the train of gears causing the vibration. If the gears are so cut as not to cause vibration we shall be doubly secure. The Authors' final conclusions rather disarm criticism. One can admire the care and industry that have produced this Paper and yet hope that it is only part of a searching process of elimination, and that by finding out all the ways in which a system of double-reduction gearing cannot go wrong, we shall be left with the one or two matters which must be right if things are to go as they should.

VOTE OF THANKS.

Mr. Ainslie

MR. A. F. AINSLIE, *Member*, moved, and CAPT. C. W. REED, *Associate Member*, seconded, a vote of thanks to Mr. Thorne and Mr. Calderwood. The vote was accorded with great enthusiasm.

CORRESPONDENCE.

Sir J. McKechnie

SIR JAMES McKECHNIE, K.B.E., *Member*: In the short time that I have had for perusal of the Paper I have come to the conclusion that the subject is one which is worthy of the best efforts of our most brilliant mathematicians. The subject, as is well known, bristles with difficulties, and strong differences of opinion exists as to how the problems should be approached. It would seem that the Authors have not confined themselves purely to the mathematical side, but have also endeavoured to arrive at, and make due allowance for, the practical side by ingenious experimental methods. They are to be highly commended for taking such a broad view of the subject, and not being content simply with surrounding their case with ponderous equations.

I should hope to find that, when the subject has been more fully investigated, simple methods may be evolved that can readily be applied in an ordinary drawing office, so that the risk of designing turbines, gearing and shafting which, combined, would have dangerous critical periods, will be avoided.

I think all engineers who have anything to do with the subject of reduction gearing have come to the conclusion that there must be a very high degree of accuracy in the cutting of the gears. Eternal vigilance in this respect is the price of safety, and the gear-cutting machines should be overhauled periodically and the greatest care taken in the checking of the actual results from the machines before putting the gear into service. The best design will be of no avail if the best workmanship and material are not put into the gearing.

Mr. Fothergill and Mr. Gardner

MR. A. E. FOTHERGILL AND MR. R. GARDNER (of Messrs. Scotts' Shipbuilding and Engineering Co., Ltd.): The information contained in the Paper is a valuable contribution to the subject of torsional vibrations in marine reduction gearing regarding which considerable diversity of opinion still exists. As we have been engaged in somewhat similar investigations extending over a long period, we feel we can express, with some degree of confidence, our own views in this connexion.

We are in agreement with the Authors' conclusion that serious critical speeds can be avoided if the question is considered in the early stages of design, without going so far as to "nodalize" the system. With regard to tooth errors, there is no doubt that enormous forces are set up by very small irregularities quite apart from questions of vibration, whether the errors are periodic or not. We believe that torsional flexibility for reducing the effect of such errors is, if not essential, certainly very desirable, even with gears having relatively great tooth accuracy.

Mr. Fothergill and Mr. Gardner

Some time ago we had occasion to derive the critical-speed equation for the arrangement covered exactly by equation number (3), using a different method to that described in the Paper, and it may be of interest to quote this in a more general form which we find quite convenient for approximating the roots :—

Suffix 1 refers to propeller and shaft, 2, 3, 4, etc., to turbine rotors and their shafts, each in double-reduction relation to the main shaft.

Rotor inertias are I_2 I_3 I_4 etc.

Primary shaft stiffnesses are C_2 C_3 C_4 etc.

$C \div I = (2\pi n)^2$ and $B = 1 - (m^2 \div n^2)$, m being the forced frequency.

Then, if H and A are the quantities referring to primary gear wheels on their shafts, corresponding respectively to I and B, the natural frequencies of the system, neglecting shaft masses, are given by the following :—

$$I_0 + \frac{I_1}{B_1} + \frac{H_2}{A_2 - \dfrac{I_2}{B_2 H_2 + I_2}} + \frac{H_3}{A_3 - \dfrac{I_3}{B_3 H_3 + I_3}} + \frac{H_4}{A_4 - \dfrac{I_4}{B_4 H_4 + I_4}} + \text{etc.} = 0.$$

A critical-speed equation for the arrangement in which separate H.P. and I.P. primary pinions gear with a common primary wheel would be of use to some designers.

We find that the mass moment of inertia of the shafting is usually not more than 5 per cent. of that for either propeller or main gear wheel, and the statement regarding the effect of neglecting this factor on the critical speeds seems to require more substantiation than is given.

It is commonly assumed, and some of the remarks made in this Paper convey the impression also, that in the "nodal drive" the gearing constitutes a node under all conditions of running; but actually this is not the case, as will be seen on examination of the expression for acceleration of gearing given in Dr. Smith's paper.

In gears of the type "D" referred to, we found that a second unworkable range corresponding to synchronism between the propeller beats and the second natural frequency of the system was of appreciable magnitude, as well as the first unworkable range, and could not be ignored. Our calculations also showed, as might be expected, that the unworkable ranges for higher periodicity criticals, taking into account harmonics of the propeller beats, were extremely small, even allowing the full amplitude of the torque variation, and in practice they can generally be left out of count.

The explanation regarding the position of nodes does not appear to be satisfactory. Referring to the diagram in the Appendix showing 3 masses connected by 2 shafts, let e_1 e_2 e_3 be the maximum angular displacements. These are proportional to the corresponding accelerations, thus :—

$$e = -\{\ddot{e} \div (2\pi m)^2\}$$

If the disturbing torque has a maximum value P_1 and is applied to I_1 we have the following equations for accelerations :—

Mr. Fothergill and Mr Gardner

$$\ddot{e}_1 = \frac{P_1}{I_1} \times \frac{n_1^2(n_3^2 - m^2)I_1 - m^2(n_3^2 - m^2)I_2 - m^2 n_3^2 I_3}{D}$$

$$\ddot{e}_2 = P_1 \times \frac{n_1^2(n_3^2 - m^2)}{D}$$

$$\ddot{e}_3 = P_1 \times \frac{n_1^2 n_3^2}{D}$$

where $D = n_1^2(n_3^2 - m^2)I_1 + (n_1^2 - m^2)(n_3^2 - m^2)I_2 + n_3^2(n_1^2 - m^2)I_3$
and $C \div I = (2\pi n)^2$.

For example, put $m = n_1$, then $\ddot{e}_2 = P_1 \div I_1$, so that I_2 cannot be a node. Again if there is to be a node at I_2 then $\ddot{e}_2 = 0$ and $m = n_3$ but this does not also make $m = n_1$. It appears quite possible for e_1, e_2, e_3 all to have the same sign, *i.e.*, there may not be a node within the system.

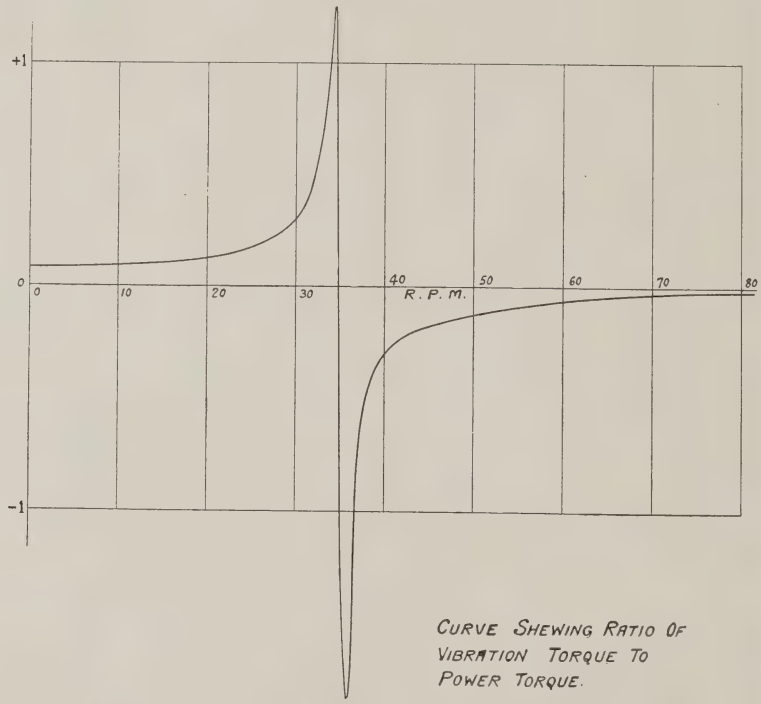


FIG. 21.

It would be interesting to know if the Authors have studied the effect of a series of fluctuating couples acting at different parts of the system in haphazard fashion and the improvement or otherwise obtained by introducing more torsional flexibility.

In the best practice, gears are gauged for detection of tooth errors and a method of estimating the magnitude of error permissible in a gear for a given maximum tooth stress requires development. The relation between the stresses and equation (2), also the inference that tooth errors cause greater stresses when the node falls at the gear than when it is at some other position is not, as stated, apparent, and further explanation is required on these points.

Mr. Fothergill and Mr. Gardner

The critical-speed equations in the Appendix do not take damping into account. We consider that the resistance of propeller and turbine blades to change of speed introduce much more important damping factors than the relatively small internal friction effect, and have worked out general equations for vibration amplitude allowing for their effect. It is found that for every critical region given by these equations there are actually two critical points—not far from each other it is true, but having a zero point between them. This is illustrated in Fig. 21, which shows the approximate fluctuating tooth pressure torque for the l.p. side of an actual double reduction gear. There is one critical speed at about 34 r.p.m. and another at about 36 r.p.m., while at 35 r.p.m. (which is about the critical speed given by the Authors' equation 3), the curve crosses the zero line. In theory, a very small damping coefficient would give this zero point, but in practice it would be impossible to keep the speed on the right spot, because the curve rises so steeply in that vicinity.

The figures obtained by the practical efforts of the Authors, for varying propeller resistance, are quite near those we developed for our own use mainly from theory, and in this connexion it might be well to emphasize the fact that Dr. Smith, when calculating the unworkable ranges for the "Melmore Head" gear, appears to have used the 33 per cent. variation of *twist*, found by Walker and Cook, for his value of disturbing couple. The actual *torque* variation at propeller corresponding to this figure is only about 8 per cent., so that the ranges he obtained were greatly exaggerated.

Generally speaking, therefore, we are in agreement with the main conclusions arrived at by the Authors. It is satisfactory to find that the trend of opinion on the question of marine reduction gearing is towards unanimity. The difficulties that have sometimes been encountered are certainly not inevitable and now that their causes are understood it should be possible to provide thoroughly reliable double-reduction gears.

Eng. Lieut.-Com Le Mesurier

Eng. Lieut.-Commdr. L. J. LE MESURIER, R.N. (ret), *Member*: Assuming the accuracy of the methods employed in calculating the natural period of vibration of the system, it is still impossible to determine either the magnitude or even the exact period of the disturbing forces produced by tooth inaccuracies in the gears. An error from one tooth to the next will cause a sudden and possibly very heavy load, which may be destructive to the teeth, while its energy, considered as a disturbing force to the system, may be insignificant. On the other hand, a periodic error due to an accumulation of the tooth errors will provide sufficient energy to produce a forced vibration of considerable magnitude, and of a frequency which cannot be accurately estimated. The fact that a forced vibration from this cause was definitely observed, as shown in Fig. 18, seems to indicate that calculations for critical speed have definitely to take as much account of cumulative tooth inaccuracies as

Eng. Lieut.-Com. Le Mesurier

of varying propeller torques; and seeing that these inaccuracies always exist, and are of a magnitude incapable of measurement, the practical value of attempting a mathematical calculation is perhaps doubtful.

We are on very safe ground in calculating critical speeds where definite periodic disturbing forces of considerable and known magnitude are involved, as for instance in a system driven by a Diesel engine. In this case the forces are relatively so large that there is no need to consider the effects of minor disturbances such as variations in the propeller torque. The vibration meter described by the Authors is very similar to an instrument used in determining the critical speed of the shafting in the motorship "Conde de Churruca," and is efficient and very simple to set up and operate.

Mr Ainslie

Mr. A. F. AINSLIE, *Member*: I should like to say how much I appreciate this Paper. It is both comprehensive and thorough, and I think the Institution is to be congratulated upon receiving a contribution which is worthy of close study, and is a valuable addition to what has been done before in this subject. It has been known for many years that the action of the propeller can set up torsional oscillations in the shafting and in the ship. It is only recently, however, that engineers have begun to make a thorough study of the whole question, and as the results of investigations, such as that which has been described in this Paper, come to be applied in practice the troubles met with at present should soon be eliminated.

At the same time it is a very provocative paper and the reader is constrained to ask himself questions all the way through. As an example of damping influence the Authors gives the internal friction of the shaft. The friction of the water is another, and one wonders whether they have any information as to the relative values of these. As is indicated in Figs. 5 and 8, the shaft bossing in these twin-screw vessels has been carried right aft to the propellers, and it is apparent that it is the bossing that is the cause of the magnitude of the variations in the torque on the shaft. With naval ships where there is a clear run aft from the spectacle eye the wake is not disturbed to the same extent and a more even torque is obtained. Would it not be an improvement if the angle of the bossing were increased aft so as to allow for the upward flow of the water as it closes in and rises at the stern? We all know how difficult it is on the trials of a ship to get constant conditions over an extended period, and as Fig. 7 shows that five speeds were investigated it is evident good use has been made of the opportunities offered. These results too are wonderfully consistent. In view of all the quantities involved very great care must have been taken with the calculations when the synchronous speed has been determined exactly and one would like to know what the assumptions were. Then, in assuming the one per revolution frequency in Case D to be due to the propeller, did they find the pitch of one blade different from that of the others?

If I might ask the Authors for a little more detailed information it would be in the direction of adding some particulars regarding the moments of inertia of the different masses together with the diameters and lengths of the various shafts. Numbers are very useful in making comparisons and in helping to crystallize one's ideas; and a quantitative result is both convincing and satisfying. Mr. Ainslie

While turbine-driven ships only are dealt with it might be worth pointing out, though it is quite outside the scope of the Paper, that the merging of all the cylinders masses into one combined mass may not be a close enough approximation for reciprocating engines fitted near the stern of the ship as the engine will occupy a large percentage of the length of the shafting.

In the early days of high-speed machinery very great difficulty was experienced in obtaining a satisfactory running balance. It was found that turning in a lathe was not sufficiently accurate and grinding had to be resorted to. So with reference to tooth inequalities it would appear it might be advantageous to grind the spindle of the hobbing-machine table to ensure that it would be quite circular and therefore run true. And when that error was corrected it would most likely be found that the headstock-spindle journals of the lathe on which the wheel was turned also required attention. It is common knowledge that the circumference of a large wheel does not run true within several thousands of an inch, and the errors in pitch are cumulative and mount up to hundredths. The setting of the wheel or pinion on the machine should be done by adjusting screws. and not by more violent means, and the centring should be from the shaft journals on which the wheel rotates in preference to the rim.

All who are responsible for the running of turbine machinery will readily agree with the Authors that the possibility of synchronous oscillations and other questions should be considered and solved during the design of the installation. Whilst striving for the best it is a mistake to demand workmanship superior to what can be obtained commercially, or to require in the materials used properties they do not possess, for there is not the slightest doubt that. with the usual materials and standard of workmanship, double-reduction gearing can be made quite as reliable as single-reduction has proved itself to be.

Capt. C. W. REED, *Associate Member*: I should like first of all to express my appreciation of the amount of labour put into the Paper which is most interesting, especially as it deals with a difficult subject that many of us have been investigating, and about which little is published. The methods used in recording the variations in torque with different positions of the propeller blade and the curves obtained are most interesting, but it is a pity that the Authors do not appear to have evolved any formulæ from them. They state on page 161, that three important factors must be known when considering vibrations, namely, critical speed, variation in resisting torque of propeller and resonance Capt. Reed

Capt. Reed

factor. They state, however, that the data available is insufficient to make possible any formula giving propeller torque variation (which is the second factor mentioned), and as the resonance factor depends upon this latter (see Fig. 2) it would appear that no formula was derived for this resonance factor.

Regarding the critical speeds, the very complex formula given in the Appendix is claimed to be obtained from fundamental principles, and not from the experiments, and depends entirely upon the statement (page 144) that the system including gearing may be regarded as an unbroken system. However the firm which I represent cannot agree with this statement, as it would only hold true if there were no tooth-clearance.

The firm by whom I am employed have now completed a large number of vessels fitted with reduction gearing, none of which are fitted with "quill" drive, and in none of which have we experienced the slightest trouble due to synchronization of torsional oscillations. The method we have used for determining the critical speeds (so that we may modify the design if near the running speeds) considers the system as a broken one, and consisting only of the propeller and main gear wheel connected by the main shaft, and this method gives a simple formula (assuming the shafting to be fixed at the gear-wheel end) which at the same time appears to gives accurate results.

Under these circumstances we cannot, of course, agree with the Authors' conclusion that the introduction of flexible shafts will solve the difficulty. We have always borne in mind the following points which are essential to satisfactory running:—great care in alignment, efficient lubrication, fitting of very strong and rigid gear cases, and, most important of all, the point that the teeth cannot be too accurately cut.

Another point about which I am not quite clear is that the formula given appears to be based on considering the shafts as "weightless" (see Appendix), yet it is stated on page 144 that "the mass of the propeller shaft cannot be neglected." Nor does the length of the propeller shaft appear to be included in equation 3. In conclusion I think that it would be very interesting if a fully worked out example of this equation could be published.

Mr. Gordon

MR. WILLIAM GORDON, *Member*: I am sorry that I shall not be able to attend the meeting owing to pressure of business, but I hope it will be well attended and that there will be a splendid discussion. The subject, I am sure, is worthy of it, and I predict that it will be the decision of the meeting that Mr. Thorne and Mr. Calderwood deserve the very best thanks of the Institution and should be highly commended for their splendid Paper on a most difficult subject. I should expect to find that it will be a Paper which will be often referred to in years to come, and may prove to be a stepping stone to greater knowledge in the

determination of suitable sizes for shafts and gearing, so that in ordinary conditions of service no trouble may be expected to be encountered due to torsional oscillations. Mr. Gordon

Dr. H. FRAHM (Translation): I have read the Paper with interest and note that the Authors have succeeded, even with a torsionmeter giving readings at one point only, in obtaining valuable data. Dr. Frahm

The great difficulties attending the undertaking of tedious research of this kind and the unavoidable discrepancies that occur when the individual points are combined to form an intelligible diagram, have led me to construct a torsionmeter that will draw a continuous curve of the variation of turning moment. I enclose a reprint of an article describing this instrument and its use.* The instrument has been used for a long time to measure the torsional vibrations of the shafting of all ships built by Blohm & Voss.

As I intend, at some future date, to publish in Germany an account of my latest research in the problem of torsional vibration, and as at present I have no time to analyse this information, it is not possible, to my regret, for me now to deal in detail with the very complicated problems dealt with in the Paper. I am very pleased to note that the occurrence of torsional vibrations is receiving the consideration of an ever-increasing number of engineers in the design of ships' machinery, and wish your Institution every success in the work it is doing on this important problem.

Mr. A. H. TYACKE (Earle's Shipbuilding & Engineering Co., Ltd.): I have read the Paper with very great interest, and think that the investigations of the Authors should be of considerable assistance to designers of geared-turbine installations in helping them to avoid such gear failures as have most unfortunately occurred recently. It is most desirable that everything possible should be done to restore the confidence of shipowners in this form of marine propulsion. Mr Tyacke

I notice that the Authors suggest the introduction of flexible shafts between turbine and first-reduction pinions, and first-reduction wheels and second-reduction pinions, with the object of minimizing the amplitude and effects of vibrations. The introduction of such shafts appear to me to complicate considerably the design of the gearing and gear boxes, and would introduce another possible element of danger, which, in my opinion, would not appeal to the ordinary engineer. The greatest simplicity possible is desirable in this class of machinery.

* The article entitled "Ein neuer Torsionsindikator mit Lichtbildaufzeichnung und seine Ergebnisse," reprinted from "Zeitschrift des Vereines deutscher Ingenieure," 1918, p. 177, may be consulted in the Institution library.

AUTHORS' REPLY.

The Authors

We should like first to express our thanks to the members of the Institution for the kind manner in which they have received the Paper, and particularly to express our gratitude to those gentlemen who have made valuable contributions to the discussion.

Before dealing in detail with the points raised, we think it would be well to quote one example of the application of the formula given in the Appendix of the Paper to a geared-turbine installation, and to give the results obtained with and without allowance for the mass of the propeller shaft, as doubt has been expressed by several gentlemen as to the necessity of the procedure which we put forward in the Paper.

The case taken is that of a double-reduction-geared-turbine installation with machinery amidships, and the inertias of the turbines, shaft dimensions, etc., are as follows:—

Moment of inertia of propeller	$= I_1 = 2{\cdot}9$	tons ft.^{2}	divided by "g"
,, ,, ,, shaft	$= I_s = 0{\cdot}241$	,,	,,
Equivalent ,, ,, second reduction gear	$= I_0 = 2{\cdot}4$	,,	,,
,, ,, ,, of H.P. first reduction gear	$= I_4 = 3{\cdot}73$	,,	,,
,, ,, ,, of L.P. first reduction gear	$= I_5 = 3{\cdot}73$	,,	,,
,, ,, ,, of H.P. Turbine	$= I_2 = 25{\cdot}7$	,,	,,
,, ,, ,, L.P. ,,	$= I_3 = 28{\cdot}8$	,,	,,
Torsional rigidity of propeller shaft	$= C_1 = 944$	ft. tons torque	per radian twist
Equivalent ,, ,, H.P. intermediate Shaft	$= C_4 = 21{,}700$	,,	,,
,, ,, ,, L.P. Intermediate Shaft	$= C_5 = 21{,}700$	,,	,,
,, ,, ,, H.P. high speed shaft	$= C_2 = 92{,}200$	,,	,,
,, ,, ,, L.P. high speed shaft	$= C_3 = 92{,}200$	,,	,,

First reduction gear ratio = 5·18
Second ,, ,, ,, = 5·86

Note.—Equivalent moments of inertia and torsional rigidities are obtained by multiplying the actual values by the square of the ratio of the revolution speed of the wheel or shaft under consideration to the revolution speed of the propeller shaft.

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Values of natural frequencies of vibration of the system :—

Moment of inertia of propeller shaft neglected in the calculation.	2·89 ; 3·87 ; 18·97 ; 29·1 ; 31·3.
Moment of inertia of propeller shaft considered in the calculation.	2·89 ; 3·87 ; 18·71 ; 29·24 ; 30·9 ; 32 ; 62·6 and multiples of 31·25.

It should be noted that the natural frequencies given when the shaft mass is neglected are limited to 5, and it is impossible that an elastic system should have a finite number of natural frequencies. When the shaft mass is taken into account there is not, in this case, any very great variation in the values of the 5 frequencies obtained when the mass is neglected, but additional frequencies, including an infinite series, are introduced, of which the two lower ones must obviously be taken into account.

In the set of machinery described above there are no critical frequencies near to the normal running speed, but if similar machinery had been fitted in a ship with shafting about 20 to 25 feet longer the second of the two natural frequencies introduced by consideration of the mass moment of inertia of the shafting would be so reduced that it would exactly coincide with the high-speed-pinion revolutions at the normal sea speed of the machinery; a dangerous condition could thus arise which would not be made evident if the mass moment of inertia of the propeller shaft were neglected in the calculation.

We think the Institution is much indebted to Messrs. Fothergill and Gardner for their contribution to the discussion, in which they give in considerable detail the results of their investigations into this problem, and it is very gratifying to note that in general they confirm our conclusions.

The equation which they give is identical with equation 3 of our Appendix except that it does not take into consideration the inertia of the mass of the propeller shaft and that it is of a general form, making it applicable to systems having more than two turbines gearing on to a common wheel. We hope that the example given above will clear up the doubt expressed as to the effect of the mass moment of inertia of the propeller shafting, and in this connexion would point out that it is not the actual magnitude of the moment of inertia of any mass that is of importance, but the relation that this bears to the rigidity of the shaft on which it is carried, and the relation between the mass moment of the propeller shaft to the rigidity of that shaft is in many cases of the same order as that of the ratio of the turbines to the rigidity of the turbine shafts. It must also be remembered that the natural frequency of an unloaded propeller shaft is as low as 2,000 per minute for as short a length as 120 feet.

We regret that our explanation of the movement of the node position was inadequate, and thank Messrs. Fothergill and Gardner for elaborating this explanation. The position of the node in a system

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varies with the frequency of the vibrating force, and, as pointed out, it is possible that there may be no node in the system, the vibration being about an imaginary node outside the system, and such will always be the case for very low frequency vibrations. Such a condition can and has been produced and observed in an experimental apparatus.

We entirely agree with the remark that in the nodal drive the gearing does not constitute a node under all conditions; it can, of course, only constitute a node at certain definite revolutions.

Messrs. Fothergill and Gardner ask if the effect of fluctuating couples acting in a haphazard fashion has been considered. Such a condition of affairs exists in a reciprocating engine driving a propeller shaft and propeller, and a considerable number of observations have been made in this case. We have been led to the conclusion that each force and the resulting vibration may be considered individually, and that the total force and the actual vibration at any point in the shaft will be the sum of the individual forces and vibrations.

Further explanation is asked for of the conditions arising when a node falls at the gear. Equation 2 in the Appendix is an equation of the equivalent inertia, at the second-reduction gear, of the propeller, propeller shaft and main wheel, and equations of similar form can be obtained for the other free ends of the system. It is obvious that each of these expressions becomes infinite for certain values of K, and when two of these expressions become infinite for the same value of K, and there is a tooth inequality in the second-reduction gear such as to produce a vibration of the corresponding frequency, tooth separation must occur, however slight the tooth inequality.

Comment is made that the critical speeds in the Appendix do not take damping into account. Damping does not affect the actual value of the critical speed, and we think there is some misunderstanding on this question of damping. The curve given by Messrs. Fothergill and Gardner is not, as they say, a curve of the total amplitude of vibration, but is, in fact, a curve showing the amplitude of the vibration at the times when the vibrating force is a maximum. The amplitude of a forced vibration of an undamped system is given by $S = \frac{E}{h^2 - p^2} \cos p.t.$ where h is the natural frequency of the system and p the frequency of the vibrating force. It follows that this vibration changes phase by 180° when $p = h$, *i.e.*, at the critical speed. In a damped system the change of phase is gradual and the actual vibration and the vibrating force are 90° out of phase at the critical speed. The ratio of amplitude of actual vibration and vibrating force is given by the expression $\frac{1}{\sqrt{(1-x^2)^2 + a^2x^2}}$. Where x = ratio of the frequency of the forced vibration to the frequency of the free end of the system about the position of the node of the forced vibration, and a is the damping term. This expression multiplied by sin (90 – phase difference) gives the curve

shown by Messrs. Fothergill and Gardner; it is obvious that this product must be zero at the critical speed. Actually there is only one critical speed, and it is the manner in which the curve has been derived that gives the erroneous impression of two. The Authors

With reference to Dr. Smith's remarks we hope that the example worked out above has removed some of the doubts expressed by him as to the utility of the equations given. The additional complications in the equations proposed by us, as compared with those given by Dr. Smith, are not great. We maintain that the definite evidence we have obtained of forced vibrations arising from gear inequalities renders it essential that account should be taken of critical frequencies (up to the maximum which may coincide with periodic gear errors arising in the first-reduction train) similar to those actually observed in the case of vessel D arising from errors in the second-reduction train. We have endeavoured to check our formula by direct observation, but until it is possible to make direct observations of forced vibrations of the high frequencies which will result from errors in the first-reduction train, we can do no more than assume that a formula which gives the lower frequencies to a high degree of accuracy will give the higher frequencies to a sufficient degree of accuracy for practical purposes, and we think this assumption is not unreasonable.

With reference to Dr. Smith's comparison of the inertia of the propeller shaft to the turbine, this has been dealt with earlier in our reply. The consideration of the moment of inertia of the propeller shaft in the case of the "Melmore Head" introduces a natural frequency of about 2,000 per minute, which, being below the speed of the h.p. turbine, cannot be neglected, and which, if added to the critical-speed diagram given in Dr. Smith's paper, materially alters the appearance of that diagram.

Dr. Smith draws attention to what he describes an error in the Paper where it is stated that with a 10 per cent. fluctuation of the propeller torque and a multiplication factor of 10, parting occurs. This statement is, of course, true only when the turbine inertias are approximately equal and this happened to be the case in all vessels with which the Authors have dealt. The theoretical limit of the multiplying factor with difference in inertia is 5, and the factor of 6 which Dr. Smith gives is probably correct as being the nearest approach to the theoretical limit which will occur in practice.

We disagree with Dr. Smith's statement that in a rigid gear the node is always at the gears, though such a statement is in most cases true if confined to the vibration arising from the propeller. It is not the case with vibrations arising from the gears, and we would again emphasize our opinion that it is these vibrations that are of the greatest importance.

We entirely agree with Dr. Smith in his statement that the actual maximum pressures cannot exceed twice the normal while the system is

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vibrating regularly, but the hammering which will occur whenever such a condition is exceeded will produce pressures of very great magnitude.

Mr. Cook expresses a certain measure of agreement with the views put forward in the Paper in that he agrees that the torsional vibrations which are caused by the propeller can be readily dealt with and there should be no possibility in a properly-designed system of any danger from this cause. He then states that there are no other external impulses acting on a geared system and thus infers that the magnitude of gear errors in modern well-cut gears is so small that they may be neglected as possible causes of vibration. We do not think that this stage of perfection has yet been reached and we think it is generally admitted that cumulative errors of the type in question do occur, but we agree with Mr. Cook that an exaggerated view has been taken when higher harmonics of the propeller disturbing force have been taken into account. Mr. Cook himself proceeds to consider the effect of a pitch inaccuracy if such could exist, and describes results of such inaccuracy in a double-reduction gear. We are entirely in agreement with the description he gives of of interaction between the gear errors, but there can be no question that any such interaction must of necessity be intensified if the frequency coincide with a natural period of vibration. Mr. Cook then admits that flexibility is at least a palliative for this interaction but he introduces a curious argument against its adoption in that it would invite torsional oscillations. There is no reason why a system containing flexible shafts should be more susceptible to torsional oscillations than one composed of stiffer shafts, the only effect of the greater flexibility being the alteration of the natural frequencies with a lowering of the frequency of the fundamental vibration, to which there is no objection. Incidentally with flexible shafts the critical speeds are much more defined and much more easily calculated.

As an alternative to flexibility Mr. Cook proposes a damping device and shows it as it could be applied to the primary wheel of a double-geared drive. To overcome the effect of vibrations produced by an external force such a damping device is very efficient and, as Mr. Cook remarks, its application for this purpose is well known. We are, however, dealing here with something of an entirely different nature. The vibration with which we are concerned is not caused by a force but by a relative movement between two parts of the system due to the gear inaccuracies. This movement cannot be eliminated and the adjustment of the damping device to give the requisite slipping effect to take up this movement presents considerable difficulties, and even so, would not be effective under all conditions. When the desired result can be obtained by so simple an arrangement as a flexible shaft there appears to us no advantage in adopting what at best is only a partial palliative at the expense of considerable complication.

We do not suggest, as Mr. Cook seems to assume, that the noise of gearing is due to synchronism between gear errors and natural fre-

quencies, but in cases where such synchronism causes actual tooth separation there is usually a definite increase in noise. There can be no question as to the external evidence of torsional vibrations in a gearing system due to variation in the bearing reaction and this effect has been observed in several cases. The Authors

We do not understand Mr. Cook's reference to the uncertainty of many of the terms in equation 3. The value of each term is easily calculated, the only doubtful one being the propeller inertia and we think there is now general agreement that an addition of 25 per cent. to the inertia to allow for the entrained water gives a fair degree of accuracy. Considerable errors in the propeller inertia would have no effect on the value of any but the lowest natural frequencies. Once the inertias and shaft rigidity are calculated the solution of the equation is comparatively simple by graphical methods, the substitution of values of K from about 5 to 400 giving a curve which covers the whole of the possible working range.

Mr. Ainslie comments on the relation between the bossing and the magnitude of the propeller variation. It may be of interest that the results obtained from our investigations have already led to some modifications in bossing design in the direction suggested by Mr. Ainslie, *i.e.*, the angle of bossing has been increased to allow for the upward flow of the water at the stern. With regard to the one-per-revolution frequency in case D this was assumed to be due to the propeller as its amplitude was so great that an abnormally large error in the low-speed wheel would be necessary to cause it, and when the gearing was running in the shop there was no evidence of this. The propeller in question was carefully pitched in the shop but there was no very noticeable difference in any one blade.

Mr. Ainslie refers to reciprocating engines and we can confirm his remark that the merging of all the moving masses into one mass will not be possible. It will be necessary to consider each crank and the torsional rigidity between cranks separately.

We are much interested in Mr. Ainslie's remarks with regard to additional precautions with regard to gear-cutting machinery. We entirely agree with what he and other gentlemen have said as to the undesirability of asking for an impossible standard of perfection but undoubtedly considerable improvement can still be made and we have not yet attained the highest accuracy which might reasonably be asked for from gear-cutting machines.

Some of Mr. Le Mesurier's remarks almost convey the impression that he assumes a relationship between frequency and magnitude of disturbing force, but we cannot think that this can have been intended; the magnitude of the disturbing force does not enter into the calculation of the critical frequency at all. We are inclined to question Mr. Le Mesurier's assertion that the gear inaccuracies are of a magnitude that it is impossible to measure and think that the instrument described

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by him in his paper on Maag gears provides a measurement of these inaccuracies to a high degree of accuracy. Mr. Le Mesurier differentiates between a considerable error from one tooth to the next and a cumulative error. It appears to us that the first category of errors are negligible in gears made by well-known gear makers, and the determination of the frequency of the disturbing force due to cumulative errors surely presents no great difficulties. The most common cause is probably that of a slight increase in pitch from one side of a wheel to the other, *e.g.*, giving a maximum and minimum pitch once in a revolution. This in a high speed pinion will, of course, produce a forced vibration of a frequency equal to the revolutions per minute of that pinion.

The torsionmeter devised by Dr. Frahm is very interesting, as are also the results obtained by its use. It seems to suffer from one disadvantage in that it measures only the torque variation, and can give no indication of the position of the node of any vibration. This latter information is, in a geared turbine, of value in tracing the source of vibration which occurs. The vibration meter which we use (which is very similar in principle to that used by Dr. Frahm many years ago) enables the position of the node to be determined.

Capt. Reed appears to have misunderstood our conclusions with regard to propeller torque. While it is impossible to put forward any formula for the propeller torque variation, we think a limiting value of 10 per cent. can, with safety, be adopted, and it appears to us that this limiting value is all that is necessary for ordinary use.

Capt. Reed is incorrect when he states that the resonance factor depends upon the propeller-torque variation. The resonance factor in any elastic system vibrating under a forced vibration depends only upon the ratio of the frequency of the vibrating force to the frequency of free vibration of the system to which it is applied. Fig. 2 is an ordinary resonance curve, and does not seem to need any explanation.

Capt. Reed appears to query the statement that the formula given is derived from fundamental principles, but this derivation is surely clearly outlined in the Appendix. He also states that "the formula depends entirely upon the statement that the system including gearing may be regarded as an unbroken system." Our assumption that the system was an unbroken one was absolutely limited to the case where the gear is transmitting a torque sufficient to ensure that the gear teeth are continuously in contact, and Capt. Reed himself accepts this assumption when he states that he considered the shafting to be fixed at the gear-wheel end. To consider the shaft as fixed at the gear-wheel end eliminates any question of tooth clearance, and implies infinite inertia in the gears. That the inertia is infinite is only approximately correct, even if the turbine inertias are added to the gear inertias, and certainly there can be no question of adding turbine and gearing inertias unless the system is unbroken.

It is difficult to understand Capt. Reed's statement that the mass of the propeller shaft is not included in equation 3. This equation involves I_e, the expression for which is given in equation 2 and which includes I_s, the mass moment of inertia of the propeller shaft, and C_r, the rigidity of the propeller shaft, these factors depending directly upon the length and diameter of that shaft. The Authors

Mr. Tyacke considers the introduction of flexible shafts complicates the design of the gearing and gear boxes. Three separate gear boxes are employed, and each of these is smaller and simpler than the interleaved gear box, more easily assembled and more easily aligned. The attachment of the flexible shaft to the hollow pinion is made by a coupling of simple design, and nothing is introduced which would justify the charge of excessive complication.

Mr. Freeman refers to the use of larger shafts for the avoidance of trouble from torsional vibrations. The effect is to raise the critical speed, and this can be done to such an extent that it is above the working revolutions.

So far as our experiments have gone we have no indication that the torque variation at the propeller is much greater with the ship trimming by the stern than when on a level keel. It seems probable that in a vessel running in muddy shoal water the propeller-torque variation will be considerably greater than is usually the case. Such a condition would increase the unworkable range at a critical speed, but would make no noticeable difference to the running of the machinery at other speeds.

Mr. Freeman asks the effect of a pitch error of 0·005. Such an error would be sufficient to cause tooth separation over a range of about 10 r.p.m. at a critical speed with rigid gears, but would only cause tooth separation over a range of 1 or 2 r.p.m. if flexible shafts were used.

The end movement of the intermediate-speed shaft appears to be due to irregularities in gear cutting.

SOME FACTORS AFFECTING THE UTILIZATION OF HEAVY FUEL OILS IN INTERNAL-COMBUSTION ENGINES.

By HAROLD MOORE, M.Sc., Tech.

[Read in Newcastle-upon-Tyne on 1st December, 1922.]

The difficulties encountered in burning heavy fuel oils in Diesel engines and other types of heavy-oil engines may be divided into three classes, namely :—

(1) Ignition difficulties.

(2) Difficulties in burning the oil after ignition has taken place.

(3) Difficulties caused by the presence of comparatively small quantities of impurities.

These types of difficulties are quite distinct, and it is very common to meet with oils which, though entirely satisfactory as regards two of these categories, fail in the third, and are therefore unsuitable for use.

The heavy-oil engines may be roughly divided into three groups, namely : —

(1) Diesel engines; that is, engines utilizing compressed air for injecting the fuel, and in which ignition is obtained solely by the heat of compression.

(2) Cold-starting engines; that is, engines in which the charge is fired by spontaneous ignition, but in which the fuel is pulverized by mechanical means, without the use of blast air.

(3) Hot-bulb engines; that is, engines in which the fuel is injected by mechanical means, but in which external heat is applied in order to insure ignition of the fuel.

This classification is not in agreement with the manufacturers' names, as several large engines which dispense with air injection are usually termed Diesel engines, whilst these would class as cold-starting engines under the above-mentioned classification. The term semi-Diesel is used indiscriminately for both cold-starting and hot-bulb engines.

The capacity of an oil engine for burning heavy fuels is not merely dependent upon the cycle and method of fuel injection employed; it is also dependent to a very marked extent upon the size, speed, compression, fuel-valve timing and numerous other factors.

We will now treat in turn the difficulties encountered in using heavy fuel.

(1) *Ignition difficulties.*—In Diesel and cold-starting engines the ignition of the charge is effected by the heat of compression of the engine,

and it is therefore necessary that the temperature resulting from compression shall exceed the temperature necessary to bring about spontaneous ignition of the fuels. With the majority of engines the compression is adjusted so as to ensure certain and regular ignition under all conditions on petroleum residua and on gas oils (heavy distillates of petroleum). When it is desired to burn coal tar products difficulty is encountered in starting and low load running, as the heat of compression does not sufficiently exceed the ignition temperature. The temperatures of spontaneous ignition of various fuel oils were determined by the writer several years ago, and it was found that there was comparatively little variation amongst fuels of the same type, but great variation between the petroleum oils and the tar oils. The spontaneous ignition

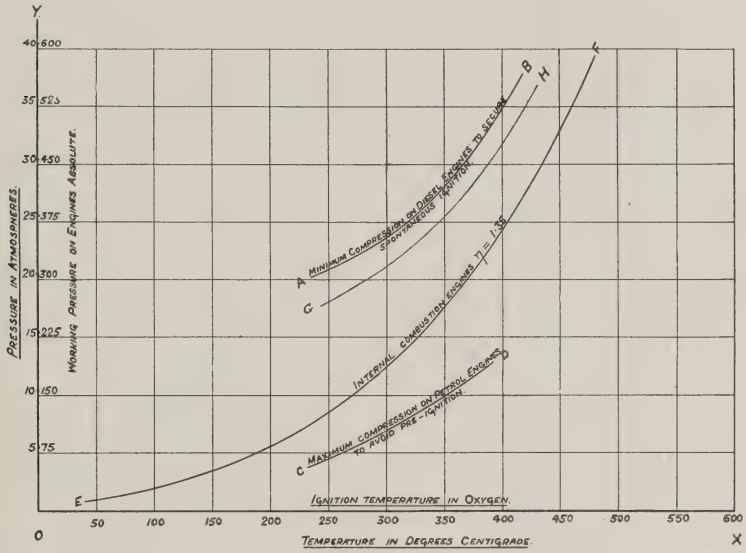

Fig. A.

temperatures of various fuels, determined in an atmosphere of oxygen were found to be as follows :—

Petroleum oils :

Petrol, 270° to 279° C.

Kerosene, 251° to 253° C.

Gas oil, 254° C.

Residual oils, 259·5° to 269° C.

Coal-tar oils :

Commercial benzol, 566° C.

Commercial Toluol, 516° C.

Creosote from gas-works tars, 415° to 473° C.

Creosote from low-temperature tars 290° to 350° C.

Shale oils : 251° C.

Animal and vegetable oils :

Whale oil, 273° C.

As a result of this work a curve indicating the limiting compressions for various fuels was prepared (*J. Inst. Pet. Tech.* April, 1920), and for the purpose of clearness this is here reproduced (Fig. A).

The ordinates and abscissæ in this diagram are pressures and temperatures respectively, and the line E F represents the temperature of compression calculated by the well-known formula,

$$T_2 = T_1 \left(\frac{P_2}{P_1}\right)^{\frac{n-1}{n}}$$

the value for n being considered to be 1·35, which is the approximate average value for water-cooled internal-combustion engines.

Now if the temperature scale be considered as the temperature of ignition of various fuels, it will be seen that in order to obtain ignition on a Diesel or cold-starting engine it is necessary that the compression pressure should exceed the corresponding pressure given by the curve E F. Therefore, the compression necessary for a fuel of any given ignition point must be such that when the two values are plotted the point must be above the curve E F.

In practice it is necessary to employ compressions considerably higher than the theoretical minimum, as the temperature of compression must be sufficiently high to ensure certain ignition under the most adverse conditions, such as starting from cold in winter, and must be sufficiently high to ensure ignition on engines which are not in good condition, and in which leakage takes place during compression. Also, a sufficient margin of temperature must be available to counteract the cooling effect of the blast. The lowest compressions that give satisfactory working with various fuels were found to lie roughly on the curve A B, which was constructed from experimental data.

Cold starting engines do not require such a large margin of excess temperature, as there is no blast air to cool the combustion space, and therefore the compressions may be slightly lower, or roughly on the line G H.

Engines operating on the constant-volume cycle compress an explosive mixture in the cylinder, and if the ignition point of the mixture is attained in the cylinder spontaneous ignition takes place. It is therefore necessary to employ a compression lower than that denoted by the curve E F. In practice it is found necessary to use a much lower compression to ensure absence of pre-ignition, as in very hot petrol engines the value for n exceeds 1·35, and it is also necessary to counteract the effect of local high temperatures, such as hot valve heads, plug points, etc. The maximum limit for petrol engines lies roughly along the line C D, but the exact limits are much influenced by the design of the engine and by the nature of the service which it is built to perform.

Recent researches on petrol-engine fuels have considerable bearing on the subject of heavy-oil-engine fuels, and are therefore worthy of particular attention. It is known that when high compressions are

employed in petrol engines the phenomenon of "pinking" is encountered. Pinking is not actually pre-ignition, but is an effect produced by the extremely rapid propagation of the flame when the temperature of the explosive mixture approaches, perhaps not very closely, the ignition point. In their investigations Midgeley and Boyd (*Ind. and Engineering Chemistry*, Oct. 1922, page 895) have found that many chemical substances exist which have the property of damping out the pinking effect in petrol engine fuel mixtures, one substance possessing 625 times the effect of benzol in this respect. By the use of such bodies the limiting compression may be raised. Though the addition of small quantities of these suppressors is very effective, it is found that further additions have comparatively little influence on the behaviour of the fuel.

In terms of our graph this means that the line C D may be brought closer to the line E F by the reduction of the velocity of flame propagation. Midgeley and Boyd also find that bodies exist that aggravate pinking by increasing the velocity of flame propagation. It is quite possible that by future research on these substances it may become possible to employ treated fuel which will operate satisfactorily with lower compressions than those at present necessary in Diesel and cold-starting engines.

The ignition of the more refractory oils does not present difficulty in the hot-bulb engine as the necessary temperature is easily available in the bulb. Any difficulties encountered in the use of the more refractory oils in this type of engine are more a question of burning than of the ignition.

The difficulty of obtaining firing on Diesel and cold-starting engines when running on oils of high ignition point has been, to a large extent, overcome by the use of pilot ignition gear, by which a small charge of easily inflammable oil is injected into the cylinder on each power stroke prior to the introduction of the main fuel charge. This arrangement necessitates the use of two fuel pumps. The consumption of the ignition oil (usually a petroleum distillate such as gas oil) is from 5 to 10 per cent. of the total fuel consumption. It is possible to burn tar oils on Diesel and cold-starting engines by using a petroleum or shale oil for starting, and changing over on to tar oil when the engine has warmed up. This method is only satisfactory when the load factor is high, as tar oil is liable to missfire on low loads. It is also advisable to increase the compression ratio slightly, and to advance the timing of the fuel valve. It is found that more satisfactory working is obtainable with tar oil if the first portion of the charge enters the cylinder *without pulverization*. For example, the burning of tar oil is more satisfactory if used with ignition gear when both pumps are delivering the tar oil than when a single pump is used. The only possible explanation of this phenomenon is that the preliminary charge of tar oil is not pulverized, and therefore avoids the cooling action of the blast. Devices have been designed with the object of

obtaining this action without the complication of using two pumps, one such arrangement tried by the Author being described in a paper read before the Diesel Engine Users' Association in May, 1918 (Fig. B). By this appliance a portion of the opening of the needle valve is "masked," except for a small port, which allows a very restricted charge to pass to the combustion space. Further opening of the valve allows the main charge to enter the cylinder. It has also been observed that old engines in which the needle valve has sunk into its seating by

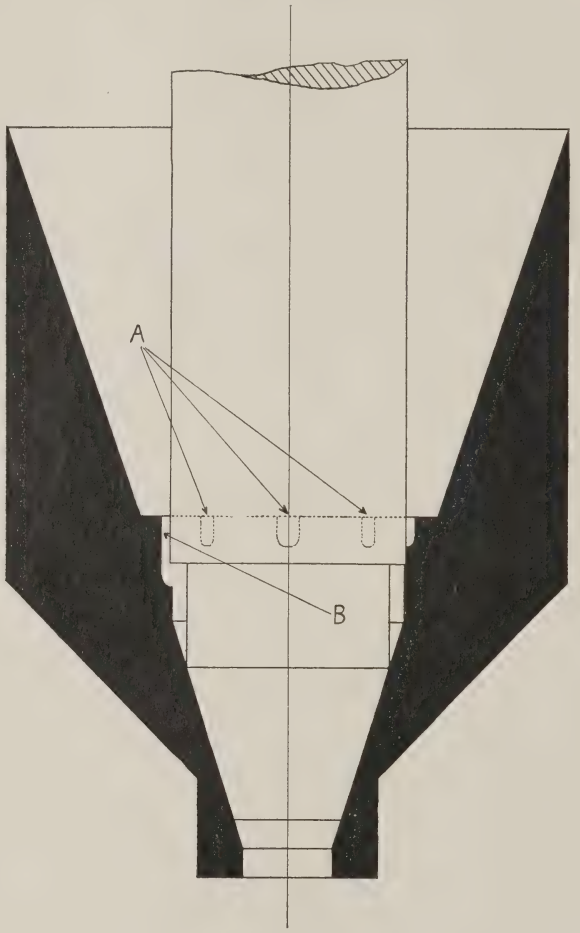

Fig. B.

repeated grinding-in work very satisfactorily on tar oil. This is undoubtedly due to the shoulder on the valve seat producing a "step" in the fuel delivery. In all cases where such devices are used it is necessary to employ a heavy advance on the timing of the initial charge: the main charge should enter the cylinder about inner dead centre.

There are a few oils at present on the market which come intermediate in ignition point between the coal-tar oils and the petroleum products. Oils and tars produced by the low-temperature distillation

of coal vary in their ignition points, depending upon the design of plant from which they originate, but are roughly intermediate between petroleum products and fuel oil produced by the high temperature carbonization of coal. If such oils are prepared in commercial quantities in the future it is probable that they will be burned in oil engines without pilot ignition gear.

(2) *Difficulties in burning the Oil itself after Ignition has taken place.*—When ignition has taken place the temperature in the cylinder increases, and the second problem is to ensure that the oil burns completely before the exhaust valve opens. In most cases the burning of the oil during the expansion stroke is a problem entirely separate from the ignition problem, but occasionally examples have occurred on Diesel engines when a feeble start has influenced the burning during the remainder of the stroke. The Author has known such examples to occur when the pulverizer settings are insufficiently restricted and the engine is running at medium loads with a low temperature of the circulating water outlet. The result is a smoky exhaust and a wavy expansion curve on the indicator card. The latter effect is liable to be incorrectly attributed to inertia of the indicator parts. Raising the temperature of the circulating-water outlet generally puts the matter right. Probably in such cases the irregularity of the combustion is somewhat similar to the "roaring" sometimes observed in Bunsen burners and in oil burners used for furnace heating.

When ignition has occurred the rate of the burning of the fuel is dependent upon a variety of factors, which are independent of the quality of the oil. Such factors are :—

(1) A high compression pressure gives a high cylinder temperature, which facilitates burning; also a high compression increases the concentration of the oxygen, bringing more oxygen to bear on each oil particle, thus again facilitating combustion.

(2) Large cylinder dimensions give slightly lower loss of heat to the circulating water, and thus increase the speed of burning.

(3) The presence or absence of blast air influences the temperature of the combustion space, and therefore affects the rate of combustion.

(4) The hot bulb increases to temperature of the combustion space.

(5) The temperature of the circulating water affects the temperature of the air in the cylinder.

(6) The degree of atomization is dependent both upon the nature of the oil and upon the mechanical arrangements in the pulverizing devices. Effective pulverization has the greatest influence on the rate of burning.

(7) The speed of the engine controls the time interval available for combustion before the opening of the exhaust. Therefore a speed of burning which might be too slow for a high-speed engine may give satisfaction on a slow-speed engine.

(8) The shape of the combustion space undoubtedly affects the rate of combustion.

Other factors connected with engine design and running conditions bear influence to a smaller degree.

As the substances present in fuel oils, apart from impurities such as ash and water, are all combustible, there is no question of "will they burn?" but only of "will they burn quickly?" The slow burning bodies present in petroleum and tar products differ in their constitution and have to be studied separately.

Crude petroleum oils consist largely of hydrocarbons, but also contain oxygen, nitrogen and sulphur containing bodies. Though the content of the latter three elements may be very small, they may be of great importance in the chemistry of the oils, as they may enter into the composition of a large portion of the oil. For example, an oil might contain only three per cent. of sulphur, but if that were present in the form of a compound of the formula $C_{20}H_{42}S$, which would contain roughly 10 per cent. of sulphur, it would mean that 30 per cent. of the entire oil consisted of sulphur-containing compounds, which might behave very differently from the hydrocarbon portion of the oil.

Apart from the liquid products crude oils also contain solid substances in solution, such as waxes and bituminous bodies. These may, or may not be hydrocarbon, and they may or may not be capable of distillation. The bituminous bodies cannot, however, be distilled without being broken up. As the fuel oils used in heavy oil engines are very frequently residual oils prepared by the partial distillation of the crude, the bituminous substances become concentrated during the preparation of the oil, and may be present in much greater proportion than they were in the original crude. The mixture of these bitumens is exceedingly complex and at the present time their chemistry is not fully understood. There is no absolute method for the analysis of these compounds, but it is usual to test oils by ascertaining the proportion of insoluble bodies which they yield when treated with various solvents. By experience it has been shown that certain solvents dissolve a comparatively small portion of a given oil, while others dissolve a greater portion, as some bituminous bodies are more difficult to dissolve than others. As a general thing the hardness and resistance to combustion of a bitumen are fairly closely allied with the difficulty of dissolving it.

The solvents usually employed are organic liquids such as mixtures of alcohol and ether, petroleum spirit, benzol, carbon disulphide, pyridine, etc. For the purpose of the present Paper it is only necessary to consider the action of two solvents, namely alcohol-ether mixture (1 to 2), and petroleum spirit. The majority of fuel oils are almost totally soluble in benzol, carbon disulphide, or pyridine.

The bitumens precipitated from an oil by the addition of an excess of the alcohol-ether solution are generally termed soft asphaltum, whilst

those precipitated by an excess of petroleum spirit are termed hard asphaltum. As a general rule it may be considered that the value obtained by the soft asphaltum analysis, that is to say, the portion insoluble in alcohol-ether mixture, is a larger value than the hard asphaltum figure, and contains the larger portion of the bitumens which are precipitated by the hard asphaltum test. That is to say, in dealing with fuel oils, the alcohol-ether value for asphaltum, though usually known as the soft asphaltum, contains both the hard and the soft together. This rule is not absolutely correct, as certain oils are known which yield hard asphaltum figures slightly higher than those for the soft asphaltum, but this is a very rare occurrence.

The bituminous bodies present in petroleum have two distinct effects on the combustion of oil in the cylinder. In the first place, their nature is such that on exposure to heat they readily form coke, instead of being distilled into a vapour, as would occur with the non-bituminous portion of the oil. This coke, being a solid substance, does not burn so rapidly in the engine, and may, under certain conditions, form deposits in the combustion space, and frequently causes smoky exhaust, which is accompanied by pitting of the exhaust valve in four-stroke engines. The second effect of bituminous bodies is that they increase the viscosity of the fuel, and therefore make it more difficult to obtain efficient pulverization.

The latter difficulty may be to a large extent overcome by the pre-heating of the fuel oil between the fuel pump and the cylinder; but there are limitations to pre-heating, as if the oil is heated to high temperatures it may be cracked, with the formation of deposits, and also a large proportion of the heat applied to the oil is lost during contact with the pulverizing devices before the fuel enters the cylinder.

Experience has shown that the bituminous bodies which are responsible for most of the trouble in oil engines are those bodies which are insoluble in petroleum spirit, that is, hard asphaltum. It may be that the real trouble is caused by some portion of the hard asphaltum, but until we have proof of this latter point it is only practical to consider all hard asphaltum as an undesirable constituent of oils. Until comparatively recently the most common test employed in the analytical valuation of fuel oils was the soft asphaltum test, but the behaviour of certain oils was not in accordance with their analysis by this test; for example, Argentine oils containing over 30 per cent. of soft asphaltum were observed to be not nearly so difficult to burn in both Diesel and cold-starting engines as Mexican oils of much lower soft asphaltum content. When determinations of both soft and hard asphaltum were made it was observed that the Mexican oils possessed much larger hard asphaltum contents than the Argentine oils, and that therefore this substance was probably responsible for the formation of carbon deposits in the engine, and for the difficulty of maintaining a clear exhaust.

The following figures explain the differences :—

Origin of oil:—	Mexican.	U.S.A. Mid-continent.	Argentine.	Texas.	Barbados.	Assam.	Egyptian.
Specific gravity ...	·966	·899	·942	·909	·970	·968	·959
Coke value ...	12·08	5·98	7·38	5·64	7·26	——	9·69
Soft asphaltum ...	20·58	4·75	23·9	5·8	5·80	23·99	19·55
Hard asphaltum ...	16·43	0·8	5·86	1·2	1·02	2·58	13·05

Though the presence of bituminous bodies increases the viscosity of oils very greatly, it does not raise the solidification point of the oil very seriously. The waxes, the main portion of which are pure hydrocarbons, and do not present any difficulty in combustion, possess the property of raising the solidification point, usually termed the "cold test" of the oils. This property of the waxes causing the oils to become solid at low temperatures, interferes with the settling of the sand and water, which are generally present to a greater or less degree in fuel oils. Therefore paraffin wax, though not directly affecting the behaviour of the fuel in the engine, is often indirectly responsible for other troubles.

Petroleum distillates, such as kerosene and gas oil, are free from bituminous bodies, which have been removed by the process of distillation, and they are therefore much more rapid in their combustion. As a general rule all petroleum distillates are satisfactory fuels for heavy oil engines. They are, however, more costly than the residual oils.

Fuel oils derived from shale oil are free from bituminous bodies, as only the distillates are marketed for fuel-oil purposes. The chemistry of the Scottish shale oils is very similar to that of many petroleum oils, and they present no difficulties in burning. The shale oils of high sulphur content, such as the oils derived from Norfolk shales (6 to 8 per cent. of sulphur) have not been tested in heavy-oil engines to the Author's knowledge, but being free from bituminous bodies he would not anticipate difficulty in burning them. Sulphur, as such, does not appear to have any effect on the engines: certainly no corrosive effects are noticeable on engines which have been running for many years on oils of high sulphur content. The sulphur is burned to sulphur dioxide and to sulphur trioxide, but the engine temperatures are too high to allow the formation of sulphurous and sulphuric acids by the combination of these gases with the water present in the exhaust gas. This condensation can take place where the temperature is sufficiently low, and the corrosion of long exhaust pipes and silencers is sometimes observed.

The coal-tar products burned in heavy-oil engines in this country are almost entirely confined to the distillates of gas works and coke oven tars; being distillates, these bodies are free from bituminous bodies and the so-called "free carbon." The difficulties encountered

in the burning of these bodies are practically entirely confined to the ignition problems already considered. At the present time very little of such oil is being consumed in internal combustion engines, as the prices are not favourable.

Raw coal tars have been utilized as fuel for Diesel engines on a large scale on the Continent. Their use in this country has been confined almost entirely to experimental work. The raw coal tars present the ignition difficulties usually encountered with coal-tar products, but in addition they contain bituminous bodies, and a solid substance present in an extremely finely divided state, known as "free carbon." This substance is not actually carbon, but on analysis is found to contain about 2½ per cent. of hydrogen and 4½ per cent. of oxygen and nitrogen. Experience shows that free carbon burns much more slowly than the other portion of the oil, and unless the engine conditions are entirely favourable, sparks will accompany the exhaust gases. The sparks are almost certainly burning particles of free carbon. This view is to some extent supported by the fact that when tars of high free-carbon content are burned, showers of sparks emerge if the indicator cock be opened. As might be expected, high free-carbon content is frequently the cause of pitting of the exhaust valves in four-stroke engines; two-stroke engines, exhausting through ports, do not suffer from this complaint.

As a general rule, the quantity of bituminous bodies present in a raw coal tar varies with the free-carbon content of the tar, and it is therefore more usual to value a tar by its free-carbon content than by the direct determination of the bituminous matter. The free-carbon content is analytically determined by dissolving the tar in a mixture of acetic acid and toluene and filtering off the insoluble portion. The analyses of tars vary very greatly, depending mainly upon the carbonizing plant from which they originate, and as a general rule they are much more difficult to burn than are petroleum products.

The average free-carbon content of a few typical tars are as follows:—

Vertical-retort tar, 5·2 per cent.
Inclined-retort tar, 17·0 per cent.
Horizontal-retort tar, 20·8 per cent.
Coke-oven tar, 10·4 per cent.
Low temperature carbonization tar, 1·0 per cent.

The main point of interest about the tars obtained by the low-temperature carbonization of coal is that these may become an important source of fuel for heavy-oil engines in the future. The better varieties of low temperature tar could be burned in Diesel and cold-starting engines with very little difficulty, as they are practically free from bituminous bodies and free carbon.

The distillates of coal tar frequently contain the solid hydrocarbons naphthalene and anthracene, which separate out from the oil in cold weather. These substances are quite good fuels, but their presence

prevents the settling out of ash and water from the oil, and is a source of inconvenience in handling the fuel during cold weather.

Both the free carbon and the bituminous bodies can, to a large extent, be precipitated from raw coal tars by the addition of a small quantity of a petroleum oil. This causes coagulation of the bitumen, which can be removed by settling and subsequent decantation. This process has been used on the Continent, and the residue of bitumen has been used for external combustion, usually under steam boilers.

(3) *Difficulties caused by the Presence of Comparatively Small Quantities of Impurities.*—The most common impurities present in fuel oils are water, sand and iron rust. Oil that has been delivered in barrels sometimes contains fibrous materials, originating from the wrappers used on the bungs of the barrels.

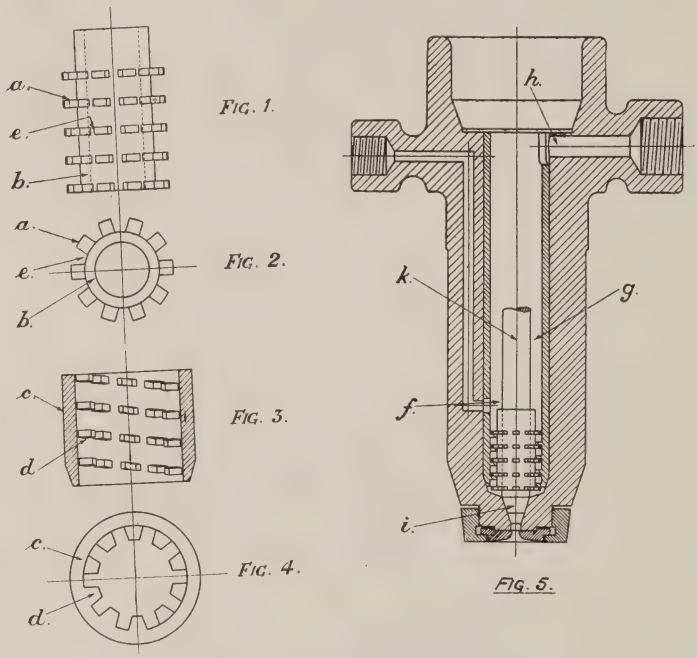

FIG. C.

Tar distillates sometimes contain sodium sulphate and sodium carbonate, resulting from their treatment in the tar distillery. The analyst determines these bodies by three tests. The water is determined by distillation. The ash value is obtained by burning away the oil, when inorganic substances, such as silica, iron oxide and sodium sulphate remain, and if desired may be subjected to analysis in order to determine the proportion of these various constituents. Organic material, such as leaves and the fibre from bung wrappers, are determined by the testing for mechanical impurities, when the oil is dissolved and the residue separated by filtration. The presence of water in a fuel oil is a very serious matter, as, being present in suspension, the proportion of water

present in each stroke of the fuel pump is irregular. Occasionally the pump will receive a full stroke of water, with the result that the engine missfires, and is overloaded on the subsequent strokes. Irregularity of firing causes a smoky exhaust, and, almost invariably, the lubricant becomes contaminated with tarry substances deposited by the half-burned fuel oil. This latter effect is most serious, as it interferes with the entire lubrication of the engine and may cause seizures of the pistons. Many troubles for which the quality of the lubricant is blamed are really caused by the incompletely burned fuel becoming admixed with the lubricating oil.

The ash content of fuel oil may have a most serious effect on the engine. Even small quantities of ash are serious, and in drafting specifications of oils required as fuels for internal-combustion engines it is usual to limit the ash content to 0·05 per cent. or 0·08 per cent. The ash formed by the combustion of a fuel oil is liable to accumulate on the cylinder walls, and becoming mixed with the lubricant it forms an erosive paste, which rapidly wears the piston and liner. Though the quantity of ash introduced into the engine during each stroke is very small, it will be found that quite a large amount passes through in the course of a few months' running. It is generally unnecessary to analyse the ash itself, as a measure of its erosive qualities can be made by rubbing it between the thumb and finger. The most common elements in ash are silica (from sand) and iron oxide (rust from pipes and storage tanks). The Mexican oils yield ashes containing vanadium oxide. The Author has examined flue dust from boiler installations fired on Mexican oils and found as much as 9 per cent. of vanadium present.

The organic mechanical impurities present in fuel oils are never present in sufficient quantities to effect the actual combustion of the oil, but they occasionally cause trouble by forming deposits in the pulverizing devices of Diesel engines. Cold-starting and hot-bulb engines are free from this trouble.

It is possible to prevent the choking of pulverizers by modification of the pulverizer design. Several devices will doubtless overcome this difficulty. One found very effective by the Author is the device shown in the accompanying sketch (Fig. C).

A cylindrical collar (Figs. 1 and 2) is fastened on to the fuel valve stem, and moves with the fuel valve. This collar has been cut by vertical rectangular slots, and has also had grooves cut round it in a lathe, in the manner shown in the sketch. This collar fits inside another collar (Figs. 3 and 4), which is fixed in the bottom fuel valve casing (see Fig. 5), and is therefore stationary. The outer collar is slotted internally to correspond with the external slots upon the inner collar, and in addition to this, is grooved in the other direction not quite horizontally, but in the form of a screw thread, so that an angle is formed by the horizontal and inclined grooves on the inner and outer collars respectively.

When this device is working, it will be observed that the obstruction to flow through the pulverizer is constant, whether the fuel valve is lifted or seated, but the direction of flow of the current is changing all the time during the period of fuel injection. The vertical grooves in both parts of the device are cleaned by the relative motion of the other parts. Such a device is, in the Author's experience, entirely free from the trouble of pulverizer choking. The formation of pulverizer deposits may not always be caused by mechanical impurities. In several cases the Author has been led to think them caused by oxidation of some constituents of the oil, which might form deposits in the presence of high-pressure air, *i.e.*, under pulverizer conditions.

Water, ash and mechanical impurities can all be removed from oil by allowing the oil to settle with or without the application of heat. As this is a somewhat slow process, it has been found advantageous with certain of the more viscous oils to employ high-speed centrifuges to remove the water, and the larger particles of ash and mechanical impurities. Mr. G. E. Windeler assures the Author that he has found the use of centrifuges most beneficial to the fuel oil when burning heavy Mexican oils on Mirrlees Diesel engines.

The presence of ash, water and mechanical impurities is not confined to any particular type of oils. Distilled oils are free from these impurities unless they have been accidentally added during the handling of the oil after distillation. The more viscous oils are generally highest in their content of these bodies, as high viscosity interferes with the settling-out process which normally takes place whilst the oil is in storage.

DISCUSSION.

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E.: Some little time time ago we had a Paper from Mr. Chorlton on internal-combustion engines in which he said that the Institution was regarded as not the least important with reference to the accumulation of data in its *Transactions* with regard to these engines. Even risking any criticism that may be made on this Paper, I will go as far as to say that we have added to-night to the valuable literature that we have already collected in connexion with Diesel engines by this admirable Paper on fuels.

Mr. Chaloner

MR. J. L. CHALONER, *Member*: The Paper which we have had the pleasure of listening to to-night is a further excellent proof of the many aspects that have to be considered in conjunction with the subject of internal combustion. It may be concluded that when a chemist analyses problems of an engineering character such a policy will act as an excellent stimulant for debate and from that point of view the Paper possesses an ideal feature by the scope which it offers for criticism and discussion. I shall confine my remarks to two sections of the Paper where I would join issue with Mr. Moore on the assumptions and assertions he makes.

Mr. Moore deals at length with the question of ignition temperatures and their importance in judging the suitability of a fuel for oil engines and he states that "the lowest compressions that give satisfactory working with various fuels were found to lie roughly on the curve AB, which was constructed from experimental data. Cold-starting engines do not require such a large margin of excess temperature, as there is no blast air to cool the combustion space, and therefore the compressions may be slightly lower on the line GH." It will be observed that Mr. Moore uses 1·35 as the exponential factor for his theoretical compression curve and whilst that value would be a very good average value for the Mr. Chaloner

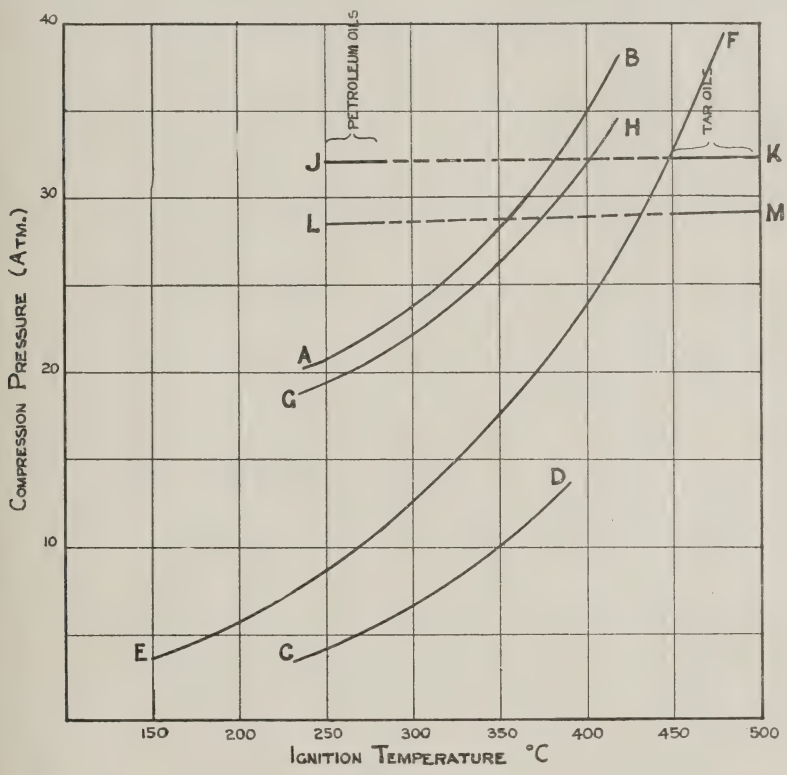

FIG. D.

exponential factor of both the compression and expansion curve, I consider that Eng. Commdr. Hawkes's figure of 1·27 is nearer the average value for the compression curve of oil engines. I desire to lay stress on this fact because it affects the characteristic of that curve and thus destroys to a certain extent the similarity that is alleged to exist between the theoretical curve and the curves AB, CD and GH. Mr. Moore with his curves constructed from experimental data uses a number of fuel oils of which he does not give us the grade or origin and this omission is regrettable because he tells us somewhere else that with regard to the ignition temperature there is comparatively little variation amongst the fuels of the same class but that there is a great variation between the

Mr. Chaloner

petroleum and tar fuels. Taking, for instance, the curve CD, and taking the ignition temperatures for a large number of petrols published by Mr. Moore, we find that the whole range falls between 250 and 270° C. for petrol derivatives and round about 500° C. for coal-tar derivatives. Mr. Moore makes no mention of fuels with a temperature somewhere round about 400° C. where his curve CD stops and it would be interesting to know first of all how he fixes the characteristics of the curve CD between 250 and 400° C., and secondly why he did not continue it to 500° C. and thus include benzols and such similar fuels.

Referring now to the curves AB and GH, similar arguments apply and I for one would like to know very much more about the nature of the tests and test results which enabled Mr. Moore to construct these two curves because, at any rate for the ordinary petroleum residual fuels and tar oils, my own observations do not agree at all with Mr. Moore's figures and follow more or less the line JK and LM, Fig. D. I would suggest on the strength of my own observations that Mr. Moore's suggested minimum compression pressures for both the Diesel engine and cold-starting engine are of no practical value as otherwise it would have to be inferred that all Diesel-engine manufacturers are using a compression pressure far above that necessary for ordinary working conditions. This conclusion brings one to the question of the practical value of ignition temperatures as determined with the ignition meter and I for one doubt whether the ignition temperatures as known to-day are any guidance in determining their ignition properties within the combustion chamber. As an instance I would like to draw attention to the ignition temperatures for alcohol and benzol. It is known, however, that alcohol will stand a higher compression than benzol when actually used in an engine. At the other end of the ignition-temperature scale it will be found that whilst shale oil and kerosene show practically the same ignition temperature they behave very differently when used in an engine.

The ignition meter has passed through a number of modifications and Mr. Moore's construction consists to-day of a solid block of metal into which is fitted the platinum crucible.

Mr. MOORE: Sometimes. It does the same without a crucible.

Mr. CHALONER: If a large block of metal makes a difference in the laboratory result, then it is clear that a very much larger difference may be expected inside the engine. Some experiments have been carried out quite recently which show that the ignition temperature approaches more closely the ignition temperature of gas formed during the vaporization and/or gasification of the fuel. It has been provided further that the ignition temperature does not depend so much on the quantity of the gas as on the quality.

My next point refers to asphalt, a word very much misused and which should be discarded. Mr. Moore refers to this constituent in different parts of the Paper as asphalt, asphaltum, bitumen and bitu-

minous bodies and the general confusion that appears to exist to-day makes me feel that before we discuss the effect of asphalt on the suitability of a fuel for an oil engine we should define this constituent and establish a definite method for determining its percentage. Mr. Moore put forward a method but in this connexion it is interesting to note that Dr. A. E. Dunstan in his paper on the "Standarization of the Testing of Petroleum and its Products" read before the Institution of Petroleum Technologists on October 10th, 1922, stated when referring to the subject of asphaltum, that "After discussion it was decided to defer making any recommendation about this test, as at present there is no definite information as to the amount of asphaltum permissible for oils for use in different makes of internal-combustion engines."

Mr. Chaloner

There is undoubtedly considerable difference of opinion on this point and I would like to ask with whom the responsibility lies. I have on many occasions drawn attention to the difference between specification and analysis, and I say that it is for the engineer to specify and for the chemist to see that the engineer gets what he specifies. I would suggest that support to this contention is given by Dr. Dunstan's above quoted reference.

The matter has now been still further complicated by the introduction of hard, soft and total asphalt and to show that Mr. Moore himself is not quite sure as to the relative importance of these three grades of asphaltum, I would like to compare his table with one published previously in conjunction with another very instructive Paper by Mr. Moore on "Some Characteristics of Petroleum Oils used on Diesel Engines" read on April 7th, 1922, before the Diesel Engine Users Association. The tables have been re-arranged to facilitate comparison :—

	D.E.U.A Table.				N.E.C.I.E. & S. Table.			
	Heavy Mexican Oil.	Egyptian Oil.	Fuel Oil ex Barbados.	Comodora Rivadavia.	Heavy Mexican Oil.	Egyptian Oil.	Fuel Oil ex Barbados.	Comodora Rivadavia.
Specific Gravity ...	·966	·959	·970	·942	·966	·955	·970	·942
Coke Value ...	12·37 %	10·48 %	6·22 %	7·38 %	12·08 %	9·69 %	7·26 %	7·38 %
Total Asphaltum	20·58 %	19·55 %	5·80 %	23·9 %	—	—	—	—
Hard Asphaltum	16·43 %	13·05 %	1·02 %	5·86 %	16·43 %	13·09 %	1·02 %	5·86 %
Soft Asphaltum	4·15 %	6·50 %	4·78 %	18·04 %	20·58 %	19·55 %	5·80 %	23·9 %

A study of the tables will I hope make clear my point that further information is required to elucidate this apparent confusion and misconception. In this connexion it is instructive to refer to Mr. Moore's own definition in his book on "Liquid Fuels for Internal-Combustion Engines" and there he states that "Asphalt is a black viscous substance often to be met with at the outcrop of petroliferous strata. Has probably been formed as a result of the evaporation and

Mr. Chaloner

atmospheric oxidation of exuding crude petroleum. A very similar product may be obtained by blowing air through the hot residues left in petroleum stills." It seems to me that on the strength of that definition it is absolutely futile to "determine" the asphalt percentage of an oil because, with an air-injection oil engine it is quite conceivable that asphalt is produced while the compressed air is atomizing the fuel charge. Asphalt is oxidized oil and I suggest that it would be more appropriate to refer to the constituent under this name.

In my opinion a matter of more importance is the question of impurities. When referring to heavy oils it is usually done in terms of the specific gravity although there are a number of oils of specific gravity 0·95 and 0·96 which are excellent fuels for internal-combustion engines. A heavy oil is judged much better by its viscosity, which is also a very good indicator of the percentage of impurities such as affect the behaviour of a fuel oil in an oil engine, viz., sulphur, asphalt and ash. Of these three it is I think agreed that the ash is that constituent which causes the chief trouble in oil engines. With heavy oils the suspension of mechanical impurities in the oil is more pronounced and average storage and distribution facilities are not ready means of filtering out the mechanical impurities. Reducing the viscosity with a view to facilitating the removal of suspended foreign matter means heating the oil. In order to keep the running charges at a minimum it is necessary to find that temperature condition which will with a minimum amount of external heat produce a maximum efficiency for the filtration operation. Mr. Moore has given 0·08 as the limit of mechanical impurities and it appears rather universal practice to restrict the permissible percentage of ash in a fuel oil to below 0·1.

Some tests have been carried out recently with a heavy fuel oil having a viscosity of 2,500 seconds at 100° F. and when using a centrifuge filtration device the percentage of mechanical impurities was reduced from 0·16 to 0·07 when filtering the oil at a rate of 25 gallons per hour at a temperature of 100° F., to 0·06 when filtering the oil at a rate of 60 gallons per hour at a temperature of 130° F., and to 0·055 when filtering at the rate of 90 gallons per hour at a temperature of 150° F. Centrifuging is, therefore, a practical means of eliminating the ash and if carried out on board prior to pumping the oil into the daily service tank would form a very effective means of making heavy oil suitable for use in internal-combustion engines.

My remarks have been made chiefly from an engineering point of view and whilst it may have been noted that these views do not coincide with the conclusions put forward by Mr. Moore, I submit my criticism without prejudice to my privilege to pay tribute to the most excellent work Mr. Moore has done in the way of chemical research on petroleum oils.

Mr. Keller

MR. K. O. KELLER: I was very pleased to read through Mr. Moore's Paper, and we should express our high appreciation for the valuable

information which it contains, particularly those members who are entirely concerned with the combustion of fuel in internal-combustion engines. One of the most important points that I was going to refer to, and to which Mr. Chaloner has already alluded, is in regard to the ash in the fuel oil. This is the greatest stumbling block in using the heavier grades of fuel oil such as Mexican fuel oil. Mr. Chaloner has said that it is for the engineer to say what he wants and for the chemist to see that he gets what he wants. Knowing that Mr. Chaloner represents an important oil company I trust that he will keep to his word and supply no more Mexican oil with ash, because neither the engineer nor the chemist wants it, and still less do they want to pay for it. Mr. Keller

About ten months ago I made extensive experiments on the third Doxford opposed-piston engine of 3,000 i.h.p., running on Mexican boiler oil of 0·945 s.g. The results were entirely satisfactory except for wear on the cylinder liners. The pistons were repeatedly withdrawn and invariably the piston face was covered with a dusting of fine powder which could be wiped off with the hand. The colour of the dust was either brown or lemon colour, and it felt very gritty, not unlike carborundum powder.

It occurred to me that this powder could not be the result of incomplete combustion—which in fact was all that could be desired—as it had no resemblance to carbon, but that it must be impurities in the fuel which the chemist calls "ash." I then made an experiment with a De Laval separator by passing about 10 gallons of this boiler oil through the machine. The analysis before and after treatment shows that the percentage of the "ash" was reduced to one half. This matter I then took up with Mr. Chaloner, who informs us to-night that a similar experiment he has made has given similar results. Thus, so far as it concerns the engineer the word "ash" is a misnomer, and would be better called "impurities" and as such it should be removed by the oil refiners just as they have to remove water—for obvious reasons. Once these impurities were removed there would be no hesitation in recommending to shipowners the use of Mexican boiler oil in internal-combustion engines.

The chemist calls these impurities "ash" because the only means by which he can determine them is by burning the oil in a crucible, and the residue is then called "ash."

I would ask Mr. Moore to enlarge upon one or two paragraphs on page 197. In heading (1) he says "A high compression pressure gives a high cylinder temperature." He probably means a high compression temperature, because a high cylinder pressure is just what we do not want.

Mr. MOORE: Yes; combustion-space temperature.

Mr. KELLER: I would also like Mr. Moore to enlarge upon item (8) the shape of the combustion space as one of the important features in an oil engine; it is very little realized that with big engines it is

Mr. Keller

of prime importance, because if particles of the air are lodged at any appreciable distance from the oil sprayer, you cannot get the amount of oil burned that you would if you could concentrate all the available air round the oil spray. I would like to know Mr. Moore's experience on this point.

The illustration of the fuel valves is very instructive, and from my own experience—not with these valves, but in general—I cannot but appreciate the value of the type of valves put forward.

To return to the question of fuel oil, I forget how Mr. Chaloner put it, but I would like to emphasize the point that, from the shipowners' point of view, it does not matter what kind of oil it is: what concerns him is £.s.d., and he buys that oil which he can get cheapest. Whatever the nature of the oil may be does not matter to him as long as he gets it 10 to 20 per cent. less than the oil prepared for Diesel engines. In this respect it is interesting to observe that a few ships we have running burn nothing but Persian oil. The shipowner calls it boiler oil because he pays boiler-oil price for it. In one of our ships 1,000 tons of oil was bunkered at 27s. per ton.

I will close by expressing my high appreciation of the excellent information contained in Mr. Moore's Paper.

Mr Belyavin

Mr. P. BELYAVIN, *Member*: Mr. Harold Moore's Paper is undoubtedly a valuable contribution to our knowledge of fuel burning in oil engines. I merely wish to add some remarks from my personal experience on the question of the wavy expansion line mentioned by Mr. Moore. In the old days this was usually attributed to the inertia of the indicator piston. However, several years ago I had an opportunity of investigating this little problem and the experiments I carried out convinced me that the inertia of the indicator had nothing to do with it at all. I obtained diagrams of the same cylinders running in the same conditions, one after the other, with several indicators of various make and size and with various springs. In some cases an extra weight was added to the indicator pistons, but the waves were still there and had approximately the same shape. In some cases the pressure dropped nearly as low as the compression pressure.

The engine on which these experiments were carried out was a quick-running four-stroke, with non-watercooled pistons. Analysis of the results of these tests showed that the temperature in some cases was not far from the temperature on the compression.

Considering these results, I came to the conclusion that the only possible explanation of this phenomenon was the alternate dissociation of the burnt gas due to high temperature and combustion when a favourable temperature is reached. The alteration in the temperature condition had, as far as I can remember, the greatest effect on this phenomenon, as Mr. Moore also mentions. Unfortunately, I do not possess the data of these experiments, otherwise I would have been very glad to produce them.

Mr. A. D. BRUCE, *Member*: Mr. Moore has indicated that heavy oils will be much more widely used in the near future than hitherto, thus increasing the range of fuels available for the internal-combustion engine. There are at least two reasons why his attitude in this matter is justified. First, referring to fuel valves provided with blast-air for effecting the atomization of the fuel, there is a great deal still to learn regarding the functions and proportions of the various parts as the following divergence in design shows. Fuel valves using blast air vary considerably in construction; the best known type is provided with a series of pulverizer plates, which offer considerable resistance to the passage of the fuel and air with the idea of thoroughly dividing the oil into small particles before reaching the combustion chamber. The throttling action which takes place makes this operation very inefficient, and a higher blast-air pressure than should be required is needed to pulverize the fuel. Mr. Bruce

The quantity of oil used is sometimes excessive on account of the poor pulverizing of the fuel, and under such circumstances considerable chilling of the charge in the cylinder takes place as a result of the greater expansion of the blast air. If an efficient pulverizer were adopted, combustion would occur more readily, especially at light loads, and the burning of the oil after ignition would be more complete, thus reducing the amount of deposits on the working surfaces of the cylinder parts. Another well-known type of valve has almost no resistance at all, the fuel being blown directly into the cylinder immediately the valve opens; both designs have the same objects to achieve, yet, the means of obtaining them are very different. It is, therefore, apparent that the laws governing the efficient pulverizing of the fuel are not well known, and a further knowledge of what actually occurs within the valve is very desirable, especially when adjusting the valve to deal with heavy fuels. Further, the mechanical separation of impurities by centrifugal action is especially useful, and would bring many oils within the scope of the Diesel engine which are at present debarred on account of their mechanical impurities.

The usual process now adopted on board ship for dealing with lubricating oil and its impurities after use is very efficient, and could easily be applied to heavy oils. The adoption of centrifuges would enable the elimination of such matters as sand and water, and if heating is necessary to reduce the viscosity of the oils before treatment, the small cost and trouble incurred would be well worth while.

The problem of dealing with heavy fuels for internal-combustion engines has been simplified by the more accurate basis set forth in the Paper. The information is also in a form suitable for reference, and although perhaps largely known by chemists, is invaluable to the engineer who may be called upon to choose suitable oils and consequently undertakes a serious responsibility.

Mr. Dalgliesh

VOTE OF THANKS.

MR. R. S. DALGLIESH, *Vice-President*: As a poor shipowner I have great pleasure in moving a vote of thanks to Mr. Moore. I have sat and listened to his Paper with a good deal of interest, but I never find these wonderful theoretical experiments and analyses existing on board ship. The engineer builds ships and he tells us that we should use so much oil. That does not always come out the same way as he says. We complain to the various oil companies that we find sand and water in the oil as has been mentioned here to-night, and they tell us that they cannot understand it; they have been using this oil since the days of Noah, and there must be something wrong. I think they must have put one of these samples before Mr. Moore, because I remember, in reply to one letter of complaint which we sent, they said that the oil must have picked up some dirt in course of transit. Mr. Moore has just used some similar words, and that was one of the latest excuses for a sample of oil which did not come up to the standard. I do not know if you can suggest a way to get over the difficulty. We have to pay for this sand and water; the combinations of these oil companies make us pay cash down, and we never get a rebate for this sand and water. During the last two years we have found a great difference in the qualities of the oil we burn, and I have been burning all the oils that Mr. Moore has mentioned here. This is a very important point for both chemists and engineers to remember. Only the other day one of our boats was chartered by an oil company, which had to admit there had been a lot of sand in the oil, and yet the oil had actually come from their own mines in Mexico. Now that is a serious thing, because we have to pay for it. But in that instance the oil company had to pay for it themselves, and they could not understand it. Now, it is impossible for an engineer to get over that difficulty on board ship unless you, gentlemen, can tell him how. However, I am sure you will all join in giving Mr. Moore a hearty vote of thanks.

The vote was carried with acclamation.

Mr. Wans

CORRESPONDENCE.

MR. OSWALD WANS, *Member*: The efficient burning of heavy fuel oils, in either air- or airless-injection engines, is dependent upon complex conditions which in some respects are antagonistic. Mr. Moore has dealt in an interesting manner with three important factors.

The requisite compression pressure is of great importance, and, strictly speaking, should be varied according to the properties of the fuel used. In practice, however, petroleum fuels may be generally classed as one in this respect, but, as stated by the Author, tar oil demands a higher compression pressure to effect regular ignition.

The curves, Fig. A., are interesting, but must be considered as purely theoretical. Actually, considerably higher compression pressures are necessary than shown by curves AB and GH. Nevertheless, a knowledge of the spontaneous ignition temperature in oxygen is of importance to the designer provided he knows its relationship to the actual temperature required in practice. In the curves, Fig. A., the Author has provided for a temperature margin, but to ensure reliable firing under the adverse practical conditions enumerated by him, appreciably higher compression pressures are necessary than given by the two top curves, if ignition is to be solely dependent upon the resulting temperature. For example, a residual oil having a spontaneous ignition point in oxygen of, say, 260° C. would, according to the curves, require an absolute compression pressure of about 260 lb. per square inch for an airless-injection engine, and 320 lb. per square inch for a Diesel engine. With such pressures, efficient and reliable firing is not possible. Generally, when using average residual fuel oils, it will be found that the actual compression pressures in airless-injection engines and Diesel engines must be about 50 per cent. higher than the pressures given by curves GH and AB corresponding to absolute compression pressures of about 425 lb. and 490 lb. per square inch respectively. Mr. Wans

With regard to the Author's statement that the ignition of the more refractory oils does not present difficulty in hot-bulb engines, whilst this may be true when running at about the working load, it does not apply at light loads, for the bulb becomes comparatively cold and the firing erratic and even dangerous.

The fuel-valve nozzle, Fig. 5, is simple, but the Author does not say whether the results were successful or not, and any information upon this point would be appreciated.

It is not clear why better results should be obtained with non-pulverized tar oil, and the writer (Mr. Wans) suggests that any improvement, when working under the conditions stated by the Author, is due to the absence of the cooling action of the blast, and not to non-pulverization.

Many types of fuel-valve have been devised with a view to dispensing with the pilot oil pump, and some have been more or less successful from half to full load. It is obvious that by efficiently atomizing or pulverizing the charge combustion is assisted, but there is a limit to what can be done in this direction. For the regular ignition of tar oil, it is essential that the temperature within the cylinder at the time of injection be sufficiently in excess of the spontaneous ignition point of the fuel, and unless this condition exists, correction cannot be made by improved atomization. The requisite temperature can be established by raising the compression pressure to, say, 600 lb., but for practical reasons, is not desirable. In the writer's opinion, the only satisfactory

Mr. Wans

means so far devised is pilot injection, and this applies equally to the air- and airless-injection engines. With a suitably arranged gear, an engine will run sweetly at all loads and without having to raise the compression pressure.

The pilot oil need not necessarily be a light petroleum distillate, thoroughly satisfactory results being obtained with fuel oil up to about 0·92 s.g.

The Author's remarks with regard to impurities in fuel oil are very interesting, and the use of centrifuges would appear to be a convenient and efficient way of getting over the troubles experienced due to this cause. Mechanical-injection engines are freer from the troubles arising from impurities in the fuel, probably because the atomizer is hotter than the air-injection fuel valve, and that obstructions are more readily removed owing to the positive action of the fuel pump.

Mr. Brown

MR. JOHN S. BROWN, *Associate Member*: It is evident that the present laboratory tests are not, by themselves sufficient to classify the different grades of fuel oils with regard to their suitability for use in internal-combustion engines. Thus, even in the comparatively simple matter of ignition temperatures, the figures given in this Paper differ from those given in the 1921 Report of the National Physical Laboratory. Progress in engine design is constantly broadening the range of suitable oils; but there is a distinct call for some new types of test which more closely represent actual engine conditions.

Thus it appears, from a Paper recently read before the American Society of Automobile Engineers, that the apparatus described to this Institution in May, 1921, by Mr. Tizard, has given information of fundamental importance in connexion with the ignition and combustion portion of the cycle in explosion engines, *i.e.*, engines of the petrol type. Perhaps it will be too much to hope that this work will be extended to cover the heavier oils, with both solid and air injection, but until this has been done information can only be gathered from experience with actual engines.

In the American Paper we learn that the cause of detonation has been traced to, chiefly, the proportion of exhaust gases present in the charge, and not, as was suggested earlier, to the compression temperature. This must be regarded as a very satisfactory piece of work as it at once explains the detonation difficulties, dealt with by Commdr. Hawkes, in connexion with the experimental Doxford engine, as the very complete scavenge obtained in this type of engine would appear to make it particularly subject to detonation. Doubtless some modification of the fuel-injection arrangements can be obtained which will eliminate this difficulty; but it is well to know that the carrying over, from cycle to cycle, of a certain proportion of the exhaust gases may be a desirable feature.

Little reference is made in the Paper to the question of coke gathering on the combustion-chamber surfaces. No engine can be regarded as

satisfactory which requires to be opened up for cleaning at more or less frequent intervals. While it would be preferable so to control the fuel injection that combustion was complete before the oil particles could reach these surfaces, such may well be impossible: indeed, in the hot-bulb engine, which is here being particularly referred to, a necessary feature is the injection of the oil against a metal surface. The temperatures attained by the metal can now be regarded as fairly well known, and it appears that an essential test for such oils should include an investigation of the minimum temperature required to give complete combustion. In a recent paper describing the Scott-Still engine some tests on these lines are dealt with and the temperatures ranged from 680° to 860° F. Thus an engine which runs clean on one oil may well be unsatisfactory when a change is made in the grade of fuel. Mr. Brown

Some recent work at the Royal Technical College, Glasgow, shows that, when estimating "hard" asphalt by petroleum-ether, the result, for a certain oil, varied from 9·2 per cent. to 6·5 per cent. depending on the boiling point of the spirit used. A close definition of the quality of the spirit is therefore required to make this test of any value.

Mr. H. J. YOUNG, *Member*: On page 200 of Mr. Moore's Paper appears a Table showing the coke values together with the percentages of soft and hard asphaltum in various types of oil. These percentages of asphaltum were given, in the majority of cases, to the second place of decimals. I have yet to discover methods of analysis of sufficient accuracy and reliability to give the amounts of soft and hard asphaltum to the second place of decimals. Indeed, it has rather been my experience in the case of high-asphaltic fuels to be glad to obtain results with an apparent accuracy within one or two per cent. Are the figures to be taken seriously? For instance, has the Mexican oil (as shown) been *proved* to contain 20·58 per cent. of soft and 16·43 per cent. of hard asphaltum? My own experience leads me to expect that if a sample of oil of this type were sent round to half-a-dozen laboratories one would get half-a-dozen results. The point may or may not be a minor one, according to the point of view, but in any case, when a reliable chemist gives results to the second place it is generally accepted that these results are accurate to, say, 0·05 per cent. Mr. Young

In the past I have corresponded with Mr. Moore on this point and Mr. Moore has been most kind and helpful. Nevertheless I have not, at this juncture, any evidence of the existence of an accurate method for the estimation of hard and soft asphaltums. I am inclined to think the question of hard asphaltum has been exaggerated. It has ever been a matter of amazement to me to hear the engineering section of the community discuss with great confidence and certainty the effects of this or that amount of hard asphaltum. To my mind there are grave doubts, firstly, whether any definite substance called hard asphaltum actually exists; secondly, whether it has ever been accurately estimated, and, lastly, whether the phenomena attributed to its presence are proved to

Mr. Young

be actually due to it and not to some other property or condition of the oil or of the engine in question. I have met several practical engineers versed in internal-combustion-engine practice who hold these views. For my part, I do not profess to know and raise the point merely to obtain the opinion and experience of others.

Mr. Onions

Mr. ROBERT ONIONS (Messrs. Ruston & Hornsby, Ltd.): The following are a few points that occur to me on reading over the Paper :—

(1) Does the Author consider that blast air improves the composition of the fuel? From certain experiments it would appear that, provided the nozzles and sprays are correctly designed, solid injection gives just as good results and in many cases better consumption.

(2) Referring specially to his remarks on pulverizers, it would be interesting to hear whether the Author can fix a velocity at which the fuel should be driven into the cylinder in relation, say, to the piston speed. Or does he consider that the highest velocity gives the best results providing, of course, that the nozzles and sprays are of normal design?

(3) From actual experiment it has been shown that the whirling action of the fuel set up by the design of nozzle, such as suggested in the Paper, gives better results in the engine, epecially for high-speed work, thus corroborating what the Author has to say with reference to this mode of introducing the fuel.

AUTHOR'S REPLY.

The Author

Reply to Mr. Chaloner.—The curves AB, CD, and GH are not intended to give a definite measurement of the compression limit. In the course of the Paper I have enumerated many factors that influence ignition, and other factors that influence combustion. As the only points referred to in the curves are temperature and pressure it is quite obvious that the curves can only be roughly applicable, as various sizes of cylinder, various speeds, etc., all affect the position of these lines. The curve AB was constructed from data from a 50 b.h.p. Mirrlees engine. The object of these curves is to attempt an explanation of the theory that governs limiting compressions. The curves are not intended as direct measurements for use on various engines for the reasons already stated. A different figure for the exponential does not affect the theory. The figure of 1·35 used for the construction of the curve was provided by the designer of the engine in question, and had been determined from the compression curve on the indicator card. Naturally the compression employed on Diesel engines is usually above the limiting compression. Diesel engine manufacturers employ a compression generally considerably higher than that necessary for obtaining ignition, which may be quite different from the compression necessary for ordinary working conditions. Though there are few commercial fuels that come intermediate in ignition point between petroleum and the coal-tar-

derivatives, for experimental purposes several fuels of intermediate ignition point were available, particularly petroleum oils of high aromatic content and the products of low-temperature carbonization. The Author

The examples of alcohol and benzol and their compression limits on petrol engines give rise to another point. I have some doubt whether alcohol will withstand a higher compression pressure than benzol when both bodies are perfectly pure. Alcohol affects the velocity of flame propagation of an alcohol-petrol mixture, and thereby allows the use of high compressions to a greater extent than does benzol when benzol is used in the form of a benzol-petrol mixture. The influence of the velocity of flame propagation on petrol-engine fuels is very marked. This is shown by the experiments of Midgley and Boyd. It is, however, to be observed from Midgley and Boyd's own statement that, though anti-detonating compounds which reduce the velocity of flame propagation will lead to an increase in the compression ratio employable, it is very notable that though a very small amount of these bodies had a very considerable effect the addition of further quantities has a practically negligible effect. In other words, this means that the pinking, which is not pre-ignition, can be avoided by the use of anti-detonators; but when the compression temperature approaches the temperature of the ignition point, anti-detonating compounds do not exert any further influence, as true pre-ignition by spontaneous ignition is liable to take place.

If Mr. Chaloner will read my original Paper read before the Institution of Petroleum Technologists he will find that various sizes of blocks and various materials, such as silica, platinum, porcelain, iron, etc., were employed in the experiments, and found to have practically no influence on the results. At the same time, of course, the ignition points are not supposed to correspond even approximately to those existing in the engine. It is only assumed that one is a continuous function of the other.

I do not agree with Mr. Chaloner's statement that the drafting out of fuel-oil specifications should be left in the hands of engineers instead of in those of chemists. In the course of my daily work I am continually having put before me lubricating- and fuel-oil specifications drawn up by engineers. Fully half of the specifications that I see demand properties in the oils that no oil has ever possessed, and which probably no oil ever will possess. It is no use specifying properties that are desired for a certain engine if such oil does not exist. The most important point in drawing up a specification is to know the analyses of all the oils available, and to know the quantities and price of such oil available. This knowledge only comes to the chemist who is continually employed on oil work, and by far the majority of engineers have neither time nor opportunity to make a study of the world's oil supplies and an analysis of every particular type of oil.

There are two methods of determining asphalt, employing different precipitating media, as explained in my Paper. The soft asphaltum

The Author

is obtained by the alcohol-ether method, and the hard asphaltum by the petrol method. Naturally the soft asphaltum, precipitated by a solvent that dissolves a smaller portion of the oil than does petrol, gives a precipitate that usually contains the major portion of the hard asphaltum, plus a substance that is soluble in petrol, but insoluble in alcohol-ether. The usual procedure is to call the total precipitate by each method hard and soft asphaltum respectively. In the paper read before the Diesel Engine Users' Association, the soft figure was called total, whilst the difference between the hard and soft was termed soft, this method of naming them being referred to in the paper, and it was explained that this was not the regular method.

The difference between the two Egyptian oils given in the two Papers is accounted for by two analyses of oils from the same source. It would be quite incorrect to refer to the asphalt present in fuel oils as "oxidized oil," as some of the asphalts are oxidation products; but a large quantity of asphalt, most noticeably that of the Mexican oils, is a thio-asphalt, and contains large quantities of sulphur. By the treatment of oil by either sulphur or oxygen either thio-asphalts or oxy-asphalts can be produced. If Mr. Chaloner's suggestion of referring to asphaltum as "oxidized oil" were adopted, I presume that the heavy Mexican oil would have to be considered non-asphaltic, as the asphalts in it are of sulphur base.

I cannot understand Mr. Chaloner's reference to judging a heavy oil by its viscosity, as such oils as the Comodoro Rivadavia Company, which burn satisfactorily in Diesel engines, are practically solid at ordinary temperatures, whereas the Mexican oils, which are much more troublesome to burn, are of much lower viscosity. Also, the sulphur content and ash content have very little effect upon the specific gravity—particularly the ash content. An oil may be quite unsuitable on account of its ash content when it contains only 0·1 per cent. of ash, which is too small a quantity to affect the gravity to any appreciable extent. The specific gravity bears no direct relationship to the sulphur content. The use of centrifugal methods for separating the ash from the oil, and also for the removal of water, is, of course, of the greatest value.

Reply to Mr. Keller.—The factors influencing combustion have been enumerated. It may be assumed that a high-compression pressure gives a high cylinder temperature, assuming all other variables to be constant. Of course, what we are wanting is a high-compression temperature, but if all other variables are constant the temperature will be a function of the pressure. There is no doubt that the shape of the combustion space is one of the most important points, and that the use of high pressures is undesirable for practical reasons. It is of the greatest importance to get the maximum possible temperature at any given compression: the shape of the combustion space decides this point. The use of a combustion space giving the minimum amount of cooling area for a given volume is of the greatest importance as this gives a higher

temperature at a given compression than can be obtained by a combustion space of which the cooling area forms a greater proportion. The shape of the combustion space also affects the accessibility of the air to the oil spray; that is to say, it decides the proportion of the air that can be burned. It is seldom realized by oil engineers that of the air pumped in on the suction stroke only half is used in practically all Diesel and semi-Diesel engines. On engines running at 100 lb. per square inch mean effectives pressure tested some time ago by the Author, it was found that the exhaust-gas analysis corresponded with 120 per cent. of excess air; that is to say, for every 100 cc. of air required for combustion 220 cc. had been drawn in on the suction stroke. There is enormous room for improvement in these figures not only improvement in over-all thermal efficiency, but also in power output per unit of volume. The Author

Reply to Mr. Young.—Regarding the experimental error in estimating hard and soft asphaltum, the methods are fairly accurate when conducted in the same laboratory. As, however, there are several methods of determining these values, if samples are sent to various laboratories the results are liable to vary to quite a large extent. Tests run on different oils under identical conditions with a view to matching will give an experimental error of about 0·05. If the tests are not run alongside the error is greater, and if different methods are adopted the error is very much greater. Mr. Young is somewhat sceptical about the troubles experienced being really due to hard asphaltum. Personally I am fully convinced from engine experience that this statement is correct. It may, of course, be due to some bodies present in the hard asphaltum, but this does not alter the fact that the difficulty of burning an oil is found by experience to be proportional to the hard-asphaltum content.

Reply to Mr. Belyavin.—I am very interested to receive Mr. Belyavin's confirmation of my experience regarding wavy lines on the expansion curve of the indicator card. The first time I noticed this phenomenon I was somewhat troubled, and asked the advice of Mr. Kimber, who had had much more experience of Diesel engine trials than myself. He promptly closed down the supply of cooling water, allowing the engine to warm up, when the expansion line on the indicator card smoothed out immediately. Since that time I have observed this difficulty many times, and there is no doubt that it has nothing to do with the indicator inertia. I do not think it is due to dissociation, but think it is the same phenomenon that is often observed on a Bunsen burner when the mixture contains a large quantity of air. It is also often observed on oil-fired boiler installations, particularly on Lancashire boilers, when the flame travels to and from the nozzle producing a roaring sound.

Reply to Mr. Brown.—There is no doubt that exhaust gases left in the charge of an explosion engine would tend to prevent detonation, but at the same time no detonation of this type can take place in a Diesel or

The Author

cold-starting engine. Detonation is entirely an effect of the velocity of flame propagation through the mixture immediately after the spark. True detonation cannot occur in a Diesel engine, as the cylinder is not filled with unburned mixture at any period of the cycle, and also no spark is employed. A very good account of the action of detonation will be found in Midgley & Boyd's Paper. The liability to coke gathering on the combustion chamber surfaces is generally proportional to the content of hard asphaltum.

Reply to Mr. Wans.—The curves AB and GH are supposed to represent the limiting figures. They are not supposed to be practical working figures, as it is well known that the pressure that will just bring about ignition is not satisfactory for general use on an engine, somewhat higher compression assisting in the actual burning.

Regarding the apparatus shown in Fig. 5, this undoubtedly had the effect of improving the ignition on a Diesel engine, but the Author does not consider it a practicable means of attaining this end, as it is so very difficult to attain correct timing and the correct size of the port, and there is little doubt that the same object could be attained by a more satisfactory method. The diagram is merely given as showing a certain method of obtaining the result. I entirely agree with Mr. Wans's remarks regarding the benefit of pilot-ignition gear for burning the more refractory oils. In my experience this method has given every possible satisfaction. I have had a good deal of experience with this system on Diesel engines, and have found it to give the greatest satisfaction under all conditions.

Reply to Mr. Onions.—The lowest consumption that the Author has known have been obtained by employing mechanical injection, but in many cases the Author has found that air-injection engines have been able to consume heavier fuels. This may not be due to the method of injection, but is very likely due to the size of the engine, as it is usual to employ air injection on a larger size of engine than those employing mechanical injection. I have not been able to state any relationship between the velocity at which the fluid should be injected and the combustion speed.

NOTE ON THE PRESENTATION OF SHIP MODEL EXPERIMENT DATA.

By E. V. TELFER, M.Sc. (Dunelm.), *Associate Member.*

[Read in Middlesbrough on the 28th November (by special permission of the Council), and in Newcastle-upon-Tyne on the 8th December, 1922.]

It has become almost an annual feature in the discussions of the various naval architectural societies for some member to complain of the diversity of the presentation of ship model resistance results adopted by the different research workers in the subject. At first sight the reason for this is difficult to understand, for the monumental work of R. E. Froude contained in his now classic "Constant system of Notation" was produced sufficiently early* in the days of experiment tanks for every subsequent research worker to utilize the system if he wished to do so.

The aim of the constant system it will be recalled was to express the experimental results in such a form that they would have the same value in any consistent system of units and also be independent of the absolute size of model or vessel under consideration. To enable this to be accomplished Froude expressed all dimensions in terms of the unit U which was equal to the length of the side of the cube whose volume was equal to the immersed volume of the ship in cubic feet; and from this quantity dimensionless numbers expressing power, velocity and length were directly established. The fact that this basis unit is not international is probably one contributory cause to the non-adoption of the Froude system internationally.

Before discussing other reasons for the non-adoption of the Froude system let us consider what is fundamentally required of any such system and more particularly from the point of view of the practitioner rather than that of the experimenter and research worker. After consideration it should be conceded that any practical system should possess the following attributes:—

(1) The system should be dimensionless and all expressions adopted should have a common basis.

(2) The units forming this basis should consist of units either at present internationally recognized or most likely to be in the future.

(3) Expressions adopted under (1) should be as few and as simple as possible in order to be immediately available for practical use without much arithmetical unravelling.

* See bibliography.

(4) The basis adopted under (1) should be such that each expression assumes an obvious physical significance thus enabling a non-ambiguous terminology to be set up. This terminology should be decided upon from international philological considerations, all related symbols being mnemonically appropriate and above all, typographically simple rather than typographically unique.

(5) As emphasized in (3) the system should be moulded for the convenience of the practitioner. This condition need not prevent the experimenter from utilizing other systems in order to accentuate pictorially some particular phenomenon; but his final presentation should nevertheless be in terms of units most convenient for practical manipulation.

Before considering the various systems at present in use, the significances of the above conditions should be carefully envisaged. Condition (2) makes it imperative that the unit of velocity should be the knot, the unit of power, the metric horsepower, the unit of weight, the metric ton; and because of probable future international developments the unit of length should be the metre. It is doubtful whether a unit of resistance is necessary in a practical system because the calculation of resistance is merely an intermediate step in the calculation of power. In any case the unit of resistance should be identical with that of weight. Condition (1) as qualified by condition (2) requires that dimensionless expressions formed of the above units should all have a common basis and this can only be consistently obtained by referring velocity, power, and length to a single basis of displacement. With this displacement basis dimensionless expressions can be established which will represent the actual numerical values of the velocity power and length of a model whose volume displacement is one cubic metre or whose weight displacement in fresh water is one metric ton.

In the light of the above considerations let us review the various systems that have been proposed from time to time by successive experimenters. Here as a preliminary amplification it is necessary to recall that the universal custom in the past (with the exception of the Taylor method) has been to assume that the Froude law of comparison held for the total resistance of the model or ship; departures from some given basis length being taken into account by correction factors based upon the different intensities of frictional resistances at each particular length. Thus practically all systems of presentation have the Froude law as their basis, total power or resistance expressed in some non-dimensional form for some standard length being generally plotted upon a base of speed also expressed non-dimensionally. The Froude law can be, and has been established in many ways. The method given in the appendix to this Paper is based upon the late Lord Rayleigh's principle of dimensions, and although well known in hydrodynamical and aerodynamical literature, has not—to the best of the present writer's knowledge—been previously published in shipbuilding literature. No apology is therefore needed for its inclusion in this Paper.

Considering now the various expressions for the non-dimensional presentation of velocity we have first the Froude (K) and (L) which are $\cdot 5834 \frac{V}{\Delta^{\frac{1}{6}}}$ and $1\cdot 055 \frac{V}{\sqrt{L}}$ respectively: next we have $\frac{V}{\sqrt{L}}$ adopted by Taylor, Semple, Robertson, Schaffran and others and very widely used in practice. In British and American work L is expressed in feet and in Continental work in metres. Next we have Mr. Baker's (P) which equals $\cdot 746 \frac{V}{\sqrt{\phi \cdot L}}$, where ϕ is the prismatic coefficient, and is, of course, merely a modification of $\frac{V}{\sqrt{L}}$. Other modifications of $\frac{V}{\sqrt{L}}$ such as $\frac{V^2}{L}$ and $\frac{V^2}{g \cdot L}$ have been suggested amongst others by Dr. Gümbel. Finally we have the Continental modification of the Froude (K) and which equals $\frac{V}{\Delta^{\frac{1}{6}}}$. It is generally used in Italy, France and Germany.

Amongst all these alternative expressions only the last has the same numerical value in British and Continental units. The expressions of which $\frac{V}{\sqrt{L}}$ is the prototype lack internationality because of their length basis. The Froude (K) refers to the velocity of a one-cubic-foot model by introducing the constant 0·5834. Thus as complete internationality should be one of the attributes of any practical system the quantity $\frac{V}{\Delta^{\frac{1}{6}}}$ which represents the velocity in knots of a one-ton model (and incidently a very usual size of model) should be adopted. Following Scribanti (5) this expression may be conveniently termed the "Relative Velocity."

Consider next the various expressions for non-dimensional presentation of power or resistance. First we have Froude's (C) which equals $427\cdot 1 \frac{\text{E.H.P.}}{\Delta^{\frac{2}{3}} \cdot V^3}$. Next we have Taylor's $\frac{R}{\Delta}$ which expresses resistance in pounds per ton; also the Italian and French practice, which is identical with that of Taylor except that the resistance is expressed in kilograms per ton. Next, that used by Schaffran and others in Germany and which equals $\frac{\Delta^{\frac{2}{3}} . V^3}{\text{E.H.P.}}$, and also a new proposal of Schaffran, namely, $\frac{\Delta . V}{\text{E.H.P.}}$. More recently we have that proposed by Bragg and Sadler in America (but which as these experimenters themselves point out is not non-dimensional) namely $\frac{\text{E.H.P.}}{\Delta}$. Next we have the quantity $\frac{\text{E.H.P.}}{\Delta \cdot V}$ recently proposed by Dr. Achenbach and which is, of course, the reciprocal of the Schaffran proposal. Finally, we have the expression $\frac{\text{E.H.P.}}{\Delta^{\frac{7}{6}}}$ familiar to all in the well-known "extended law of comparison"

and which, although not used by any experimenter up to the present, was used by Mr. A. W. Johns in his 1907 Institution of Naval Architects paper on "Approximate Formulæ for Determining the Resistance of Ships."

Reviewing the merits and demerits of these several proposals from the standpoint that we have adopted and considering first the Froude expression, we find this was most probably specifically designed to flatten the power or resistance curve which is obtained, say, in ordinary full-size trials or with the use of the Taylor expression $\frac{R}{\triangle}$. Whilst it is thus convenient for the fairing of experimental results because of its flatness, yet there seems to be no important justification in practical work for not making the higher powers that are required at the higher speeds immediately visible to the eye of the designer. (C) has the advantage of being completely international, but in spite of this fact its Continental adoption has been very meagre and of recent years seems to have entirely passed out of favour. This disfavour is perhaps due to the arithmetical complication of (C).

The Taylor $\frac{R}{\triangle}$ and the corresponding Continental expression due to their extreme simplicity are very popular with designers. Each expression suffers, however, from lack of internationality and also from the fact that each represents a resistance instead of a power, thus introducing unnecessary subsequent arithmetical work.

The Schaffran $\frac{\triangle^{\frac{2}{3}} V^3}{E.H.P.}$ whilst international in its units is inconvenient for as the effective horsepower is in the denominator it cannot be split up to represent its various components. The expression thus suffers from the defects of (C) and has not this latter's advantages. The alternative Schaffran proposal $\frac{\triangle . V}{E.H.P.}$ whilst arithmetically simpler than the other, still contains the power in the denominator and hence is still inconvenient practically.

The Sadler $\frac{E\,H.P.}{\triangle}$ whilst undoubtedly simple, is not dimensionless, and thus cannot be recommended as a practical expression.

The Achenbach $\frac{E.H.P.}{\triangle . V}$ is probably without exception the simplest of all expressions and is, of course, completely international. It represents the power required to drive the one-ton model for each knot of its velocity, and is merely the Taylor expression in a disguised and more practical form. However, the appearance of V in the denominator imposes perhaps an unnecessary qualification upon the one-ton model

basis, and thus one is lead finally to the John's expression $\frac{\text{E.H.P.}}{\triangle^{\frac{7}{6}}}$. This expression is, of course, international in its units, and represents the horsepower of the one-ton model. Its basis is thus consistent with that of the "Relative Velocity" and it may be conveniently termed the "Relative Power."

As mentioned previously the foregoing expressions are all based upon the Froude law, and really should only refer to that portion of the total power absorbed by wave-making. The customary practice of taking the total horsepower, as following the Froude law and introducing length correction factors, has been more or less necessitated by the empirical nature of our knowledge of the subject of frictional resistance; but with the introduction of the Rayleigh or Reynold's law into naval architecture this coarseness of method can be considerably ameliorated. The Rayleigh law is deduced in the appendix to this Paper in a manner similar to that adopted previously for the Froude. It is there shown that for two similar vessels at corresponding speeds the frictional horsepower varies directly as $\triangle^{\frac{7}{6}-\frac{n}{2}}$ and adopting the value of n recently obtained by Dr. Gebers, *i.e.*, 0·125, it therefore follows that at corresponding speeds the quantity $\frac{\text{E.H.P.}_f}{\triangle^{\frac{53}{48}}}$ is constant. In the general case $\frac{\text{E.H.P.}_f}{\triangle^{\frac{7}{6}-\frac{n}{2}}}$ is constant and for any given form n can be determined from a knowledge of the resistance results of two similar models by a method outlined in the appendix. However, it is probable that it will not differ materially from the value 0·125 and thus, when the frictional power at any displacement and speed is known, the frictional power of the one-ton model at the corresponding speed is given by the expression $\frac{\text{E.H.P.}_f}{\triangle^{\frac{53}{48}}}$. Thus when, as is shown in the appendix, the wave-making and frictional components of a model's resistance are accurately determined, the relative wave-making and frictional powers of the one-ton model can be simply calculated and from these the total effective power required for any similar vessel can be directly obtained, the relative wave-making power being multiplied by $\triangle^{\frac{7}{6}}$ and the relative frictional power by $\triangle^{\frac{53}{48}}$. In a subsequent portion of this Paper dealing with terminology and symbols the wave-making power of a vessel and its one-ton model are respectively indicated by P_w and P_{rw}, whilst the corresponding symbols for frictional power are P_f and P_{rf}. Thus, from the above considerations we may write :—

$$P_w = P_{rw} \cdot \triangle^{\frac{7}{6}} \text{ and } P_f = P_{rf} \cdot \triangle^{\frac{53}{48}}.$$

With most other methods and generally (although not always) with the Froude constant system, a knowledge of the wetted surface of the vessel or model is necessary. With the present method this necessity

completely disappears, the above expressions being sufficient for all subsequent manipulation of the basis model results.

Finally, we must now consider the various expressions for the non-dimensional representation of length or any linear dimension. First, we have the Froude (M) which equals $0{\cdot}3057 \frac{L}{\Delta^{\frac{1}{3}}}$ and which is the length in feet of a one-cubic-foot model; next we have the French, Italian and German expression $\frac{L}{\Delta^{\frac{1}{3}}}$ which gives the length in metres of a one-ton model; next, comes the Taylor "fatness" ratio $\frac{\Delta}{\left(\frac{L}{100}\right)^3}$ which gives the displacement of a 100 foot model; and finally we have an expression

NOTES: (SUCH AS) REMAINING FORM COEFFICIENTS: ACTUAL LENGTH OF MODEL: LOWEST RELIABLE SPEED OF MODEL; RATIO OF ACTUAL MODEL DRAFT AND BEAM TO THOSE OE TANK; CONDITION AND NATURE OF MODEL; TANK WATER TEMPERATURE, DENSITY AND VISCOSITY, ETC.

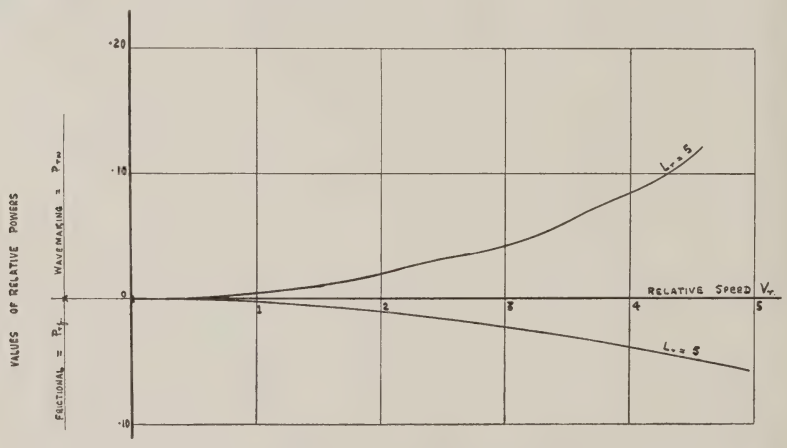

FIG. I.—SHOWING TYPICAL PRESENTATION OF MODEL EXPERIMENT DATA FOR USE WITH "RELATIVE" SYSTEM.

similar to the Taylor and used by Schaffran which represents the displacement of a ten-metre model and is thus $\frac{\Delta}{\left(\frac{L}{10}\right)^3}$

Of these expressions the only one representing the linear dimension of a one-ton model is the Continental expression $\frac{L}{\Delta^{\frac{1}{3}}}$. The fact that this relation expresses length in metres makes its value very nearly numerically equal to that of the length in feet of the Froude one cubic foot model. This expression may be used to express any linear dimension of the one-ton model. With the Taylor or Schaffran expressions beam and draft are expressed as the actual beam and draft of the 100-foot or 10-metre model respectively. As a displacement-length coefficient the Froude and Continental expressions are much

easier to visualize and more convenient to use because of their smaller range of value than the Taylor or Schraffran expressions. The Continental expression, which is herein adopted can be conveniently termed the "Relative Length."

Now, let us consider how the expressions selected from the foregoing deliberations lend themselves to diagrammatic treatment. Fig. I. shows the suggested diagram and represents the properties of the one-ton model. The value of the relative speed and power per merchant and passenger ship forms will rarely exceed 5 and 0·30 respectively. The relative wave-making power is set off above the base while the relative frictional power is set off below it. Experimental results set off in this form, it is suggested, offer the most convenient presentation for the needs of the practitioner. However, these relative values have yet to be simply convertible for any given displacement into actual horsepower and knots. The alignment-chart principle offers the simplest means

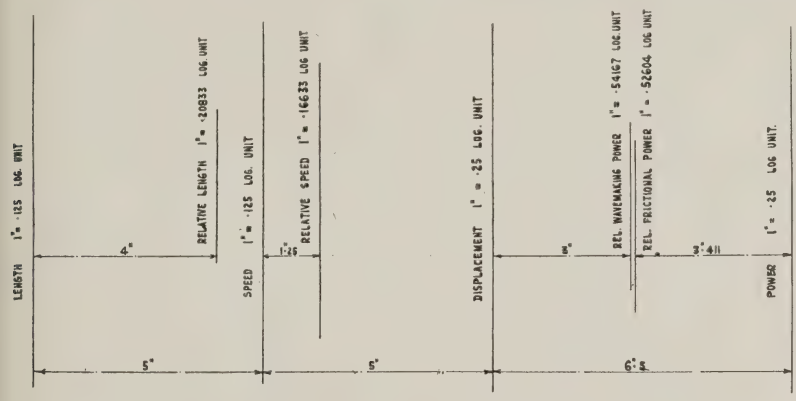

FIG. 2.—SKELETON DIAGRAM OF PLATE I.

of conversion and Plate I. gives such a chart from which any relative value of speed, power or length can be obtained in terms of the usual absolute British or Continental units.* The only portion of this chart calling for special explanation is that dealing with relative power. Because of the different laws of comparison of frictional and wave-making power represented by the respective equations—

$$P_f = P_{rf} \cdot \triangle^{\frac{53}{48}} \text{ and } P_w = P_{rw} \cdot \triangle^{\frac{7}{6}}$$

two relative-power scales must be provided. By joining any absolute power to any absolute displacement the connecting-line will intersect these two relative power scales at their correct respective value as represented by these equations. Conversely, by joining from a particular displacement through the particular relative powers obtained from such a diagram as Fig. 1 the correct frictional and wave-making horsepower are at once obtained. A convenient practical method of preparing an actual speed power diagram would be to trace the basis diagram and

* See Appendix 4 for method of construction of chart.

correlate the power and speed scales by using the respective alignment charts. This done the two power curves can be simply superimposed and the ordinary speed power diagram at once obtained. For the basis diagram it is probably clearer to keep the frictional power below the base and the wave-making above it. Moreover this arrangement can be said to emphasize the differences in these phenomena. These differences consisting of different laws of comparison, opposed effect of relative length, effects of temperature and surface roughness on the relative frictional power and not on the wave-making, are such as would make the suggested separation not only convenient but also desirable.

The foregoing treatment is restricted to the consideration of that portion of model-experiment work dealing with the presentation of naked model results. However, this portion is that to which Froude restricted himself in his Constant system and embraces practically the whole of the research results at present published.

Finally it should be an attribute of any system which endeavours to be at all international that not only units but terminology and symbols should be so chosen as to observe the philological principle of maximum internationality. The number of people likely to make use of any system such as is the subject of this note in the principal shipbuilding countries will be such that, in selecting a terminology, the practice and the language of the countries should be respected in the following order: British and American, German, French and Italian. Considerations of arithmetical internationality have produced a system which is not identical with any in actual extensive use in any one country but which is, however, in reasonably close agreement with most systems in all countries, and is perhaps nearer to the Italian system than any. In view of this fact the existing terminology of Italy should be examined from the point of view of its possible international adoption. These expressions as already used throughout this note were suggested by Scribanti, and are respectively the "Relative Power," the "Relative Speed" and the "Relative Length." In Table I. it is shown that these expressions can be directly translated into all of the languages considered, the term "Relative" being identical almost in spelling and certainly in significance in each. The presentation suggested in this note can thus be called the "Relative System." This nomenclature appears to be more appropriate and less in conflict with accepted interpretations of terminology than is that of the Froude. The use of the expression "Constant" in the Froude system requires considerable qualification in order to convey accurately its full and peculiar significance. This difficulty of interpretation is reflected in the fact that the Froude terminology has never been wholeheartedly adopted by Continental workers.

We must now consider the most appropriate symbols by which to characterize our expressions. Here we must be guided by mnemonic considerations as well as international practice. Any symbol chosen must be simple, concise and precise. It should involve no typographical difficulty. First we have power. The international unit of power is the horsepower and the fact that the word horse is brought into our terminology has completely upset internationality of symbols. Practically all countries have given undue preference to the horse instead of to the power. Thus we have for, say, the expression effective horsepower the English symbols E.H.P., which form, although precise, is not concise and international; the German symbols PS_e or N_e, the P in the former being derived from Pferde, meaning horse and not from power, and the N from Nutz, meaning useful and taken from the older German expression for effective horsepower, *i.e.*, Nutzpferdestärke, the suffix *e*, however, meaning effective being the newer expression Effective Pferdestärke. The Italian and French seem to prefer F and the former sometimes make use of the English expression H.P. The one common symbol amongst these various systems is P. It is mnemonic for power in all languages except German in which, however, it is mnemonic for horse. Thus, mnemonically it is a suitable symbol. Used alone it is rarely preferred by English workers, but incidentally it is used exclusively by Taylor in his propeller work. In English it can possibly clash with P the pitch of a propeller but Taylor avoids this by using p for pitch. It does not clash with naval architectural symbols in German and Italian but in French it does so with P used for "poussée," meaning displacement or buoyancy. The French, however, also use other symbols for displacement so this clashing is unimportant. Thus, we see that P should prove a convenient international symbol for the representation of power. To characterize the particular power the German practice of adding a suffix seems preferable to the English practice of using prefixes. Thus P_e, P_i, P_p, P_s, P_f, P_w, can be used almost internationally to characterize uniquely effective, indicated, propeller, shaft, frictional and wave-making horsepowers. Finally the respective wave-making and frictional powers of the one-ton model should be written P_{rw} and P_{rf}, the word relative being as previously mentioned international.

The next symbol to be decided upon is that for displacement. The American, Italian and German practice is to use D. The British practice is to use $\triangle$. The French seem to prefer either P or W in resistance work but occasionally $\triangle$ is used. The British practice has much to commend it, for the $\triangle$ symbol is precise and it relieves the symbol D of one of its many interpretations. Moreover, when inverted to ∇ it gives a happy distinctive symbol for volume which in turn leaves the symbol V free to represent speed. As it is mnemonic and fairly international it is probably the most convenient symbol available.

The symbol for speed can be quickly decided upon as V is used almost invariably in all countries. The relative velocity thus becomes V_r.

The symbol L for length is also completely international and thus the relative length symbol becomes L_r

The symbols for beam and draught are not so easy to allocate. The symbol B is used in British, American and German practice and the symbol *l* in French and Italian. As thus the majority use B it can be profitably adopted. The beam of the one-ton model can be characterized by the symbol B_r . The symbolizing of draught has always been a source of trouble in British writings. Generally we fall back upon the hard-worked D; American writers prefer H, German T, French *p* or *i*, and occasionally *t*, and Italian writers *i*. Undoubtedly we require another symbol in English and of the available Continental symbols probably the German T is most appropriate. It is mnemonic in French and of course German and is phonetically mnemonic even in English. T as a symbol is but rarely used in English : it is occasionally used for propeller thrust but not exclusively. Its reservation for draught is perhaps to be preferred and should be easily brought about. The draught of the one-ton model can thus be represented by T_r .

The various coefficients of fullness, such as the displacement, water-plane area, midship-section area and prismatic coefficients, are almost without exception internationally expressed as δ, α, β, and ϕ. All these symbols are both convenient and more or less mnemonic. The present British tendency to use such symbols as C_B $C_{W.L}$ C_M and C_P is thus hardly necessary. Although they are admittedly convenient, from an international standpoint they are hardy desirable.

In Table I. the terminology and symbols in the suggested relative system are grouped for ease of reference.

Conclusion.—In the foregoing pages an attempt has been made to examine as many existing presentations of model experiment results as possible in order to determine whether any one system possesses advantages over all others and especially for the needs of practice. The Relative System, proposed by Scribanti and as modified and extended by the present writer, is suggested as the most convenient. It is put forward as especially suitable for the ordinary presentation of model results and is moreover very convenient for ultimate conversion into such systems as that adopted by Taylor in presenting his well-known "Standard Series."

The chief advantage of the system lies in the fact that the one-ton model is the unique basis of the presentation. The conception of the one-ton model is being used causally or intuitively by most designers in their daily practice. Such expressions as deadweight, steel weight, etc., per ton displacement really refer to the actual deadweight etc., of the one-ton model. The "Weight Equation" as developed by Normand and Hovgaard is also a generalization of the constituent weights of a one-ton model. The Relative System, therefore, will enable the "Weight Equation" to be taken up as a more practical instrument by designers

TABLE I.—SHEWING SUGGESTED TERMINOLOGY AND SYMBOLS OF "RELATIVE SYSTEM" IN ENGLISH, GERMAN, FRENCH AND ITALIAN.

	German	French	Italian.	EXPRESSIONS.	
Relative System	Relativ System	Système Relatif	Sistema Relativo	SYMBOL	FORMATION.
Relative Power	Relativ Pferdestärke	Pouvoir Relatif	Potenza Relativa	P_{rw} and P_{rf}	$\frac{P_w}{\triangle^{\frac{7}{6}}}$ and $\frac{P_f}{\triangle^{\frac{53}{48}}}$
Relative Velocity	Relativ Geschwindigkeit	Velocité, Vitesse } Relative	Velocità Relativa	V_r	$\frac{V}{\triangle^{\frac{1}{6}}}$
Relative Length	Relativ Länge	Longeur Relatif	Lunghezza Relativa	L_r	$\frac{L}{\triangle^{\frac{1}{3}}}$
Relative Breadth	Relativ Breite	Largeur Relatif	Larghezza Relativa	B_r	$\frac{B}{\triangle^{\frac{1}{3}}}$
Relative Draft	Relativ Tiefgang	Tirant d'eau Relatif	Immersione Relativa	T_r	$\frac{T}{\triangle^{\frac{1}{3}}}$

N.B. P_w = Effective Horse-power (in metric units) absorbed in wave-making.
P_f = ,, ,, ,, ,, in overcoming friction
V = Speed in knots.
$\triangle$ = Displacement in metric tons, fresh water.

L = Length in metres.
B = Beam in metres.
T = Draft in metres.

and should assist in the unifying and crystallizing of designing methods and processes.

The Author ventures to hope that some of the contents of this Paper may be of interest and will possibly prove useful to both practitioners and experimenters alike. The Paper has been written in the first place to present the existing position of the subject both in this country and abroad; secondly, and chiefly, with the desire to promote a representative discussion which will thereby secure an authoritative and, if possible an international, consensus of opinion upon this—at times very annoying—subject of the presentation of ship model experiment results.

In conclusion the Author wishes to place on record his appreciation of the facilities accorded to him by this Institution in the preparation of this Paper—first indirectly, through his being elected to the Institution Scholarship in 1916; and secondly, directly through the vast amount of British and Continental information available to the student in the Library of the Institution.

APPENDIX.

The quantities entering into the problems of wave-making and frictional resistances together with their physical dimensions, are tabulated below.

Quantity.	Dimensions.
Velocity, v	$\frac{L}{T}$
Acceleration, g	$\frac{L}{T^2}$
Force, R	$\frac{ML}{T^2}$
Density, ρ	$\frac{M}{L^3}$
Kinematic Viscosity, ν	$\frac{L^2}{T}$

1. *Proof of the Froude Law.*

The resistance of a vessel due to wave-making may be expressed* in the most general terms as

$$R = f(\rho, l, v, g) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and whatever the actual formation of the right-hand side of equation (1) may be it must necessarily have the dimensions of a force.

Let equation 1 be expressed thus

$$R = \rho^a \cdot l^b \cdot v^c \cdot g^d \quad . \quad . \quad . \quad . \quad . \quad (2)$$

∴ Equation 2 expressed dimensionally becomes

$$\frac{ML}{T^2} = \left(\frac{M}{L^3}\right)^a \cdot L^b \cdot \left(\frac{L}{T}\right)^c \cdot \left(\frac{L}{T^2}\right)^d \quad . \quad . \quad . \quad . \quad (3)$$

* *i.e.*, assuming that the wave-making resistance will depend upon the density of the water, the size and speed of the vessel, and the value of g in the particular locality. Equation (1) is the functional representation of this statement.

The powers of M, L and T on each side of equations (3) must be necessarily equal,

∴ Comparing powers of M

$$1 = a \qquad (4)$$

Comparing powers of L

$$1 = -3 + b + c + d$$

$$\therefore \; b + c + d = 4 \qquad (5)$$

Comparing powers of T

$$-2 = -c - 2d$$

$$\therefore \; d = 1 - \frac{c}{2} \qquad (6)$$

∴ from (5)

$$b = 3 - \frac{c}{2} \qquad (7)$$

∴ substituting these values of a, b, and d in equation 2 we have

$$R = \rho \cdot l^{3-\frac{c}{2}} \cdot v^{c} \cdot g^{1-c}$$

i.e.

$$R = \rho \cdot g \cdot l^{3} \left(\frac{v^2}{l \cdot g}\right)^{\frac{c}{2}} \qquad (8)$$

For given values of ρ and g and affecting the substitutions $l^3 = \triangle$ and $l = \triangle^{\frac{1}{3}}$ equation 8 becomes

$$R = K \cdot \triangle \cdot \left(\frac{V}{\triangle^{\frac{1}{6}}}\right)^{c} \qquad (9)$$

Equation 9 forms a convenient presentation of the Froude law, for from it, it at once follows that when $\frac{V}{\triangle^{\frac{1}{6}}}$ is constant the resistance varies directly as $\triangle$ the displacement of the vessel. Equation 9 can be simply converted to express power instead of resistance.

Thus

$$P = K_1 \triangle^{\frac{7}{6}} \cdot \left(\frac{V}{\triangle^{\frac{1}{6}}}\right)^{c+1} \qquad (10)$$

These equations 9 and 10 could form the basis of approximate formulæ for wave-making resistance. For example, rewriting equation 9 as

$$R = K^1 \triangle \cdot \left(\frac{V^2}{L}\right)^{\frac{c}{2}}$$

and making $K = 12{\cdot}5\,\delta$ and $c = 4$ the well-known Taylor formula is at once obtained, viz :

$$R = 12{\cdot}5\,\delta \cdot \triangle \cdot \frac{V^4}{L^2}$$

---

2. *Proof of the Rayleigh Law.*

The frictional resistance of a vessel may be expressed* in the most general terms as

$$R = f(\rho \,.\, l, v, \nu) \qquad (11)$$

as before let

$$R = \rho^{a} \cdot l^{b} \cdot v^{c} \cdot \nu^{d} \qquad (12)$$

Equation 12 expressed dimensionally becomes

$$\frac{ML}{T^2} = \left(\frac{M}{L^3}\right)^{a} \cdot L^{b} \cdot \left(\frac{L}{T}\right)^{c} \cdot \left(\frac{L^2}{T}\right)^{d} \qquad (13)$$

* *i.e.*, assuming that the frictional resistance depends upon the density and viscosity of the water, and the length and speed of the ship. Equation (11) is the functional representation of this statement.

Comparing powers of M

$$a = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

Comparing powers of c

$$-3 + b + c + 2d = 1$$

$$\therefore \quad b + c + 2d = 4 \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Comparing powers of T

$$-c - d = -2$$

$$\therefore \quad d = \quad 2 - c \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

$\therefore$ from 15

$$b = c \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$\therefore$ Substituting these values of a b and d in equation 12, we have

$$R = \rho \cdot l^c \cdot v^c \cdot \nu^{2-c}$$

$$= \rho\, l^2 v^2 \left(\frac{v \cdot l}{\nu}\right)^{c-2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Instead of l^2 we may write A the value of the wetted surface.

$$\therefore \quad R = \rho \cdot A \cdot v^2 \left(\frac{v \cdot l}{\nu}\right)^{c-2} \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Equation 19 contains the usual enunciation of the Rayleigh law for it once follows that when $\left(\frac{v.\,l.}{\nu}\right)$ is constant $\frac{R}{\rho\, A\, v^2}$ is also constant.

For the study of the ship resistance problem equation 19 can be modified into a more convenient although admittedly approximate presentation. For standard density and temperature and effecting the substitution $c - 2 = -n$, equation 19 may be written

$$R = K \cdot A \cdot \frac{v^{2-n}}{l^n}$$

or in terms of power

$$P = K_1 \cdot A \cdot \frac{v^{3-n}}{l^n} \quad . \quad . \quad . \quad . \quad . \quad (20)$$

Letting $A = \triangle^{\frac{2}{3}}$

and $l = \triangle^{\frac{1}{3}}$

equation 20 becomes

$$P = K_2 \cdot \triangle^{\frac{2}{3} - \frac{n}{3}} v^{3-n} \quad . \quad . \quad . \quad . \quad . \quad (21)$$

and multiplying top and bottom of the R.H.S. of 21 by $\triangle^{\frac{3-n}{6}}$ we have

$$P = K_2 \triangle^{\frac{2}{3} - \frac{n}{3} + \frac{1}{2} - \frac{n}{6}} \cdot \left(\frac{V}{\triangle^{\frac{1}{6}}}\right)^{3-n}$$

$$= K_2 \triangle^{\frac{7}{6} - \frac{n}{2}} \cdot \left(\frac{V}{\triangle^{\frac{1}{6}}}\right)^{3-n} \quad . \quad . \quad . \quad . \quad (22)$$

Equation 22 forms the most convenient presentation of the approximate form of the Rayleigh law of frictional resistance from the naval architect's standpoint. For from it at once follows that when $\frac{V}{\triangle^{\frac{1}{6}}}$ is constant the frictional power varies directly as $\triangle^{\frac{7}{6} - \frac{n}{2}}$

Equations 10 and 22 are thus analogous and are of fundamental importance in the powering of ships. The value of n may be determined by a method given in Part 3 of this appendix. Its value for thin planks has been recently (8) exhaustively examined by Dr. Gebers, of Vienna, and to it he assigns a value of 0·125.* Assuming that n

* Dr. Gebers' experiments justify the application of the approximate form of the Rayleigh law to planks which are not dynamically similar.

retains the same value for solid forms as for thin planks then equation 20 becomes

$$P = K_1 A \cdot \frac{v^{2.875}}{l^{.125}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20a)$$

similarly equation 22 becomes

$$P = K_2 \triangle^{\frac{53}{48}} \left(\frac{V}{\triangle^{\frac{1}{6}}} \right)^{2.875} \quad . \quad . \quad . \quad . \quad . \quad (22a)$$

Hence, if P_{rf} be the frictional power of the one-ton model and P_f the frictional power of a similar vessel of displacement $\triangle$ at the corresponding speed, then

$$P_f = P_{rf} \triangle^{\frac{53}{48}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

3. *Determination of the frictional resistance of a ship formed solid.**

When the resistance results of two similar models are known, one preferably twice the length of the other, the correct frictional and wave-making components of the resistance can be simply determined if the validity of the approximate form of the Rayleigh law is granted.

Let the total resistance of the smaller model be split up as follows

$$r = f + w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

The total resistance of the larger model λ times the smaller at the correct corresponding speed becomes

$$R = F + W$$

which from equation 22 when $\triangle^{\frac{1}{3}} = \lambda$, F can be shown to equal $f\lambda^{3-\frac{3n}{2}}$

$$\therefore \; R = f\lambda^{3-\frac{3n}{2}} + w\lambda^3 \quad . \quad . \quad . \quad . \quad . \quad . \quad (24a$$

from equations 24 and 24a f can be determined.

Thus
$$f = \frac{r\lambda^3 - R}{\lambda^3 - \lambda^{3-\frac{3n}{2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

Similarly for any other pair of corresponding speeds

$$f_1 = \frac{r_1\lambda^3 - R_1}{\lambda^3 - \lambda^{3-\frac{3n}{2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25a)$$

Thus
$$\frac{f}{f_1} = \frac{r\lambda^3 - R}{r_1\lambda^3 - R_1}$$

but
$$\frac{f}{f_1} = \left(\frac{v}{v_1}\right)^{2-n}$$

$$\therefore \left(\frac{v}{v_1}\right)^{2-n} = \frac{r\lambda^3 - R}{r_1\lambda^3 - R_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

$\therefore$ Taking logs and rearranging

$$2 - n = \frac{\log (r\lambda^3 - R) - \log r_1\lambda^3 - R_1)}{\log v - \log v_1}$$

$$\therefore \; n = 2 - \frac{\log (r\lambda^3 - R) - \log (r_1\lambda^3 - R_1)}{\log v - \log v_1} \quad . \quad . \quad . \quad . \quad (27)$$

from equation 27 when n is obtained as the mean value derived from a number of pairs of corresponding speeds it can be substituted in equation 25 and hence the frictional resistance of the smaller model determined at each speed. Equation 25 becomes when $n = \cdot 125$

$$f = \frac{r\lambda^3 - R}{\lambda^3 - \lambda^{2.8125}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (25b)$$

Thus, in order that the percentage error in f may be equal to that in R, the denominator $\lambda^3 - \lambda^{2.8125}$ should equal unity and hence λ should be approximately 2. Providing this scale ratio be respected the above method should prove to be quite practical. It is very desirable that the method be tested in an experiment tank in order that the correct value of n for solid forms may be determined. In the case of the

* See also an article by the present Author in the *Marine Engineer and Naval Architect*, October, 1922; also W. Den Boer in the Dutch Shipbuilding Journal, *Scheepsbouw en Scheepvaart*, September, October, November, 1922.

William Froude Tank, the auxiliary tank with moving water and fixed models would appear to be especially convenient for use with this analysis, once of course its present constructional difficulties have been overcome. In commercial research the duplication of experiment necessitated by this method may possibly prove prohibitive. From the pure research standpoint the method should be used in determining for an assumed value of n equal say ·125 the necessary form, butts, laps, etc., increase over the "equivalent plank" power and thus the frictional power of the one-ton model calculated from equation 20a. In this expression for v in knots A in square feet and l in feet for salt water $p = 1{\cdot}025$ and temperature $= 10°$ C. and for a thin plank the frictional power becomes

$$P = \frac{{\cdot}01352}{326} \cdot A \cdot \frac{v^{2{\cdot}875}}{l^{{\cdot}125}}$$

This expression can be modified by an appropriate form factor and the relative frictional power curve of the model can be simply drawn in. Once this curve is constructed the practitioner need only refer to the alignment chart in Plate I., to obtain the frictional power of any similar vessel.

Appendix 4.

To those designers who would wish to make use of Plate I. in their general practice it is probably advisable to reconstruct the chart on stiff board. To enable this to be done all the scales used in Plate I. are given in the skeleton diagram in Fig. 2. The actual layout need not be detailed here, and the reader is referred to any standard work on nomography for a description of the principles of these charts. Plate I. covers a very large range of size. It is probably advisable in a designing office to make use of a much smaller range and reconstruct the chart accordingly.

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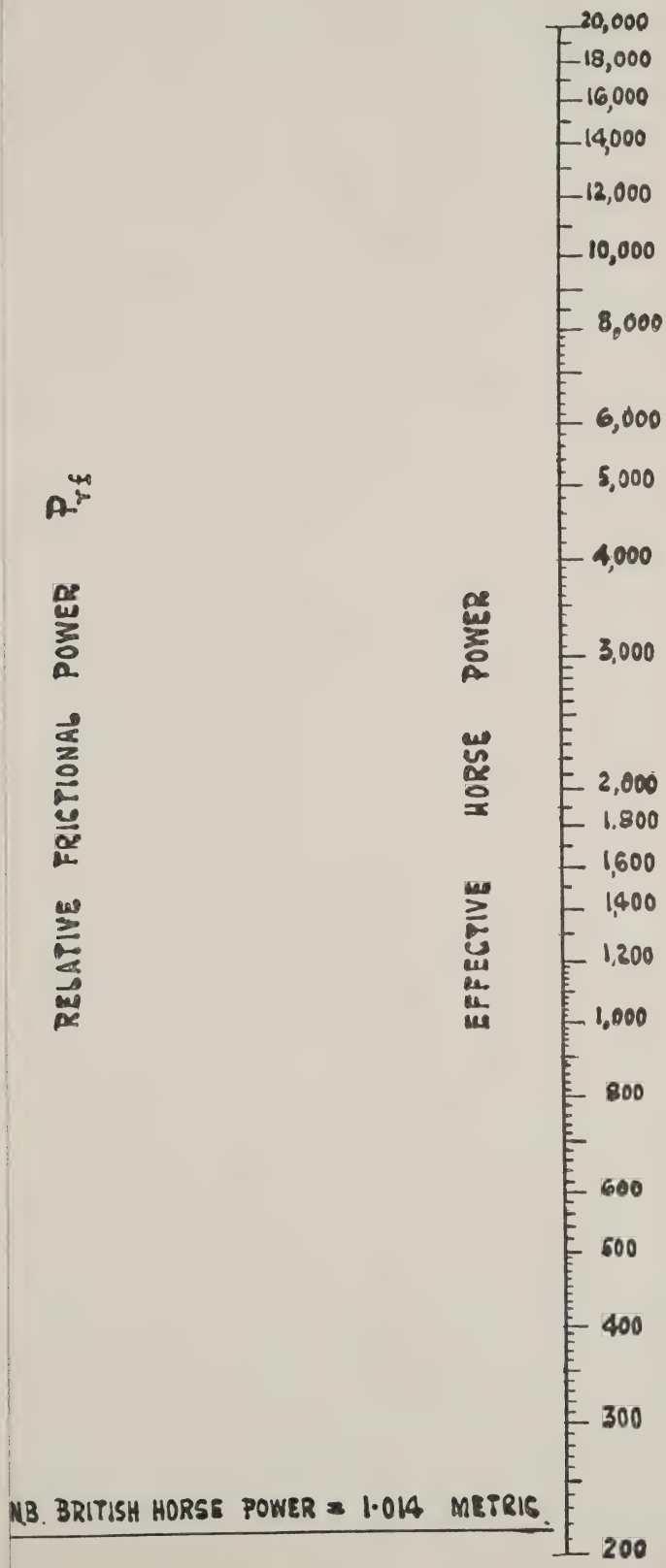
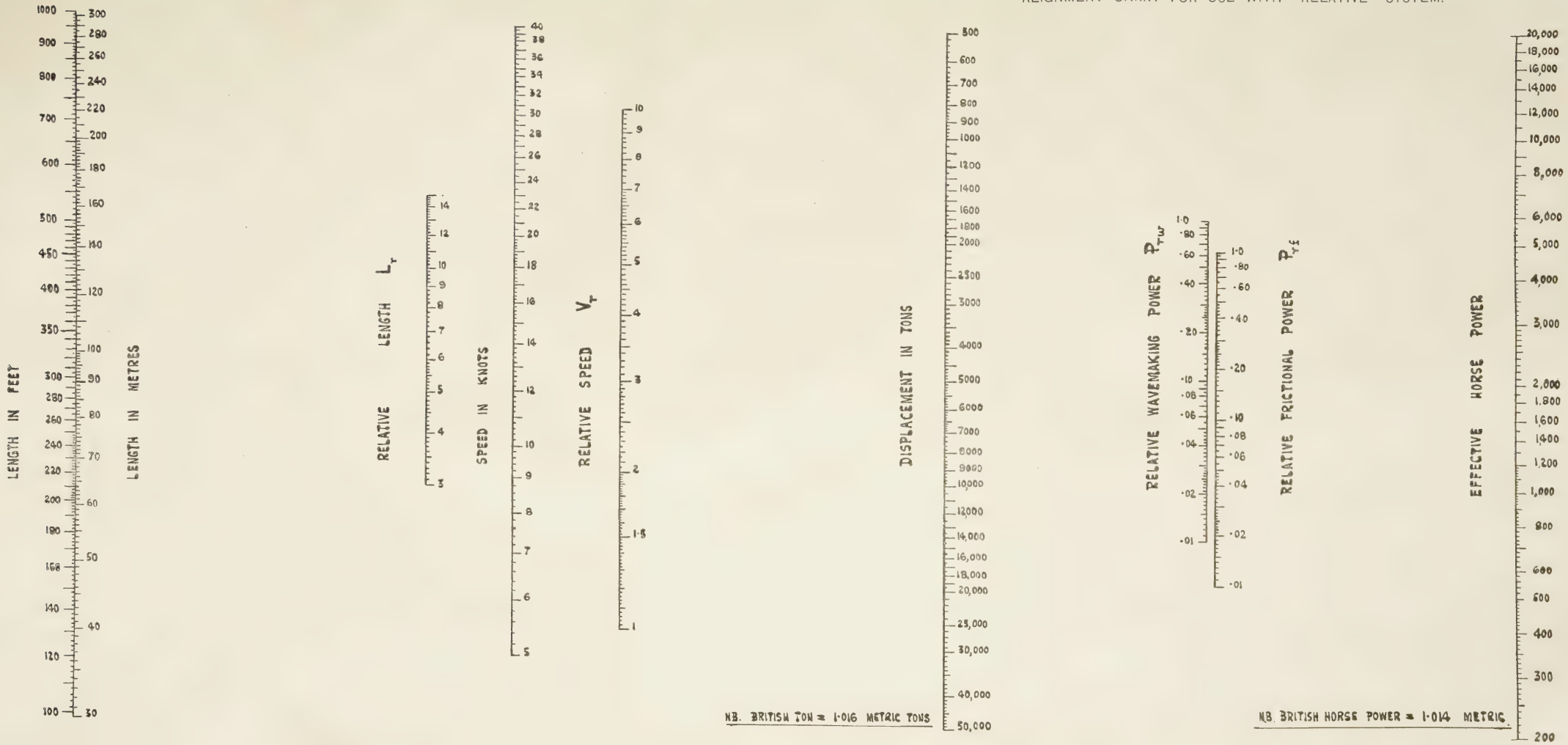
TH "RELATIVE" SYSTEM.



Illustrating Mr. E. V. Telfer's Paper, "Note on the Presentation of Ship Model Experiment Data."

ALIGNMENT CHART FOR USE WITH "RELATIVE" SYSTEM.



DISCUSSION.

Mr. H. G. WILLIAMS, *Member of Council*: All practising naval architects will agree with Mr. Telfer that it would be a great convenience if the scientific gentlemen who make tank experiments would present their results in some common form. Mr. Telfer's proposals with that object in view are evidently based on a very careful study of the subject, and I certainly do not profess to criticize them to-night from any equally strong standpoint. I only propose to make some remarks on the subject from the point of view of one who has used model experiment data a lot and has been occasionally betrayed into the use of unparliamentary language by the trouble necessary to compare those given by one experimenter with those given by another. Mr. Williams

With this premise, I must say that I think Mr. Telfer brave to the verge of imprudence in advocating the adoption of metric units. The average English-speaking business man (and, of course, some naval architects are business men) meets the suggestion that he should think and calculate in metres and kilograms in much the same spirit as Naaman of old met the suggestion that he should bathe in the River Jordan and be clean. The leprous Syrian, as no doubt you remember, went right off at the deep end, but not into the River Jordan; and so does the professedly practical man when you ask him to adopt the metric system.

It appears to me that the object in view can be attained without asking the man who thinks in feet and pounds avoirdupois to change to metres and kilograms or *vice versa*.

The law of comparison demands that speed should be indicated by a number expressing the ratio between conventional speed and the square root of some linear dimension on which the size of the ship depends. The worst trouble of the present state of affairs is that experimenters do not agree on the linear dimension that should be used in forming this ratio. As pointed out by Mr. Telfer, some use $\triangle^{\frac{1}{3}}$ where $\triangle$ is the volume of displacement, some use L the length on the load waterline or between perpendiculars, and Mr. G. S. Baker ploughs a solitary furrow with PL, P being the prismatic coefficient. The worst of it is that there is something to be said for each of these usages. Each brings out a somewhat different aspect of the way in which resistance varies with speed and dimensions. Personally, I prefer the use of L, the length of the ship between perpendiculars on the ground that it is the most fundamental dimension in a design, that it is easier to calculate $\frac{V}{\sqrt{L}}$ approximately in your head than $\frac{V}{\triangle^{\frac{1}{6}}}$ and therefore to know quickly whereabouts in the scale of relative speed any speed in knots of a given ship stands; and it is easier to memorize the general nature and magnitude of the resistance results associated with a value of $\frac{V}{\sqrt{L}}$ than with a value of $\frac{V}{\triangle^{\frac{1}{6}}}$ or $\frac{V}{\sqrt{PL}}$.

Mr. Williams

But any such preference is largely a matter of taste and habit, and we all know how little use it is arguing about matters of taste. Of course, the real solution of the difficulty is that experimenters should present their results in all three forms, *i.e.*, on abscissa values or equal speed diagrams of $\frac{V}{\triangle^{\frac{1}{6}}}$, $\frac{V}{\sqrt{L}}$ and $\frac{V}{\sqrt{PL}}$, but I suppose that is too much to ask of them. I should be satisfied if they only gave them on the two bases $\frac{V}{\triangle^{\frac{1}{6}}}$ and $\frac{V}{\sqrt{L}}$.

These quantities have the dimensions $\frac{\sqrt{\text{length}}}{\text{time}}$, *i.e.*, the square root of an acceleration, and it is desirable to make them non-dimensional (so that they are expressed in the same number in all systems of units) by dividing by the square root of the number representing the same acceleration in those units.

This is what Froude does to make his (K) value the same when expressed in any system of units, and I think it would be an improvement if $\frac{V}{\sqrt{gL}}$ were used instead of $\frac{V}{\sqrt{L}}$ by Taylor and others who use this type of speed number. But this point is not of so much importance as the selection of the linear dimension to be used, which affects the nature of the resulting diagrams, whereas the use of a divisor such as $\sqrt{g}$ only affects the scale, and is easily allowed for in comparing one set of results with another.

If in the expression $\frac{V}{\triangle^{\frac{1}{6}}}$, V is expressed in knots and $\triangle$ in tons, not in cubic units, the expression is the same for all practical purposes, whether metric or avoirdupois tons are used. The inaccuracy caused by regarding these units as identical is less than the probable error in experiment and correction for friction.

According to the law of comparison, the resistance on a basis of corresponding speeds is proportional to the cube of the linear dimensions and therefore must be represented by a quantity of the form $\frac{R}{\triangle}$, which is non-dimensional if $\triangle$ is the weight displacement in tons and R is expressed in tons, it is, in fact, simply the fraction of the displacement that the resistance represents. It would be a somewhat inconvenient form of presentation if used without a multiplier, because the fraction would be very small, and if expressed as a decimal would often have several 0's before the significant figures. Taylor gets over this by expressing his resistances in lb. per ton of displacement, and loses in universality what he gains in convenience for people using English weights and measures. I think it would have been better to take a conventional multiplier, say, 1,000, for the fraction of the displacement represented. At any rate, users of the metric system would probably

be more willing to express resistances in thousandths than in two-thousand-two-hundred-and-fortieths of the weight displacement. Mr. Williams

Of course, horse-power, not resistance, is the quantity generally required by the designer, and to give this directly the quantity shown should be some multiple of $\frac{RV}{\triangle V}$.

The constancy of this quantity for all sizes of ships is not affected by multiplying or dividing it by the undimensional speed number to which it refers, and in this way R. E. Froude, Baker and others use it in the form $\frac{E.H.P.}{\triangle^{\frac{2}{3}} V^3}$ which does not really give horse-power any more readily, *i.e.*, with any less arithmetic, than the $\frac{R}{\triangle}$ form, inasmuch as it involves the $\frac{2}{3}$-power of the displacement and the cube of the speed. As far as I can recollect, the only reason given by R. E. Froude for adopting it was that in its inverted form it was familiar to shipbuilders as the Admiralty constant. It has the merit of having practically the same value in both British and metric ordinary units, since the knot is universally used as the unit of speed and the metric h.p. and ton are so near the English h.p. and ton that the value of the expression is substantially the same in both systems of units. It wants some large multiplying constant to make its values somewhere near unity, and that is one of the merits of Froude's multiplier 427·1. Although I certainly do not despise the judicious use of the Admiralty constant in estimating powers, I do not think there is any strong reason for presenting model e.h.p.'s in a form that resembles the Admiralty constant. On the whole I should prefer these to be presented in Taylor's form as resistance forces expressed as fractions of the displacement though there can be little objection to $\frac{E.H.P.}{\triangle^{\frac{7}{6}}}$ or $\frac{E.H.P.}{L^{\frac{7}{2}}}$ with numerical factors to make their values the same in all systems of consistent units.

The geometrical quality of the form of ship should certainly be presented in non-dimensional units. Froude's length constant is quite satisfactory in this respect, and it is to be wished that everyone would use it. Taylor's $\frac{\triangle}{[\cdot 01 L]^3}$ is not undimensional since $\triangle$ is the weight displacement, and is therefore not the same in metres as in English units. The length, breadth and draught constants as used by R. E. Froude, the length of parallel middle body expressed as a fraction of the length of the ship, and the prismatic co-efficients of the forward and after ends beyond the parallel body and the fraction of the length of ship over which they extend, with the area coefficients of the midship section and the fore and after ends of the water planes, are all undimensional, and I do not see how they can be improved upon as numbers defining the more important features of the shape of immersed hull which affect the resistance to an extent that can be measured and reduced to an empirical law.

Mr. Williams

To me the most interesting part of Mr. Telfer's Paper is Appendix 3. The result he reaches there is new to me, and I have no doubt it is right in form though I have had no time to satisfy myself by going through the argument and algebra in detail. I doubt very much whether the experiments that have been made on the viscosity resistance of solid forms in water and air bear out the adoption of Herr Geber's value of the index n as a constant for all shapes and for all values of VL.

Mr. Telfer is to be congratulated on having given us a very able and interesting Paper, which I, at any rate, shall endeavour to find time to study carefully.

Mr. Tutin

MR. JOHN TUTIN, *Graduate*: This Paper is one for which the world of naval architecture has been waiting for some time, and I am very glad that Mr. Telfer has had the courage to come forward with this "Relative" system, since all who have studied the problem must have felt that it was an imperative necessity that some such thing should be done.

The proposed system appears to satisfy almost completely the various conditions which should govern the formation of the ideal international system as enunciated in the Paper. Few naval architects are better qualified to discuss international systems than Mr. Telfer who has had a unique experience in studying the various European and American technical works on the subject.

I agree with Mr. Williams that the most fascinating portion of the Paper is the Appendix. Mr. Telfer, I imagine, must have done a certain amount of unseen work, in obtaining his proofs of the Froude and Rayleigh Laws. The only point upon which I should like to have a little more explanation is as to whether these proofs, as they stand, are mathematically rigorous. The assumptions are hardly sufficiently explicit, but this is probably due to an attempt to secure condensation. In Equation 2 on page 232, the Author assumes a particular function to consist of various factors, the different variables being raised to certain indices. This is not altogether justifiable: it would be mathematically more complete to assume that, for example, resistance varies with speed in a more general manner :—

$$R = x_0 + x_1 v + x_2 v^2 + x_3 v^3 + \ldots x_c v^c + \ldots$$

There is no reason why the Laws should not be established in this way, but otherwise the solution obtained must be regarded merely as one particular solution.

With regard to Appendix 3, I would like to bring out an interesting point. The Rayleigh law requires that at corresponding speeds, $\left(\frac{V}{\Delta^{\frac{1}{6}}} \text{ constant}\right)$, the frictional power varies for similar models, directly as $\Delta^{\frac{7}{6}-\frac{n}{2}}$. This involves, however, a definite relation between the indices of

displacement and speed in the general expression for frictional power for a ship, displacement $\triangle$, and speed V_1, *i.e.*— Mr. Tutin

$$P_1 = K \triangle_1^M V_1^N$$

[assuming that the phenomenon is mathematically "homogeneous" (see above).]

Now, for a slightly larger vessel of displacement $\triangle_2$, and corresponding speed $V_2 = V_1 \times \left(\frac{\triangle_2}{\triangle_1}\right)^{\frac{1}{6}}$

$$P_2 = K \triangle_2^M V_2^N = K \triangle_2^M V_1^N \left(\frac{\triangle_2}{\triangle_1}\right)^{\frac{N}{6}}$$

whence :— $\frac{P_2}{P_1} = \left(\frac{\triangle_2}{\triangle_1}\right)^M \left(\frac{\triangle_2}{\triangle_1}\right)^{\frac{N}{6}} = \left(\frac{\triangle_2}{\triangle_1}\right)^{\frac{7}{6} - \frac{n}{2}}$ . . . (Rayleigh Law)

Equating indices of $\frac{\triangle_2}{\triangle_1}$:—$M + \frac{N}{6} = \frac{7}{6} - \frac{n}{2}$

whence :— $n = \frac{7 - N}{3} - 2\,M.$

Thus, from experimental determinations of M and N, the corresponding value of n can be directly obtained.

In conclusion I wish to emphasize the great value of this Relative system put forward by Mr. Telfer. It is useful to naval architects from two aspects; from the educational aspect it will simplify the explanation of ship resistance to students because it will be so easily visualized; and, of course, it is invaluable from the professional point of view because of the ease with which we shall be able to go from one set of experimental results to another set performed possibly in different countries under different systems. I hope it is not too much to suggest that the Institution may lend its weight to push forward this new system, because I am sure it would be appreciated by the rest of the professional naval architects of the world. Nothing but good could accrue from the universal adoption of such a system, and Mr. Telfer is to be congratulated upon having done his work so conscientiously and so successfully.

MR. ROBERT HINCHLIFFE, *Member of Council*: I should like to associate myself with the previous speakers in their congratulations to Mr. Telfer upon his very able Paper. All who, in their daily work, have to use the data that is from time to time given out by the various experimental tanks will appreciate the reasons that have led Mr. Telfer to urge the necessity for a standard international system of notation and presentation of results, and many of them will regret that such a scheme was not put forward and adopted 20 years ago. Mr. Hinchliffe

The proposals set forth in the Paper under discussion, however, will commend themselves, I expect, most strongly to the naval architects of Mr. Telfer's generation; those who, like myself, have been using resistance data for a number of years, will probably be more lukewarm in their support, owing to the fact that for some considerable time they have been wedded to one or other of the notation systems and have all their data recorded in that system.

Mr. Hinchliffe

My own note-book contains the results of trials of somewhere between 500 and 1,000 models, Froude's "Constant" system being employed in the majority of cases; I should look forward with terror to the task of converting all these into any other system, however excellent it might be. I have always used the Froude "Constant" system and find it very convenient for everyday work—possibly because I am thoroughly familiar with it. On the other hand, the young naval architect who is just beginning to collect his speed data and is busy extracting from the transactions of the various technical societies the wealth of information contained therein, will quickly appreciate the necessity for a uniform system of notation, and will wish the Author of this Paper all success in his efforts to bring this about.

I would point out to Mr. Telfer, however, that it will be necessary for him to be rather more explicit with regard to his definitions. It will not be sufficient, for example, to fix an international symbol for length unless we are sure that all experimenters are applying the term to the same dimension. If one writer uses the symbol to denote "length between perpendiculars," a second to denote "length on load water-line," whilst a third uses the same symbol to represent "mean immersed length," we shall be as far from uniformity as ever. Similarly with "breadth" we must know whether "moulded breadth," "extreme breadth" or "breadth over mean thickness of shell" is intended, and so on with practically every other dimension or coefficient.

I should also like to suggest to Mr. Telfer that he carries his system of international symbols a step further to include propeller and propulsion data, as in these the methods of presentation appear to lack uniformity to an equal or possibly greater extent than is the case with ship-resistance problems.

There is a small difficulty which I—and probably others—have encountered when using model results, which arises when the constants for a new design have been worked out and it is desired to determine quickly which model, in the data available, is most suitable for use as a type. Various methods might be used: in my own work I use a form of graph index in which the (M) values are plotted as ordinates with the prismatic coefficient as abscissæ, the resulting spot being given a distinguishing number.

By this means it is possible at once to determine the models which approximate to the given ship as regards length ratio and prismatic coefficient. It would add to the value of this chart if it could also include a third dimension—say breadth-ratio—and this could possibly be done by the use of some form of three-dimensional graph. I have not, however, as yet, had either the time or the energy to develop this idea fully, and I pass it on to my friend Mr. Telfer in the hope that he may find time to apply his inventive brain to its solution.

I sincerely trust that this Paper will receive the attention it deserves from those gentlemen who control the various experiment tanks and

that it may lead to their getting together and agreeing to the adoption of this or some other international standard system for the recording and publishing of experiment data connected with problems of ship propulsion. Should this very desirable result be obtained naval architects of future generations will reap a considerable benefit and ought to bless the name of Mr. Telfer. Mr. Hinchliffe

Mr. W. R. G. WHITING, *Member*: It is a pleasure to associate myself with previous speakers in congratulating Mr. Telfer on this instructive and constructive Paper. When his proposals have been fully considered and finally adopted scientific progress will have again furthered the cause of international amity. My only regret is that in this respect Mr. Telfer shows no originality. He pursues the conventional method of negotiating with the Continent: improvement of foreign relations is once more identified with surrendering our own identity and the long suffering Christian units are thrown to the metric lions! Mr. Whiting

I am not quite clear as to the use of the word, dimensionless, which appears to connote standardized or referred *i.e.*, relative to a common basis. The final expressions all contain dimensions, as mathematically understood, and the numerical values depend on the system of units employed. Now a dimensionless quantity, such as the number of beams in the length, is independent of the unit in which the length is measured.

I take it Mr. Telfer does not propose to exclude different presentations of the same data. The mass of resistance phenomena is one bulk but the methods of delineating its characteristics, its interior stratification, are, and should remain, many. For example, Taylor's contours are convenient in the form in which they are expressed. Convert the pounds per ton into kilogrammes per metric ton if you will, and the American "fatness ratios" into their metric equivalents, but do not (indeed you cannot without sacrifice of essential features) dispense with resistance in favour of power, nor juggle with displacement to the one-sixth instead of length to the half. One experimenter directs attention to the effect of prismatic coefficient, another to water plane, another to run and entrance. All these forms are useful, none are essentially superior to all others. Each of the three views in a sheer draught completely and independently defines a certain solid, but one cannot be preferred to the exclusion of another, nor are they interconvertible in a continuous sense, in two dimensions. In fact, these proposals gain force as their application is understood to be limited to the regularization of nomenclature and, if need be, units. They gain force, too, if they are recognized as leading to a wider field. The presentation of propeller results demands similar work which will be of even more importance. Not only presentation, but the basis of conception needs to be considered and, if possible, scientifically

Mr. Whiting

refounded. Correlation with aeronautical research is essential and common agreement necessary as regards both symbols and definitions. May I recommend this Augean stable when Mr. Telfer next feels inclined to wield his broom?

Prof. Welch

THE CHAIRMAN, Prof. J. J. Welch, *Vice-President*: There are only one or two words which I wish to add to what has been said, because the points have been so fully covered. I may say that we all agree that the methods of presentation of results of model ship speed and power trials are very varied, and that it would be a great thing if we could persuade experimenters to present their results in one particular way; and if our foreign friends, as well as some English experimenters, could be persuaded to present them all in the Froude constant system, I for one would be quite satisfied with that, because these constants have the same numerical values whatever system of units is employed. The only difference is that for other systems of units some modification of the multipliers of the several "constants" would be required; for example, for countries using the metric system the (K) multiplier would be 0·585 instead of the 0·5834 of Froude, and so on. The numerical values of the constants would be the same in each, and direct comparison is at once possible without interference with national units. However, Mr. Telfer comes to the subject and finds it is not the case that all experimenters are using the same constant system, and he presents us with a very complete alternative, provided that our British and American friends will accept the metric system. Well, if that were to happen it would be very convenient, but personally I am not quite so hopeful as Mr. Telfer appears to be that future international developments will be in that direction. If it were so, then Mr. Telfer suggests a distinctly simpler method of presentation of results, which has the distinct advantage amongst others which he has pointed out of being easily visualized.

Like several other speakers, I have been very much interested in the appendix portion of the Paper, and I rather agree with Mr. Tutin that the proofs of the Froude and Rayleigh laws could be somewhat expanded with advantage. I think Mr. Tutin pointed out that whilst the resistance could be written in general form, the step is rather too rapid to say that it can be expressed in the one particular form chosen without some explanation. Two or three lines will be quite sufficient, and would add to the value of the proofs, if Mr. Telfer could see his way to make this addition.

I am not quite sure what Mr. Telfer means on page 223 by saying that the Froude (K) refers to the velocity of a one-cubic-foot model. The (K) is the ratio of the speed of a ship to the speed of a wave having a length equal to half the length of the side of a cube having a content equal to the volume of ship's displacement, and in its most general form may be written $\frac{V}{D^{\frac{1}{6}}} \times \sqrt{\frac{4\pi}{g}}$ where D is the volume of dis-

placement; in that form it has the same numerical value in all consistent systems of units, and I am not sure that it does give exactly the corresponding speed of the one-cubic-foot model, which is my interpretation of Mr. Telfer's statement. Prof Welch

As I said earlier, I think Mr. Telfer must be congratulated on the wide interest taken in his Paper, an interest which I am sure is by no means exhausted by the discussion which we have had to-night. In addition to that excellent discussion, written contributions have already been received from Mr. Froude, Mr. Baker and others, all of which will go to enrich our *Transactions*.

Mr. E. LIDDELL, *Associate Member*: The "Relative System of Notation" proposed by Mr. Telfer is, in my opinion, perfectly convenient and adequate. There are, however, a few points, chiefly connected with the expressions for relative velocity and relative power on which I would like to elicit an opinion from the Author. Mr. Liddell

I have always been under the impression that investigators who used (C) or $\frac{V}{\sqrt{L}}$ did so on the broad assumption that bow and stern wave interference was in some definite way connected with the ratio:—

$$\frac{\text{Wave length appropriate to Speed of Ship}}{\text{Length of Ship.}}$$

Messrs. Baker and Kent, for their part, have for some time now replaced their original relative speed expression (K) by another modification of $\frac{V}{\sqrt{L}}$ which—they have argued without finding much opposition—enables them to connect the humps and hollows of their (C) curves due to wave interference with definite abscissa values. There appears, then, to be a prima facie case for giving closer consideration to the expression (P) than to the other modification of $\frac{V}{\sqrt{L}}$.

Plotting on this base (P) would seem to enable the investigator to make up his mind whether particular humps are to be set down to wave interference and thus to read his relative-power curve more clearly. Again, if the prismatic length is taken to be the principal wave-making feature, the designer will be largely guided by the resultant value of (P) in co-ordinating length, speed, and prismatic coefficient. It would be the (P) values of models for any given relative power that he would be interested in.

I am, of course, perfectly sensible to the greater simplicity obtained in abstracting from particular linear dimensions and in referring to a model of unit size. However, adopting the metric ton in speed power work without adopting the metric system in toto would have disadvantages of its own, especially if deadweight, etc., continued to be expressed in tons of 2,240 lb. On the other hand, in the case of "well formed"

Mr. Liddell

ships, wave making and wave interference are, after all, the principal phenomena on which model experiments are at present intended to throw light. There seems, thus, to be at least something to be said for correlating speed and wave-making features rather than speed and displacement.

The unit of relative speed $\frac{V}{\Delta^{\frac{1}{6}}}$ is an arbitrary one. The unit value of (P) on the other hand would seem to have a more definite significance yielding as it does the absolute speeds beyond which only exceptional ships can be driven without appreciable changes of immersion —without loss of hydrodynamical "identity" as it were.

It is this apparently more pronounced physical significance that has impressed me and I would be very glad to have the Author's opinion on this point.

As regards internationality there is, of course, the disadvantage that a different constant has to be used in calculating (P) and lengths have to be converted in working with Continental results. The characteristic values of (P) $= 1, \frac{1}{\sqrt{2}}$ etc., would, however, still have the same significance, *i.e.*, coincidence of first crest of stern system with middle height of front slope of third crest of bow system, etc. For a given prismatic length whether expressed in metres or feet associated values of V and (P) would be identical.

$$(P)\ \text{metric} = \frac{1852}{3600}\sqrt{\frac{6{\cdot}28}{9{\cdot}81}} \times \frac{V}{\sqrt{\phi L_m}} = {\cdot}412 \text{ approximately } \frac{Vkn^2}{\sqrt{\phi L_m}}$$

In connexion with the ordinate of the (Relative) Power Speed Diagram I would like to touch on what seems to be the underlying reason for a good deal of the divergence in usage.

Mr. Baker has put it on record that he adheres to (C) "because all that people want to know is, what have you got to multiply your speed and displacement by to get your horse-power," and again "why should you cut up your cake in two or three pieces when you are selling it?" It would appear from this that he is of the opinion that model-tank results will in the first instance be used by the engineer and the propeller designer whose chief concern will be with the total e.h.p. and who, within limits, will leave the design of the hull to the naval architect. The same would apply to experimenters who plot total relative powers with correction curves for friction. (In passing, it may be observed, that the Schaffran expressions which have e.h.p. in the denominator might almost be taken to indicate a tendency to start from e.h.p. as the fundamental datum of design).

Admiral Taylor and the Author in plotting separately relative frictional and relative wave-making resistance (and power) appear to be concerned chiefly with the qualities of "models" as such thus enabling ship designers to work out readily the amount by which total relative resist-

ance or power exceeds the irreducible minimum given by the equation of the form— Mr. Liddell

$$Pf = fAv^n$$

the power absorbed by a plank of length area and roughness of the vessel under consideration.

I have sometimes thought that the ratio—

$$Pf \text{ (Equivalent Plank)} \div P \text{ total}$$

might possibly afford a criterion of what, in order to avoid the term hull or form efficiency, could be called the value or quality of the model or ship form, the variation of which ratio would show the influence of absolute size and speed. I am therefore particularly interested in the discussion of the Rayleigh Reynolds law in which, it seems to me, the Author is pursuing similar trains of thought. The expressions, on page 227, for P_w and P show, if combined, the gradual change of percentages of wave-making and frictional powers as the size is increased—

$$\frac{P_w}{P_f} = \frac{P_{rw}}{P_{rf}} \times \frac{\triangle^{\frac{7}{6}}}{\triangle^{\frac{63}{48}}} = \frac{P_{rw}}{P_{rf}} \times \triangle^{\frac{1}{16}}.$$

It may be permissible to point out that this is perhaps the only feature which plotting in "relative" co-ordinates does not make immediately visible. It is easy enough to take from such a diagram, without preparing further curves, values for e.h.p., V, or $\triangle$ if for say, $\triangle$ the appropriate scales of e.h.p. and V are appended. With the help of the same scales of $\triangle$ for any given speed it is possible to lift off and compare from the same diagram the ordinates of M. Doyère's chart of variation of relative power with absolute displacement at different absolute speeds; but even if P_f and P_w are plotted separately the "visible" ratio $P_f \div (P_f\, P_w)$ or $P_w \div P_f$ only applies to the unit model. The Author's alignment chart should, of course, go a long way to remedy this defect (if it can be so called).

Finally, is it necessary for the purposes of the practitioner to analyse the residuary resistance or power into its component parts, *i.e.*, wave-making such eddy making as may exist with the naked model, and the augmentation of friction over that of the "equivalent plank," due to increase in mean rubbing velocity? This resistance (or power) on account of both eddy making and increased friction, would seem to depend on the velocities and pressures incidental to the stream, *i.e.*, the ship form and near the surface they would be influenced by the wave pattern. It might be justifiable to neglect the error committed in assuming them to vary with the Froude law, just as Admiral Taylor allowed for eddy making by adopting the slightly larger frictional coefficient of Tideman. It is admitted, of course, that the ingenious and refined method proposed by the Author for the experimental determination of "n" applicable to the given model would as nearly as is possible yield the ideal solution.

The law of similitude for friction being established, we could proceed experimentally and theoretically to the further problem of the "form factor" which, combined with relative length and wetted surface per ton,

Mr Liddell

governs the relative frictional resistance, a problem which has so far not received much attention owing to the difficulties in the way of extending model results to actual ships. This accomplished, the way might be found open to the combination of the "frictional form factor" and the principal "wave-making feature" and thus to the determination of a single geometrical "resistance characteristic."

In conclusion I wish to express my sincere appreciation of the suggestive and informative work done by Mr. Telfer in his Paper.

Mr. Fletcher

MR. ALFRED FLETCHER, *Graduate*: There are several points that one would like to mention, but I proceed at once to Appendix 3, which presents to me the most interesting part of the Paper. I think the method by which Mr. Telfer proceeds to determine the value of n is particularly elegant. There is an equation after 26 which is not given any number which refers to the power due to frictional resistance. Now, I take it that that is based upon the work of Dr. Gebers, and I have calculated some resistances based upon this and other experimental presentations. The resistance, as calculated on length of 400 feet, speed of 10 knots, and wetted surface of 33,200 square feet, according to the formula given here would be 15,850 lb. Mr. Baker has given a curve of frictional resistance at the end of his book; using that curve gives a resistance of 17,340 lb. If one allows for laps and butts, that amounts to 19,100 lb. The original results given us by Froude give the resistance for the same ship at the same speed, of 19,600 lb.; so that the authorities on frictional resistance give us varying values which range between 15,850 and 19,600 lb. In face of this one wonders what actual real value one should take.

VOTE OF THANKS.

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E.: We have had a most excellent evening. I have felt recently that the engineering side, and possibly the Diesel-engine side, has had rather an unfair preponderance, and that in recent Papers read before the Institution the engineers were rather taking the lead in putting forward complicated formulæ. But to-night the shipbuilders have got it back, and it is interesting to think that there is such a variety of opinion as to how to determine and how to criticize these tank-model results, and that it is considered they might be given in much simpler forms, considering the many calculations that have to be taken into consideration in the early stages. We engineers are somewhat in the same position as the shipbuilders in regard to using terms for expressing power, a difficulty which I hope we shall be able to overcome, for we have no fewer than six ways of talking about the power for driving a ship. I hope we shall be able to stop that, and eliminate this complication.

The President then moved a vote of thanks to Mr. Telfer which was enthusiastically carried.

CORRESPONDENCE.

Mr. R. E. FROUDE: I am much obliged for your letter, and for the very able and interesting Paper which you enclose, a large part of the information in which is in fact quite new to me. Partly for this reason I do not attempt any discussion of the Paper as a whole and confine my remarks, herewith enclosed, to what I take to be a misapprehension, on the part of the Author, of the considerations which led to the expression for the Resistance Constant (C), of the Admiralty Experiment Works, taking the form which it does. Mr. Froude

In the second paragraph of page 224 of this most interesting and able Paper, it appears to be surmised that in "the Froude expression" for (C), the presence of V^3 in the denominator and the consequent flattening of the power curve, was specifically designed, and is valued, merely on the score of convenience in fairing the curves given by the experimental results. I therefore think it advisable to remark that, while this convenience for fairing purposes is indeed an incidental result attaching to the use of the adopted form of (C), the formula for (C) was in fact devised for quite another purpose, namely, one which to my thinking has, for experiment tank work, far greater importance. This purpose is, that for all varieties of hull form (of reasonably ship-shape character at least), and throughout every reasonable range of relative speeds with those forms, the various ordinates obtained (*i.e.*, the (C) values) should vary only within such a moderate range of absolute value, that all alike can be plotted on one and the same scale, and be read off on that scale, with serviceably equal percentage accuracy.

According to my recollections of our work both at Torquay and at Haslar, this purpose may be said to have been quite unexceptionably fulfilled, and I am inclined to go further and say that I find it impossible to imagine how such work as we had to do there could be successfully carried on, except on those terms. Whether for the wider review of conditions, which appear to me to motive the Author's treatment in this Paper, the same considerations equally apply, is another question, and one on which I am unwilling to pronounce any confident opinion on the basis of my present knowledge.

General GIUSEPPE ROTA (Royal Italian Navy): Mr. Telfer's Paper belongs to those which deserve special attention. His ideas, which are presented with exceptional skill, have as their aim the increased dissemination and comprehension of the results obtained by experiments with ship models. The attainment of that aim would certainly benefit the experimenters, the practitioners and the research workers, as Mr. Telfer classifies those who are interested in these studies. General Rota

The presentation by a standard method of the dimensions and the peculiar elements of ship forms and resistance or power data will be

General Rota

admitted by every one to be desirable. I think that no difficulties could arise from Mr. Telfer's proposed units, that is, from the adoption of the metric system, which is used by most of the experiment stations, and, for velocity, the knot.

Considering the method of presentation, we must distinguish between the presentation of model dimensions and the presentation of their experimental results. The Relative system as applied to the dimensions data corresponds to what has been in use since 1889 in our experiment tank in Spezia. It consists in expressing the relative values for length, breadth and draught by the ratios $\frac{L}{\triangle^{\frac{1}{3}}}$ $\frac{B}{\triangle^{\frac{1}{3}}}$ $\frac{T}{\triangle^{\frac{1}{3}}}$, and I would add that, in the official reports on the experiments made at Spezia, apart from other complementary geometrical particulars, according to the well-known Froude's Constant system, is also given a complete plan of the transverse sections of the hull, presented according to the Relative system as above said, so as to simplify the drawing of the same at the required size.

Following Mr. Telfer's proposal for presentation of resistance and corresponding power, Froude's law is evidently out of the discussion; for velocity Mr. Telfer suggests the adoption of the ratio that was applied in 1911 by Professor Scribanti, that is to say $\frac{V}{\triangle^{\frac{1}{6}}}$, and it seems to be preferred in comparison with the same in Mr. Froude's Constant system. For the fraction of e.h.p., which depends upon wave-making, Mr. Telfer suggests the ratio $\frac{P_w}{\triangle V}$, or better $\frac{P_w}{\triangle^{\frac{7}{6}}}$, which is certainly simpler than the others in use and also to be preferred to the similar ratio of the "Constant system" which we have used since 1889. Consequently we could follow Mr. Telfer's proposal of presenting the wave-making power as the relative value, if no difficulties arose for the presentation of the other factor of the total power which depends upon the frictional resistance. Here uncertainties and divergences begin: uncertainties and divergences which arise from the deficiency of available elements and, above all, from the hypothetical nature of the data for calculating the frictional resistance, or corresponding power of ships of different sizes.

Till now we have heartily accepted the coefficients suggested by Froude in 1872, which were deduced from plank experiments, accepting the equality in the effects of the plank's surface with the wetted surface of the ship; but the recent experiments of Dr. Gebers would seem to produce results much different from those obtained by Mr. Froude's coefficients. Which of the two systems, both containing surmises, must we accept as the more suitable? This is a point of great uncertainty which is met with in model experiments.

In practice, no inconvenience so far has been experienced, the results being taken relatively and no absolute value given to them; it is, how-

ever, desirable to eliminate this uncertainty by making some appropriate full-size-ship towing experiments. General Rota

The calculation proposed by Mr. Telfer for the determination of the right value of frictional resistance in model experiments is very interesting, comparing as it does two similar models tried in perfectly equal conditions of temperature, etc., without the necessity of knowing in advance the value of the wetted surface or other coefficients. The same method applies also for the determination of the value of the exponent of the velocity in the formula for frictional resistance. I hold in great consideration this proposed method of calculation in conjunction with model experiments, for application at the experiment tank at Spezia.

According to Mr. Telfer's proposed system of presentation, the relative value of $\frac{P}{f}$ as applied to the full-sized ship, viz.: $\frac{P}{rf}\ \frac{P_f}{\triangle^{\frac{53}{48}}}$, assumes the acceptance of the Rayleigh law and a constant value for the coefficient in the relative formula. With this hypothetical condition, it is perfectly right, as Mr. Telfer states, that the method proposed is the only one which—as distinguished from its predecessors—does not require the wetted-surface value and other coefficients for calculating the value of $\frac{P}{f}$ to be known.

However, with the actual knowledge we have on the subject there is no sufficient reason to justify the giving up of the Froude's coefficients in preference to those of Dr. Gebers. In the meantime, both methods require a form factor to correct the calculated value of frictional resistance. Very skilful is the approximate analytic method proposed by Mr. Telfer in his article, "The frictional resistance of ships,"* taking into account the ratio between the girth of the horizontal section of the well-known Kirk's solid and the length of the same, which ratio, as Mr. Telfer states, agrees with Mr. Baker's experiments on the form correction. I think that, to elucidate this important question, towing experiments with ships at small velocity would be very interesting. The slowest speed is necessary in order to find by a method similar to those adopted in model experiments, the practical true value of the frictional resistance which, in these conditions, can be considered equal to the total resistance value obtained in the towing experiments.

By that means much useful data could perhaps be obtained for the correction to be introduced eventually in the usual formula for frictional resistance, or it might even be possible to have a new formula in order to eliminate the above uncertainty. Towing experiments on a large scale might also be made in order to establish the actual, in comparison with the calculated resistance valued at different speeds. Such full-sized experiments have been advocated for some time. Evidently they would require much labour and would prove very expensive, but considering their general character and their importance to naval architectural research, they must depend upon state means for financial support.

* *Marine Engineer and Naval Architect*, October, 1922.

General Rota

In conclusion, if the above-said uncertainty is overcome we agree with Mr. Telfer's proposed method of presenting relative-power values.

With regard to the proposed graphical method for calculating wave-making and frictional-power, I must add that the adoption of the "Quadro Calcolatore" prepared on Mr. Denholm Young's general basis—which was presented by him before this Institution in 1893—seems to be preferable. The "Quadro Calcolatore" is greatly in use in Italy and is accompanied by an auxiliary scale in order to introduce also graphically the well-known frictional-correction, thus taking into account the influence of the length of the ship in applying Froude's coefficients. Such a "Quadro Calcolatore" may be used eventually, adopting Mr. Telfer's method, for calculating $\frac{P}{f}$ also since a new logarithmical scale could he added to that for the calculation of P_w, the latter having the modulus equal to $\frac{6}{7}$ that for displacement $\triangle$ and the former $\frac{48}{59}$ of the same.*

Before concluding this brief contribution I very gladly take the opportunity to express my admiration for the interesting paper presented by Mr. Telfer, and for the mass of work he produces in the field of Naval Architecture.

Rear-Admiral Taylor

REAR-ADMIRAL D. W. TAYLOR, U.S.N. (ret.): Mr. Telfer's Paper deals with an important but very difficult subject. As one of the earliest offenders by departing from the classical Froude system still so much used in England, I welcome the opportunity afforded by the courteous invitation of your secretary to contribute briefly to the discussion.

In the first place I agree entirely with the Author that model-basin experiment data should be presented in a form which considers the practitioner rather than the research worker. That was the essential reason why, at the U.S. Model Basin, we felt it necessary to adopt

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* In my book: *La vasca per le esperienze di architettura navale*, edited in 1898, and firstly in my paper: "Determinazione grafica delle curve di propulsione di navi simili nella sagoma della carena—Correzione grafica d'attrito," in *Rivista Marittima*, 1893—I exposed a complementary method to that of Mr. Denholm Young for obtaining graphically, according to Froude's rule, the correct P_f value as is done for the P_w value.

The well-known ratio:

$$\frac{P^1{}_f}{P_f} = \frac{K^1}{K} = \left(\frac{L^1}{L}\right)^{3\cdot4125}$$

for two ships of lengths L^1 and L and equal relative velocity, or

$$\frac{P^1}{P} = \left(\frac{K^{1-3\cdot4125} \cdot L^1}{K^{-3\cdot4125} \cdot L}\right)^{3\cdot4125} = \left(\frac{\mathscr{L}^1}{\mathscr{L}}\right)^{3\cdot4125}$$

makes it possible to add on the upper part of the "Quadro Calcolature" a logarithmical scale whose modulus is 3·4125 times that for P_w value, putting on the same the various subdivisions for L values instead of the corresponding $\mathscr{L}$ values. The tracing-curves for P_w and P_f values for a ship of dimensions Δ and L being prepared on a transparent piece of paper and completed by placing the index for Δ, L values on the ordinary corresponding scale and L value on the conventional $\mathscr{L}$ one, the determination of $P^1{}_w$ and $P^1{}_f$ values becomes very easy and is effected by putting the transparent figure on the right position on Δ^1 value. By Mr. Denholm Young's method, the $P^1{}_w$ values are read immediately both to the velocity and the $P^1{}_f$ values before displacing the movable transparent chart in the direction of the P scale, till the superposing of the index for length on the $\mathscr{L}$ scale to the new L^1 value.

simpler methods than the Froude system. A genius like Froude, making research his life work, can handle results in a manner impossible or very difficult to the average naval architect, dealing with questions of resistance and propulsion only at intervals. Rear-Admiral Taylor

The U.S. Act of 1896 authorizing our Model Basin required specifically that experiments should be made at cost—without overhead—for private shipbuilders, and in order to encourage our shipbuilders to use the model basin it was desirable that our methods should be readily followed by the practitioner. It should be considered, too, that twenty-five years ago the model experiment method by no means had the standing in the estimation of the average naval architect that it now rejoices in.

The difficulties of establishing common international units or systems dealing with model experiment work are the same in kind, though not in degree, as the difficulties of establishing a common international system of weights and measures and, briefly, grow out of the fact that there is a past as well as a present and future. A new establishment, however, can start with a clean slate and if there is some common agreement on the most desirable system of notation even the long established institutions will probably fall in line in the course of time. This Paper, bringing together for comparison so many systems, differing sometimes fundamentally and sometimes trivially, will be very helpful towards bringing us all upon common ground.

Some of the suggestions put forward, meritorious as they are from the point of view of uniformity, need very careful consideration from other points of view. Take, for instance, the proposition to use $\frac{V}{\triangle^{\frac{1}{6}}}$ as the dimensionless quantity involving speed instead of $\frac{V}{\sqrt{L}}$. The latter has the great practical advantage that in a given case it involves the two things which most affect the resistance. Curves of resistance are not fair curves. The "humps" and "hollows" due to wave resistance come close to the same values of $\frac{V}{\sqrt{L}}$ regardless of displacement; while if we eliminate L and come to $\triangle$ I fear we should soon be struggling with auxiliary diagrams or calculations bringing in L, a much more vital factor in the practical resistance problems of the practitioner than beam and draught, which have equal weight in $\triangle$.

There is something to be said for the proposal to drop resistance as a primary thing to be calculated, but there is something to be said against it. In the first place as long as resistance is what we measure it is practically necessary to spot and plot resistance first. This is also necessary in order to separate the frictional and residuary resistance. It is unfortunate that residuary and frictional resistances follow different laws. As long as this is the case, from the point of view of the practitioner it is probably simpler to calculate our frictional resistance always from our somewhat unreliable coefficients and assumed powers of

Rear-Admiral Taylor

speed rather than apply a law of comparison which has lost the beautiful simplicity of the law applying to residuary resistance.

Although the application of model resistance is mainly to determine curves of horsepower, such curves vary too rapidly with resistance to be comfortable curves to handle, particularly for high-speed vessels. For such vessels and curves of horse-power on any practicable scale the ordinates are objectionably small at low speeds and the curves are objectionably steep at high speeds. For some purposes it is almost necessary to reduce such curves by dividing V^3 as in Froude's notation or by some similar device.

All things considered, the Author's proposition to make e.h.p. the primary thing to be characterized in connexion with a model resistance is sound and his proposed basic expression $\frac{\text{E.H.P.}}{\triangle^{\frac{7}{6}}}$ is almost the ideal one for the purpose. It has the objection, however, that except for fast vessels at their upper speeds our powers would be represented by numbers less than unity and in many cases by quite small decimals. This is repugnant to the average man, possibly because for so many centuries man's fingers were his only computing machine. It is probably this human characteristic that has resulted in our having tables of units instead of single units. Why should we have inches, feet, yards, furlongs, miles, all to measure length, when from a scientific point of view, one unit would meet all requirements? I suggest that the Author consider the advisability of adopting as his basic expression or unit $\frac{\text{E.H.P.}}{\triangle^{\frac{7}{6}}} \times$ some convenient power of 10. The power of 10 to be such that the actual numbers we have to deal with will be seldom less than unity.

I should like very much to see this Paper extended to cover the propeller field. Speaking for myself, ship-model information from tanks of different nationalities has always seemed more easily translated to a familiar system than results of propeller experiments. Standard and uniform methods of dealing with propeller-model research would involve accurate definitions of such things as slip and pitch, as well as many others not so controversial.

Aeronautic developments have enormously extended the propeller field and our aeronautical engineers are liable to ignore the time-honoured practice of the marine engineer. In trying to analyse recently a very valuable experimental investigation of aeroplane propellers, I found myself completely at sea for a time until I discovered that the Author had defined effective horse-power as the power delivered to the propeller instead of power delivered by the propeller. If we ignore the previous practice in the marine field this definition is entirely justificable.

It would seem that in the propeller field there is need for clarification of notation and symbols, not only internationally, but between the aeroplane engineers and the marine engineers in each country.

Mr. G. S. BAKER, O.B.E., *Member*: It is a little difficult to see how to begin on this Paper. Mr. Telfer has dealt with his subject from two standpoints—internationalization of results and the reduction of the laws that govern resistance to trite formula—and the Paper is intended to cover simply ship-model-experiment data. Perhaps the best way to deal with the first part of the Paper is to explain what has governed our own procedure, and to work from this end. Mr. Baker

In 1911 it became necessary to fix the manner in which model resistance data should be presented before the various institutions. It was decided to give this in the form of (C) to a base of (K) and later to a base of (P), the (C) being correct for a 400-foot ship, using Froude's skin-friction data to obtain the skin-friction correction.

The data is then independent of size and can be rapidly converted into power for ship of any dimensions. Comparisons of forms can be still more rapidly made. Special features can be brought out in a way that cannot be achieved by any method departing seriously from that adopted. We had in mind this also, that every man *trained* to naval architecture understands these constants, and if he does not it is better for him not to meddle with model data at all. Froude, the Froude Tank, Semple, and more recently even (1920) Robinson of the Washington Tank use (C) for their model data. There is a vast amount of data in this form, and any other system must carry most serious and obvious advantages if it is to be adopted. If it is required, let us say, to alter the bacteria contents of the various grades of milk sold, it is easy to do it, as milk is a consumable article and the old is soon replaced by new. But ship data is not consumable, it must be altered or confusion will result between the old and the new systems.

In 1914-15 this matter had to be considered again by us. Research had shown better methods of working out skin-friction correction, and the unreliability of data used in some cases. It was obvious that if we changed, everything would have to be changed, and absolute units adopted. Now in recent years this question of absolute units has fortunately been considered in some sciences before data took concrete form. Absolute units have been adopted in aeronautics throughout the world, and these data can be used with any complete system of units whether C.G.S. or foot, lb., second. These units are also percolating into other sciences, and we considered it would be putting too much upon our fellow man to ask him to adopt a second set of units to express the same things, just because we deal with a different fluid. No scientific man would listen to it. But at that time we were comparing friction in air and water, and were necessarily driven to find a common basis for comparison, and adopted the absolute units in use in aeronautics in presenting the results. So far as I know this was the first occasion on which absolute units were used in ship data. Comparing this system with that suggested by Mr. Telfer, the latter is seen to have

Mr. Baker

obvious defects. There are two units of length in his system, the metre, and the foot used in his velocity term. The ton is a movable feast, already having different meanings in England, France and America, and Mr. Telfer proposes to give it still another by prefixing " metric " before it. This juggling with words may satisfy his mnemonic instincts, but is intensely confusing to the ordinary mind. Moreover, it requires a little more argument than has been used to persuade one that expressions of the form here used, viz. $\triangle^{\frac{53}{48}}$ are "without much arithmetical unravelling " an attribute claimed for the system by the Author

Passing to the expressions proposed, one is reminded irresistibly of the efforts of the *Daily Mail* a few years ago to find a hat that would suit everyone on all occasions in all weathers. It failed, of course, and the suggestion that model data shall be expressed in one term, $\frac{\text{E.H.P.}}{\triangle^{\frac{7}{6}}}$ must fail for the same reason. We have different things to express and constants to bring out these features must be used. You cannot replace V by $\triangle^{\frac{1}{6}}$ with impunity in expressing model results. The result is dimensionally correct but is liable to be useless, and may be used inaccurately.

The trouble here brings one back to the origin of the Author's effort. The real reasons at the root of the complaint about diversity of presentation are either a desire to avoid work, or a want of knowledge of how to compare the data. The latter is the real trouble, and should be amenable to treatment. All tanks should work on common friction-correction data, and correct their data to a definite temperature and ship length. If this were done one would hear very little more on this point.

Finally, there are a few points in the appendices dealing with the laws of resistance. First, with regard to Appendix 1, in my book on Ship Resistance published in 1914, Froude's Law is dealt with from considerations of dimensional units. Second, with regard to Appendix 2, I think I am right in saying that Rayleigh stated his law in this wise: " *If* the frictional resistance may be expressed " . . . . then the result is as the Author gives it. Stanton and Pannel dealt with this in a fuller manner in 1914, and the first part of the Appendix of my 1915 Paper was based on their work, which went to show that so long as $f\frac{vl}{v}$ was the same in any two fluids the type of motion would be the same, and so to deduce the Rayleigh law.

The Author has not mentioned these three English authors in his Bibliography.

In Appendix 3, if I have understood it correctly, an assumption has been made which is not exactly correct. The Author tacitly assumes here that " n " is the same for f and F. If it is so, his result holds good, but this does not represent our experience. The law of skin-friction variation with speed and dimensions shows a marked variation even when the size is merely doubled. Although the suggestion here made

is most ingenious, I think it must fail for this reason. It would give an " n " value but it would not be what it purports to represent. Mr. Baker

Finally, with regard to the statement that " n " has a value of 0·125. So far as I know, every bit of data on friction of planks has been analysed here, and all of it shows an " n " value of 0·14 at the upper limits of length and speed, decreasing to nil for low speeds and short lengths. Gebers' data was included in this analysis, and showed no cause to depart from the general result. Lee's paper before the Institution of Naval Architects might be read with advantage in this connexion, as it deals with the probable variation of " n " in going to ship lengths and forms. This paper also might be added to the Author's Bibliography, so that the fact that these matters were first and correctly treated by English authors shall be presented to one seeking for further data.

Dr. Ing. GEBERS, *Director of the Vienna Experiment Tank* : I congratulate Mr. Telfer on his most valuable Paper and I think that he has given us a very interesting method of effecting the internationally uniform presentation of ship-model experiment data. Also I congratulate him on his attempt, as an Englishman, to adopt the metric system and, with regard thereto, I should like to ask him to advance still further than he has ventured at present. Dr. Gebers

If the " metre " is already adopted as the unit of length, it is quite a natural consequence to express the speed in m/sec. and not in knots. Moreover three reasons may be given for this proposal. Firstly, knots as used in England and on the Continent are not of the same value; according to the British system the knot represents 1,853 m/hour (6,080 ft./hour) while the Continental knot is equal to 1,852 m/hour. Secondly, in the future, model *propeller*-experiment data should also be represented if possible in a manner consistent and uniform with the *ship*-model-experiment data; so that we must take into consideration this point in fixing the uniform presentation of the ship-model-experiment data. For propeller-experiment data, I think the unit of the length must be of such character as to be applicable both to the *speed* of the model and the *pitch* of the propeller. Thirdly, the speed of the river boats is not expressed in knots, but always in km. per hour on the Continent and often in statute miles per hour in England. In the Continental experiment tanks, we are making a great many model experiments for ships and propellers for river service even if this may not be the case in British tanks. Also, I think, it sometimes will be necessary to adapt the water experiment data for use in aeronautics, so that we should also take into consideration the usual unit of speed adopted in aeronautics which is expressed in km./hour or m/sec. on the Continent.

I should like to propose a few minor alterations to the symbols proposed by Mr. Telfer. He has selected the one-ton model, *i.e.*, the model whose weight is one ton, as the basis of presentation. However, it seems to me that, instead of the one-ton model, the one-cubic-metre model—

Dr. Gebers

more shortly, m^3 model—which means the model whose displacement is one cubic metre in volume, is more convenient for our purpose. The adoption of the one-ton model as the basis will often affect the comparison of various forms of hulls, because in almost all experiment stations the tank is filled by fresh water and the draught is dependent upon the specific gravity of water. For the experiment tank, for the river, lake, and inland sea (Baltic sea) as well as for the ocean, the size of one-ton model of a ship is directly proportional to the specific gravity of the water. Although the mistake due to the comparison of not strictly similar draughts and consequently not strictly similar volumes will be practically compensated by the increase of resistance which is proportional to the increasing specific gravity of the water, it seems to be better either to adopt the above-mentioned proposal of m^3 model or to define the one-ton model as "one-ton model in fresh water."

Further, I do not like to omit from Table I. and from the basic formula the expression, relative resistance. In my opinion, the resistance is rather more important than the horse-power. We use the resistance for the design of the propeller and we measure with it the power of a tug boat as measured at the tow rope. The symbol for relative resistance may be R_{rw} and R_{rf} and the relation $\frac{R_w}{\triangle}$ and $\frac{R_f}{\triangle^{\frac{15}{16}}}$ for the wave-making and frictional resistance respectively.

Finally, it seems to be unpractical to denote the power which is necessary to overcome the resistance of the naked hull without propeller by the symbol e.h.p. *i.e.*, "effective horse-power" because it might be easily misunderstood as the effective horse-power of the machinery even if the latter is often called the shaft horse-power. Consequently I have long used the words "Schlepp-Pferdestarke," but I admit that the determination of the short symbol for it is not an easy matter because either "S" or "Sch" might be understood as representing "Schraube." For the purpose of obtaining international recognition, it will perhaps be best to adopt one of the symbols "P_t," "HP_t" and "THP" which corresponds to the English word "Tow," to the French "Toueur," to the Italian "traina" and to the German seamen's word "trekken" (ziehen).

The remaining parts of Mr. Telfer's Paper are quite acceptable and I hope that some supplement dealing with the case of model-propeller experiments will be made in the near future and that a general international agreement on such method of presentation will be decided upon. The proposed simple alignment chart representing the fundamental relations deserves also the highest appreciation.

I should have liked to discuss in detail the frictional resistance of bodies, but fear that this would take up too much space. I will therefore limit my remarks to rather secondary points because it is my definite intention to publish in the immediate future the further results of the continuation of experiments which are referred to by Mr. Telfer in his Paper.

One part of the deductions given in Appendices 2 and 3 was previously pointed out by me in another form in "Werft und Reederei."* The method proposed by Mr. Telfer of determining "n" by the measured resistances of various similar bodies was also published in 1908 in "Schiffbau"† by me. Dr Gebers

I think that the small tank in Teddington is too small for the purpose of such experiments. The model for this purpose must be of so large dimensions that the laminar state of friction cannot exist and if we adopt a large model, the influence of the sides and bottom of the tank will come into existence because the stream-line motion of the water along the side of the model will be effected by them and even in a large tank such influence will exist much sooner than it has been usually expected.

However, all these objections are of no importance in comparison with Mr. Telfer's valuable Paper and I think we must express our unreserved thanks for the publication of his valuable work.

Dr. Ing. G. KEMPF, *Director of the Hamburg Experiment Tank* (Translation): Mr. Telfer's Paper is worthy of considerable attention and I would say at the outset that the Hamburg Experiment Tank would be quite prepared to adopt the Author's proposed, or a similar, system for the presentation of its model data provided that the system be approved and adopted by the majority of experimenters concerned. This adoption, however, is qualified in one respect by an objection I would raise with regard to the significance of "relative" velocity. Granted that a practical system should be formed of internationally recognized and the simplest units, it is yet moreover necessary to preserve the physical significance of the "relative" expressions in order that the best practical comparison can be effected. Dr. Kempf

The Author's expression for relative speed $\frac{V}{\Delta^{\frac{1}{6}}}$, whilst admittedly international seems to me to be too great a concession to the simplicity of the system. The physical significance of relative velocity lies in the fact that a definite relation exists between the length of a ship and the length of the wave produced by it. This relation is expressed by the ratio $\frac{\text{wave length}}{\text{ship length}}$. As the wave length is proportional to V^2 this expression reduces to the forms $\frac{V^2}{L}$ or $\frac{V}{\sqrt{L}}$. These alternative expressions as pointed out by the Author have been widely and internationally adopted with, of course, different units for the length L. In spite of the numerical difference, I do not consider it desirable that this simple physical significance should be obliterated by the substitution of $\Delta^{\frac{1}{6}}$ for $\sqrt{L}$. In any case 1-ton models of different lengths also have different relative velocities for the same absolute velocity. The ship's length in a decisive criterion for relative velocity for dissimilar ships.

* Werft und Reederei 11. Jahrg. 1921, Heft 3 Seite 68.

† "Schiffbau" lx. Jahrg. Heft 12 und 13.

Dr Kempf

So long as units are not international, whilst I do not fail to appreciate the difficulties which oppose the adoption of $\frac{V}{\sqrt{L}}$, yet I would be sorry to see the expression $\frac{V}{\triangle^{\frac{1}{6}}}$—which is admittedly suitable for the economic comparison of similar ships—adopted for the general presentation of experiment data.

For the presentation and calculation of frictional horse power, from practical considerations, one must be provisionally content with the use of the expression $\triangle^{\frac{53}{48}}$. However, one must remember, as pointed out by the Author, that this value is based upon experiments with thin planks of lengths up to 10 metres and speeds up to 15 knots. In this connexion it is very desirable to extend research to ship-shaped forms of greater lengths and speeds. With regard to the method given by the Author in the Appendix, for the determination of the frictional resistance of a ship-shaped solid by means of similar models of different scales, experiments were carried out over a year ago by Herr Brackhoff at the Hamburg Tank on different types of ships, plates and rotating discs of different scales, and their frictional resistance calculated by this method. It was found that the velocity exponent n is not constant and evidently depends upon the value of the speed-length ratio $\frac{V}{\sqrt{L}}$. However, this at present is only a matter of research and in the present state of knowledge I regard Mr. Telfer's proposed relative system of the presentation of ship model experiment data as both useful and important.

Professors Sadler and Bragg

PROFESSORS HERBERT C. SADLER AND EDWARD M. BRAGG (University of Michigan): Mr. Telfer is to be congratulated in bringing into prominence the thoughts that have been in the minds of many experimenters, as well as naval architects and shipbuilders, for many years. A vast amount of valuable data in connexion with resistance and propulsion of ships has been published during the past ten to twenty years, but unfortunately the busy shipbuilder or designer has not always found the time to "convert" this data into such a form that he can understand and apply it to his own problems.

The Author has laid down five attributes that any system of recording data should possess; but while all of these are certainly desirable, some of them would appear to be of far more importance than others. For example (4) "obvious physical significance"; (3) "immediately available for practical use," and (5) "moulded for the convenience of the practitioner," should perhaps be given as much or even more weight than some of the more idealistic attempts. It was something similar to the above that led the writers to suggest the use of e.h.p./ton plotted on a speed-length ratio base, which, while not conforming to the non-dimensional condition, seemed to be well within the limits of accuracy for all practical purposes.

Professors Sadler and Bragg

A point that is sometimes overlooked, and which after all is very obvious, is the fact that any particular model with a given ratio of dimensions can apply in practice to only a very limited size of ship. For example, vessels of nine to ten beams to the length will be found only in the larger sizes, and a system which would demand that the qualities of such models should be presented in such a shape that they could be applied to vessels of, say, one to two hundred feet in length, would seem to be unnecessary, particularly if the presentation became thereby unduly complicated. One difficulty in the past has been that experimenters have been prone to present their data in their particular "pet" form, based usually on sound principles, but after all, framed more for the convenience of experimental analysis than for immediate use by the practitioner.

"Physical significance" may be illustrated in the choice of the speed co-ordinate. Speed and *length*, not speed and the side of a cube having the same volume as the ship, are the two factors that present a real picture of what actually occurs; for example, in length of bow wave, combination of crests and hollows of the stern, etc. Also, if one wishes to compare two vessels of the same length, but of slightly different displacements (a condition often occurring in the early stages of design) or the same vessel at varying drafts, the fact of a varying speed co-ordinate in each case might tend to confusion. This same objection has been raised to Mr. Baker's (P) value. From the above it would appear that the form $\frac{V}{\sqrt{L}}$ would be preferable, and its conversion from the metric to feet units a comparatively simple matter.

Until the recent paper, it was our custom to present the results of our Tank experiments in the "Taylor" form, which in many ways possesses all the qualities of simplicity and means of comparison that the average designer wishes. It must also be borne in mind that in any case, the surface friction calculation, even in the method suggested by the Author, must sooner or later be introduced, so that the extra arithmetic involved is more or less negligible. This brings up a point which should be strongly emphasized, namely, the subject of surface friction. We have at the present time many methods and coefficients for estimating this factor, also some question as to its variation with temperature. If there could be some international agreement in this matter, it would certainly eliminate some of the difficulties that occasionally occur in comparing the results as presented by different authors. We do not *know* what the surface friction of say, a 400-ft. ship is, and yet many total effective horse-power curves have been published for this or other sizes of ships without a statement as to what coefficients, corrections for contour, allowance for butts, etc., or temperature, if any, have been used.

It might easily happen that the cumulative effect of these differences would amount to more than the difference in two models due to form.

Professors Sadler and Bragg

Presumably all tanks have made their own experiments upon friction of planes, so that, based on this information, it should not be difficult to obtain fairly consistent results for the residuary resistance, provided all used the same method of estimating friction.

Until this is done, it would seem hopeless to suggest any international method of plotting or presenting e.h.p. curves, and perhaps after all the more simple form of residuary resistance in pounds or kilogrammes per ton of displacement (or the same in power units) would appear to be the most desirable, because to get this information is *as a matter of fact* the main reason for experimenting with ship models. Fig. 1 of the Paper has therefore this advantage at any rate so far as the ordinates are concerned.

With regard to other factors that would identify a ship's form, the Author's suggestion of the ratio of length to the cube root of the displacement is perhaps as good as any, provided also that the usual ratios of length and breadth, and breadth to draft, as well as all coefficients of form, be always stated.

The further suggestion in connexion with appropriate symbols is well worthy of serious consideration by all nations, and appears to cause, on the whole the minimum amount of change; but it is an open question whether it is desirable to have a system of plotting or presenting results which must conform to both the British and metric systems. After all, if results are presented *in the same form* any conversion from one system to another is usually a very minor matter.

Mr. Mumford

Mr. EDWIN R. MUMFORD (of Messrs. William Denny & Bros., Ltd.): Mr. Telfer's aim and endeavour are to devise a system of presenting ship-model-resistance results which is to be such an improvement on other methods and so international in character and nomenclature, as to command general adoption. He assumes that the adoption of such a system as he advocates will be very convenient for practical manipulation, though it need not prevent the use of "other systems in order to accentuate pictorially some particular phenomenon."

Mr. Telfer has dealt very exhaustively with various systems and expressions, and is to be commended for the thoroughness of his work and its high purpose. It is doubtful if he will succeed in accomplishing his object. It has never been found an easy matter to cause the universal adoption of one language, or of one method of recording results for reference: it is still less easy to persuade those who have accumulated a very large stock of data in one form, and one nomenclature, with which they are therefore thoroughly familiar, to adopt an entirely new system. If such a system as Mr. Telfer proposes were recommended and presented by an international conference of representatives of the various technical societies, it might perhaps ultimately become general.

There are many good points, and some sound argument, embodied in the Paper, and good reasons are given in support of the "Relative

System," and of the nomenclature proposed. It is possible that too little attention is sometimes given to the consideration that the surface friction-power of similar ships at corresponding speeds does *not* vary in the same way as the wave-making power (*i.e.*, as $\triangle^{\frac{7}{2}}$), and it should therefore prove helpful to have the wave-making and frictional horse-powers presented as in Fig. 1, along with the displacement index to be applied to each. Mr. Mumford

The results of any particular form, by whatever method presented, can only be applied with accuracy to similar forms of various magnitudes. Serious miscalculations have occurred from failure to realize the varying effects of varying dimension ratios, so that, while it is without the scope of Mr. Telfer's Paper to deal with these effects, yet it may be pointed out that the adoption of such a system as he recommends facilitates the determination of such variation from available data. It will be found, from docketed results so treated, that rate of variation of power in terms of varying beam, draught, or fineness of lines, covers a wide range of indices for each of these variables. The actual rate of variation of P_{rw} in terms of variation of beam, or draught, depends on the ratio of each to the other dimensions and also on the speed-length ratio. The rate for varying fineness of lines of the same character and distribution depends on the speed-length ratio.

It is not improbable that many would find it inconvenient to use Mr. Telfer's expression for "Relative Power" on account of the indices $\frac{7}{6}$ and $\frac{53}{48}$, and it would simplify their application to construct curves for general use covering a wide range of numbers raised to these indices.

Prof. T. B. ABELL: This is a very valuable Paper. The statement of requirements of a system of notation is full, but might be enumerated in a different order. The writer would place them as follows, (1) (4) (3) (2) whilst (5) might be omitted as being embraced by (3). Internationally, it is more important to have a common form of expression than to have common units. With a common form of expression where each symbol has a physical meaning, characteristics of any particular set of experiments can be seen at once from a graphic representation of the results. Actual evaluation of the graphs is after all a simple matter of arithmetic and can be deputed to computers. Common units will not help much in this particular instance until a common system of units be universally adopted. Professor Abell

Mr. Froude's practice was to place on the (C) curve to a base (K) a scale of (L) so that variations of (C) with speed might be assessed in relation to $\triangle$ (as involved in (K)) and in relation to length (as involved in (L) $= C\frac{V}{\sqrt{L}}$. This scale of (L) was most useful and more so than (K) because it did bring out the features of wave interference of the bow and stern systems of waves. We now know that length without reference

Professor Abell to fullness of form is not sufficient to interpret the characteristics of wave interference and Baker has suggested the use of (P) $a \frac{V}{\sqrt{PL}}$. The writer feels that this expression, although its physical interpretation is not perhaps definite, should be commonly used for the abscissa of speed because it does seem to give greater uniformity of type to the (C) curve than when plotted to a base of (L) or (K). It has all the requirements stated by the Author. The suggested uniform use of $\frac{\text{E.H.P.}}{\triangle^{\frac{7}{6}}}$ can hardly be improved upon for calculation purposes, but personally the writer thinks the factor of speed should be included explicitly and $\frac{\text{E.H.P.}}{\triangle^{\frac{2}{3}}V^3}$ used in recording experiment results.

The writer prefers the proof of the law of comparison by Rayleigh's theory in which viscosity is included with the wave-making. By the method indicated in the Appendix it can be shown that—

$$R = \rho v^2 l^2 \phi \left(\frac{vl}{\nu} \quad \frac{gl}{v^2} \right).$$

For it brings out very clearly that in order to apply the law to ships it is necessary to assume that the total resistance may be split up into two components so that—

$$R = \rho v^2 l^2 \left[f_1 \left(\frac{vl}{\nu} \right) + f_2 \left(\frac{gl}{v^2} \right) \right] \quad \ldots\ldots\ldots\ldots (a)$$

i.e., the ordinary assumption made that the total resistance equals the sum of the skin friction and wave-making resistances.

In the discussion on the Paper which G. S. Baker read before the Institution of Naval Architects in 1916, the writer suggested that, given the total resistance curves of two models over a large range of speed and assuming the law above (a) to hold, it is practicable to determine the components of the resistance curve. Would not such a process enable us to progress further than by continuing to apply the equivalent-plank supposition? Only the arithmetic involved and the knowledge that where a method depends upon differences very great accuracy is needed, deters one from facing the problem.

Mr. Ainslie MR. A. F. AINSLIE, *Member*: The problem of ship resistance is a very important one, and the prediction from model experiments most fascinating, and all who have to make use of ship-model data will sympathize with Mr. Telfer in his desire for uniformity in the presentation of the results from the different tanks. As is only natural, however, each tank publishes its information in the manner that appears to those in authority as most convenient for their own particular purpose, and having adopted one system of constants or coefficients much labour would be involved in recasting all their past experience into some other system.

The real problem for the practitioner is to know which system he should adopt for his own use. This will require study and careful con-

sideration, and the method having been determined on, it is not much trouble to transform new results, as they appear, into that particular notation in which he has learned to think about the subject. Certainly much more labour is involved in obtaining the original results. Mr. Ainslie

Those who make model experiments know that the greatest difficulty is with the first plotting of the actual results, obtained as the model runs are in progress. These results must be as accurate as ever possible because they form the basis of all further analysis. If then the actual speed-resistance values were published together with the necessary particulars of the model, we would be at liberty to analyse them in the manner required for our own use. Thus, with a given model of known lines, displacement, surface and coefficients, if the total resistance were given on a speed base we could make our own deductions from these results, and apply them as required.

At the meeting of the British Association held in Newcastle in 1916, Dr. T. E. Stanton showed, from dimensional reasoning, that the total resistance of a ship at any one speed could be predicted from that of a model floating in mercury, but he pointed out there was not much hope of the experiment being made. Probably it would be cheaper to tow a large ship at a suitable speed to find the frictional resistance at a high value of $\frac{VL}{\nu}$ as Mr. Baker suggested to the Institution in 1915. As far as the practical problem is concerned that is the part which is most open to question. There is very little doubt about the frictional resistance of models; it is when we come to the frictional resistance of the full-sized ship that we are working in an uncharted sea, and although this was realized by W. Froude in 1874 we are still waiting for the full-scale experiment to be carried out.

Mr. F. H. ALEXANDER, *Associate Member*: If I read Mr. Telfer's Paper rightly, it seems to have three principal aims, viz:— Mr. Alexander

(1) To advocate an international system of symbols, units and formulæ, for use by all experimenters in ship resistance in presenting their results for publication.

(2) To show how frictional resistance may be expressed so as to avoid the necessity of calculating and using the area of wetted surface in order to make corrections when the total resistance is computed by means of the law of comparison.

(3) To show how that part of total resistance which is due to friction may be accurately separated from the said total, when desired, by means of towing experiments upon two models of the same vessel but of differing sizes.

As regards this third aim, it seems to have importance in so far as it will enable us to find out how far the fullness of form of the ship may affect her frictional resistance. Some experiments made by Mr. G. S. Baker have seemed to show that fullness may add considerably to this part of the resistance; but more light is needed on the subject.

Mr. Alexander

As regards the second aim, labour can be saved by the use of the method, which is specially applicable to cases where total resistance or total e.h.p. has been computed from model to ship, as by the usual British methods.

Some variations of opinion are likely to be held regarding the desirability of the international system which Mr. Telfer advocates. He would have presented a strong case if he had stated more clearly what advantages are to be gained by the use of such a system when compared with that of the several separate systems now in use. He admits for example that Taylor's method is a favourite with designers and yet shows that it is a very faulty method from the point of view of his suggested system.

He succeeds in showing that so many systems are at present in use, and they differ so widely, that it has become usual for a designer to fix upon a system with which a little practice makes him familiar, and he thereupon refuses to be bothered with the work involved in using any data (British or foreign) which have been presented in a different manner. One can see that there is here a loss of efficiency for which I at least am not prepared to lay the whole blame on the designer. An immense amount of valuable experimental work has been done in Germany, France and Italy, and some of that work (such as research into the resistance and propulsion of canal craft) is almost unknown in this country. Of course difference of language accounts to some extent for this, but even if that were overcome by translation there would still be the obstacle of method of presentation. The time of our designers is usually far too limited to allow them to worry out the variations, and as a result much useful material is, in a practical sense, not available to them.

I agree with Mr. Telfer that if experimenters wish to present their results on some system of their own (however good the reasons for that system) they ought also to present the results for general use in accordaice with a standardized system such as he describes.

I would also like to urge, that, as part of any standardized system, it should be the practice to publish the actual values given by the experiments, and not only faired curves which go near the actual values. This fairing of curves, although it is done no doubt with the best of intentions, may well keep out of sight phenomena which the actual values would disclose; and the explanation of these phenomena may thus be delayed for a generation. In most scientific and technical publications it is usual to give results of experiments in the double form, namely, actual values, and the curves of average or mean values deduced from them.

Mr. Hök

Mr. WILLIAM HÖK, *Member*: I have always regretted that from the very beginning, *i.e.*, from the day when Mr. R. E. Froude published his 1888 Paper, the recording of trial data has, in my opinion, developed on wrong lines. By my 1893 Paper read before the Institution I tried

to direct matters into another groove, but still I find that the development goes in the majority of cases in the direction of a "displacement basis" rather than in the direction of a "length basis," that is to say, that the majority of experimenters compare resistances (or powers) *of ships of equal displacements*, but unequal lengths and forms, rather than resistances (or powers) of ships *of equal lengths*, but unequal displacements and forms. I pointed out in my 1893 Paper that the latter method is, as regards ships, more rational and in practice much simpler to apply than the former, but I can well understand that it is an elaborate matter to change methods and re-plot experiment data, apart from it being fascinating and of value in more or less following Mr. R. E. Froude. But as Mr. Telfer so well points out, the result has been that we have now about as many methods as there are experimenters and very few of the methods are directly comparable. It is, therefore, high time that the different experimenters in some way or other agreed on a common method. I should not for this purpose propose an international conference, but I would suggest that the method of recording trial data as set forth in my 1893 paper be seriously considered as, I submit, to this very day that method is still the simplest, the soundest and the most practical method proposed and in use. Mr. Hök

Mr. W. J. LOVETT (of Messrs. Workman, Clark & Co., Ltd.): Mr. Telfer has brought ability and enthusiasm to bear on a difficult subject, and expresses an ideal all would like to see consummated. There are many difficulties in the way, not the least of which is the human element. Mr. Lovett

Froude's "constant" system of notation was easily first in the field, yet his successors found they had to break away from the methods of expression which he favoured. The naval architect in charge of an experiment tank will only acquire confidence in his work by long practice and experience, and it is the invariable rule that these men gradually adopt a method of their own in which to present their results. The tank experts at Haslar, Teddington, Clydebank, and Dumbarton have their own separate methods; and in America, with the same units of dimensions and weight, other methods are evolved. As in our country and in America so it is in France, in Italy, and in Germany. It seems therefore that there is a groove in which each experimenter can best think and express himself, and any attempt to get him into another groove, as suggested in this Paper, will almost certainly restrict his work. I believe it is not the Author's intention that each method of expression should be suppressed in favour of that now proposed, but that experimenters should finally present their results in this new way.

We should not forget that the United Kingdom and America own over 39 million tons of shipping, while France, Italy, and Germany together own only 8 millions; and, from the viewpoint of volume of work dealt with, we might expect the latter countries to give the results of their labours in such a fashion as will be readily assimilated by us.

Mr. Lovett

I think, however, that an attempt should first be made to standardize the method of presenting results in our own country and in America; and, if possible, a common method should also be determined upon for the Continental countries. We would then have a uniform method for both the non-metric and the metric countries and it should be possible to determine some simple conversion factors to enable equivalents to be quickly obtained for either system.

The main objection to Mr. Telfer's treatment of resistance is that it would always be necessary to run two models at various corresponding speeds to split up resistance into its skin and wave components, even though the value of "n" has been definitely established by a series of preliminary experiments. This is surely no advantage over present practice, especially when it must be conceded that all methods in use to determine power are only approximate.

Mr. Haver in his "Monitoria" presented a problem to naval architects which, to my mind, has never yet been successfully answered; for our present conception of resistance and power does not fit in with the results as published for that vessel, and the bulges which have recently been built on some naval and merchant ships present the same features as the corrugations on the "Montoria."

I do not like the comparative speed basis of $\sqrt[3]{\Delta}$ as speeds to my mind must be expressed on a $\sqrt{L}$ basis, especially where wave-making has to be taken into account.

In conclusion I would like to express to Mr. Telfer my best thanks for an interesting and instructive bit of work. The Paper has been well written and displays great analytical power and I have found the reading of it and the study of its various formulæ most beneficial.

Mr. McAlister

MR. F. MCALISTER, *Graduate*: I am wholly in agreement with the object of Mr. Telfer's Paper, namely, the unification of the methods of presenting tank results by the adoption of a common basis, and I think he deserves the thanks of the Institution for bringing forward in this manner a system which, if accepted by the various experimenters, would undoubtedly relieve the presentation of model-experiment results—and particularly the comparison of such results—of many of the present difficulties, and would co-ordinate the wealth of resistance-research results which have been issued by the various experiment tanks. Whilst I agree with the object of the Paper, at the same time the Froude circular system needs defence.

Lecturers on naval architecture find it somewhat difficult, after demonstrating the advantages of the Froude system of presentation, to explain why subsequent investigators have adopted a different method. For instance, for a given (c) value, this merely needs correcting for length and evaluating to determine the e.h.p. whereas the $\frac{R}{\Delta}$ of the Americans, after evaluation still requires an independent estimate of the

skin friction resistance and the whole must then be converted into e.h.p., for after all, in the majority of cases it is the power that is required. There now appears, in America, a movement to forsake the $\frac{R}{\Delta}$ method and present results on (C) and $\frac{V}{\sqrt{L}}$ basis. Mr. McAlister

In practice, once one has become accustomed to it, Froude's method of presentation is to be preferred; but for those who disfavour the method due to arithmetic complications, I advise the use of an alignment chart.

One objection to (C) values quoted by Mr. Telfer is that there seems no important justification in practical work for not making the higher powers that are required at higher speeds immediately visible to the eye of the designer. I had really regarded this flatness of the (C) curve as more of an advantage than otherwise, as, in a given (C) curve the rise in the value of (C) at the higher speeds marks that point to which the vessel may be forced without undue waste of power, or virtually separates economical from extravagant propulsion; and of course, this point is more apparent in a (C) curve than, for example, in a curve of resistance. The original form of the resistance coefficient was $\frac{R}{\Delta}$ and Froude, in order to reduce the steepness of such a curve divided that value by $(K)^2$. Thus the flatness of the (C) curve was premeditated and not merely the outcome of a natural law.

I presume Mr. Telfer is familiar with the method of presentation to which I refer, but it may interest some of the members to know that it is the practice of the experimenters at a well-known private tank to increase every model to a vessel 100 feet long, and plot the results at the rate of 100 square feet of wetted surface. The tank in question has now seen some forty years service and at the time of its inception, Dr. Kirk's analysis was used considerably in the ascertainment of power for a given speed. This method, so far as I am aware, is still in use.

In striving after unification of the form of presenting results it might be as well to strive after an agreement in methods of calculation for frictional resistance also, as apparently Baker calculates upon a different coefficient to that of, say, McEntee. In fact, the latter gentleman has shown in the technical press how a variation of $7\frac{1}{2}$ per cent. in the e.h.p. for a 400-foot ship has been made with consistent model results due to the Washington tank staff estimating upon a frictional coefficient for the 20-foot model of $0{\cdot}00969\ V^{2{\cdot}854}$ whilst Baker used the Froude coefficient of $0{\cdot}01031\ V^{2{\cdot}825}$. If such differences as these arise there is a great need for co-ordination in this branch as well as in the presentation of the final results.

Our thanks are undoubtedly due to Mr. Telfer for bringing forward this plea for agreement, and I trust that the time will soon come when we shall have an international method of presentation adopted by the various experimenters.

Mr. Perring

MR. W. G. A. PERRING, *Graduate*: In presenting model results there seems to be two classes of people for which they have to be prepared: the practical shipbuilder and engineer to whom perhaps the curve of E.H.P. on a base of speed makes the most useful presentation of the results of any particular model, and the designer and experimenter to whom results in a constant or relative system are best for comparison, for revealing any features of the model resistance and the effect of differences of form upon these.

The Paper has discussed mainly a basis upon which results can be presented in this latter form to be useful internationally. In setting up such a system the ordinate and the abscissa besides being international should present the results in such a way that they can be easily interpreted. The ordinate that lends itself to this best has the same form as ⓪ $\frac{\text{E.H.P.}}{\triangle^{\frac{2}{3}}V^3}$ while for base perhaps $\frac{V}{\sqrt{L}}$ or some modification of this is best for certain classes of work, $\frac{V}{\triangle^{\frac{1}{6}}}$ or some modification may possess advantages in other. Results of model experiments with ordinates $\frac{\text{E.H.P.}}{\triangle^{\frac{2}{3}}V^3}$ on either of these bases have the following advantages:—

(1) The skin friction varying very nearly as the second power of the velocity makes the frictional power vary nearly as V^3, and the skin friction correction is practically a straight line, slightly sloping down, but very slightly so that the model results display directly the features of the wave-making resistance.

(2) $\frac{\text{E.H.P.}}{\triangle^{\frac{2}{3}}V^3}$ also has the advantage of showing for any particular displacement how the resistance varies with the speed—for when the curve is horizontal the resistance is varying as the square of the speed, when down with a power less than the square. The expression of results when it slopes up, with a power greater than the square, and $\frac{\text{E.H.P.}}{\triangle^{\frac{2}{3}}}$ would obscure this advantage.

In Appendix 3 of the Paper is suggested a method of finding "n" the index in

$$R = \rho A v^2 \left(\frac{\nu}{vl}\right)^n$$

It suggests taking two models, one twice the length of the other, running them in an experimental tank or holding them and allowing water to flow past, obtaining the total resistance, and then separating this in to wave-making and frictional on the assumption that "n" is constant.

This disregards the fact that "n" varies with the length especially for low values of $\frac{vl}{\nu}$.

If a curve of Log $\frac{R}{\rho A v^2}$ be plotted on a base of Log $\frac{vl}{\nu}$ this feature

will be readily seen, from such a curve it will be observed "n" is only sensibly constant for values of Log $\frac{vl}{\nu} > 7{\cdot}2$. Mr. Perring

Where v is in $\frac{\text{ft}}{\text{sec.}}$

l is in ft.

ν in $\frac{\text{ft.}^2}{\text{sec.}}$ units.

Taking an ordinary 16-ft. model and the kinematic viscosity of fresh water at 55° F. as $1{\cdot}31 \times 10^{-5}$. This gives a speed of model equal to 13 ft./sec. While for one twice this size and at the corresponding speed the speed would be 18·4 ft./sec. Difficulties attending experiments on a 32-ft. model at this speed would be great and the results obtained since they would correspond to speeds of the order of 1 knot in a 400-ft. boat, would be of little practical value. Model experiments to compare with ship conditions would necessitate extremely large models and high speeds which render them impossible.

Mr. J. LOCKWOOD TAYLOR, *Graduate*: There are two fundamental causes of difficulty in the satisfactory presentation of model-experiment results :— Mr. Lockwood Taylor

1. The use of different systems of units in different countries.
2. The inconsistency of the various units in our own system, and to a less extent, in the metric system (owing to the use of the knot and "metric horse-power.")

Until these difficulties are removed, the presentation of results will not be entirely satisfactory from an international point of view, whatever the system adopted.

The Froude notation, was of course, designed for complete internationality, but unless much wider adoption abroad than has hitherto occurred, is to be anticipated, there seems to be no reason to retain the odd multipliers which so complicate it. The ratios involved in the Froude constants may be retained without multipliers or with merely decimal multipliers for convenience in handling, and the transfer to any Continental non-Froudian system will be no more difficult than at present. This gives the system—

Speed constants $\frac{V}{\triangle^{\frac{1}{6}}}$ $\frac{V}{\sqrt{L}}$.

Resistance or Power Constant $\frac{\text{E.H.P.}}{\triangle^{\frac{2}{3}} V^3}$.

Form Constants $\frac{L}{\triangle^{\frac{1}{3}}}$ $\frac{B}{\triangle^{\frac{1}{3}}}$ $\frac{D}{\triangle^{\frac{1}{3}}}$ $\frac{\text{W.B.}}{\triangle^{\frac{2}{3}}}$.

The chief difference from the system recommended by the Author is in the Power Constant, probably the most important of all. While recognizing the advantage of having a power of the displacement only, in the denominator of each ratio, I do not consider that the Author has proved his case for the substitution of $\frac{\text{E.H.P.}}{\triangle^{\frac{7}{6}}}$ for the expression given above: the form of the Froude power constant is very well established

Mr. Lockwood Taylor

both here and abroad, for reasons that are indicated in the Paper and which it is unnecessary to recapitulate.

The dimensional proofs (Appendix 1 and 2) of the Froude and Rayleigh laws of comparison afford an interesting comparison showing the fundamental nature of the two resistances generally known as "wave-making" and "frictional" respectively. Equation (11) of the Rayleigh law is derived from (1) of the Froude law by substituting viscosity, v, for gravity, g. Hence the terms "gravity" and "viscosity" might be substituted for those generally used in describing the resistances, surface waves and fluid friction being isolated results of the two natural phenomena named.

Equation (8) would be more accurately written

$$R = \rho g l^3 \Sigma \left\{ \left(\frac{v^2}{gl}\right)^{\frac{c}{2}} \right\} \quad \text{since } c \text{ may have any value.}$$

This is equivalent to

$$R = \rho g l^3 \; f_1\left(\frac{v^2}{gl}\right), f_1 \text{ being any function of } \left(\frac{v^2}{gl}\right).$$

Similarly (18) becomes

$$R = \rho l^2 v^2 \; f_2\left(\frac{vl}{\nu}\right).$$

and (19)

$$R = \rho A v^2 \; f_2\left(\frac{vl}{\nu}\right).$$

This explains why, as the Author says, (20) is only an approximate expression of the law, the reason for which statement is otherwise not apparent. It should not be forgotten that (20) and the succeeding expressions for frictional power are not inherently superior to other approximations, and are only to be judged on their merits. The available data show that for moderate and large values of (vl) the approximation is good; but for small values, even above the "critical value," there is not the same confidence in applying the approximate law. A model $\frac{1}{25}$ full size running at the corresponding wave-making speed, $\frac{1}{5}$ that of the ship, has a (vl) value only $\frac{1}{125}$ that of the vessel Thus, even if the frictional resistance of the model is accurately known, in deriving from it that of the vessel, by the use of an approximate law, we are working over a very large range of (vl) values. To this extent, the discovery of the Rayleigh law has shaken our confidence in model experiments.

The Author makes a very bold and interesting suggestion (Appendix 3) when he speaks of obtaining the frictional resistance of a model independently of all experiments on planks, etc., simply by running another model of say twice the size, at corresponding speeds for wave-making. He ingeniously shows how values of the index and coefficient in the frictional resistance formula may thus be obtained.

Thus the index n is to be got from the equation

$$\left(\frac{v}{v_1}\right)^{2-n} = \frac{r\lambda^3 - R}{r_1\lambda^3 - R_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (25c)$$

Mr. Lockwood Taylor

The right-hand side approaches the indeterminate form $\frac{0}{0}$, the difference $(r\lambda^3 - R)$ being small compared with either of the quantities $r\lambda^3$ and R. This being so, I have applied the Theory of Probability to determine the probable error in n corresponding to a probable error of 1 per cent. in each of the quantities r, R, r_1, R_1, and find that for a representative case, this is about 180 per cent. The use of an n value with a probable error of this amount in equation (25) gives the probable error of the frictional resistance of model as 160 per cent. Such large errors are almost beyond the scope of the Theory, but the errors are certainly very great.

A numerical example will make this clear.

If v is about twice v_1, and $\lambda = 2$, reasonable values of the resistances, in unspecified units, substituted in right-hand side of (25c) give

$$\left(\frac{v}{v_1}\right)^{2-n} = \left(\frac{63-59}{17-16}\right) = 4.$$

If a mistake of one unit is made in observing R, we have

$$\left(\frac{v}{v_1}\right)^{2-n^1} = \left(\frac{63-60}{17-16}\right) = 3.$$

Then

$$\left(\frac{2-n^1}{2-n}\right) = \left(\frac{\log 3}{\log 4}\right),$$

and if $(2-n) = 1{\cdot}875$ (the correct value according to the Paper), then

$(2-n_1) = 1{\cdot}487$, $n^1 = 0{\cdot}513$ instead of $0{\cdot}125$

and from (25)

$$f_1 = \frac{63-60}{2^3 - 2^{3-\frac{3n^1}{2}}} = 0{\cdot}94 \text{ the correct value } f \text{ being } 4{\cdot}0.$$

Even if the value of n in (25) were assumed to be 0·125, the error in f would still be 25 per cent. Assuming that no further error arises in obtaining the ship's frictional resistance by the approximate form of the Rayleigh law, the under-estimation of frictional resistance by 25 per cent., and corresponding over-estimation of wave resistance, affects the speed of the vessel for a given engine power and the propulsive coefficient etc., by some 5 per cent. (See the Author's article in the *Marine Engineer*, quoted on p. 235).

The Author is mistaken in saying that the percentage error in deriving f by equation (25b) is commensurate with that in R; the actual error is commensurate, but the percentage error is much greater, since f is considerably less than $\frac{1}{8}$R.

It is recognized, of course, that in applying the method a number of pairs of corresponding speeds would be taken, giving a number of values of n although the possible range of speed is limited: (i) by the necessity of avoiding for the smaller model, the critical speed; (ii) the impossibility of obtaining reliable results at high speeds in a short tank; further, (iii) at high-speed length ratios, wave resistance is so high that frictional resistance phenomena are apt to be masked. Even if we take

Mr. Lockwood Taylor

as large a speed range as the above considerations allow, where the probable error of any individual value of n is as large as has been indicated, the probable error of the mean value is also considerable. Theory shows that if there are ten observations (*i.e.*, pairs of speeds, each double the other) the probable error of the mean is one-third that of a single observation, or in the case quoted, 60 per cent. above or below the true value.

The only way to make the method more accurate is to increase the ratio λ, for which the Author suggests a value of 2. The probable error in using the right-hand side of (25*c*) varies inversely as λ so that to reduce the error in estimating n to within reasonable limits, what will be practically full-sized towing experiments must be carried out.

In (25*b*) however, the error decreases much more rapidly with increase of λ; thus for $\lambda = 3$, it is one-fifth the value when $\lambda = 2$. It is with the help of this equation (*i.e.*, by using an assumed value of n, rather than by using (25*c*) to find n) that model experiments can be made to throw light on the question of fluid friction.

Thus, while full-sized experiments are necessary to determine the best value of the index n, as well as to make us independent of the approximate form of the law, the running of large models over a large range of speeds will help to extend our knowledge of frictional resistance.

AUTHOR'S REPLY.

The Author

Reply to Professor Welch.—In the first place I should like to thank Professor Welch for the exceedingly kind manner in which he introduced me to the meeting. Any satisfaction that I have personally derived from the wide interest taken in my paper will, I know, be shared by Professor Welch because of this interest having been occasioned by one of his former students.

I am pleased to have Professor Welch's opinion that the method that I propose is distinctly simpler than the Froude system. However, I am sorry that he stresses so much (amongst Britishers at any rate) the fact that my basic expressions are in terms of metric units, because for all practical purposes, the units of speed, weight and power are identical in both the British and the metric systems. The only unit that is uniquely metric, which is the metre used for length, brings the relative length expression similarly almost exactly equal to the Froude (M). The adoption of the proposed system is thus hardly a revolutionary sacrifice of British units.

With regard to the proofs of the Froude and Rayleigh laws, the amplification suggested by Professor Welch is now given as a footnote to the respective Appendices.

My statement that the Froude (K) really refers to the velocity of a one-cubic-foot model probably requires further explanation. In the Relative system I adopted the one-ton model as the basis; and all

resulting expressions such as relative power, speed and length were the actual powers, speed and length of the one-ton model. This basis admits of the simple visualization of the significance of the expressions used. Froude adopted a much more abstract basis for his fundamental expressions and Professor Welch mentions Froude's derivation of (K) However, as Froude's (M) is really the length of a one-cubic-foot model, in discussing (K) I considered it analogously to Relative system basis and stated that (K) referred to the velocity of a one-cubic-foot model. To be strictly correct (K) is 1·055 times the velocity of this basis model and I think it is this discrepancy which Professor Welch has in mind when he draws attention to my statement. The Author

Reply to Mr. Williams.—One of the most important attributes which I had persuaded myself that the "Relative" system possessed was that, whilst rigourously in terms of metric units, actually it could not be distinguished from British units in common use to-day. While I do not think that Mr. Williams has overlooked this coincidence, yet I think that he somewhat underestimates its patriotic importance.

Mr. Williams shows a preference towards some form of $\frac{V}{\sqrt{L}}$ as the relative-speed term. This preference he substantiates on the ground that the length of the ship is the most fundamental quantity in a design and also that the expression $\frac{V}{\sqrt{L}}$ is much easier to calculate than the expression $\frac{V}{\Delta^{\frac{1}{6}}}$. Mr. Hinchliffe in his remarks ably emphasized the diversity of interpretations placed upon length by designers and experimenters alike. Further, I think Mr. Williams will agree that the displacement of a ship is a much more fundamental quantity in a design than the length. For these two practical reasons I think that length is best left out of the relative-velocity expressions. This particular point I have dealt with more fully in my reply to Prof. T. B. Abell. Mr. Williams wisely points out the pseudo-dimensionless nature of the expressions $\frac{V}{\Delta^{\frac{1}{6}}}$ and $\frac{V}{\sqrt{L}}$. However, I do not think that the introduction of g into the latter expression, although it does make the expression truly dimensionless, is to be desired from the practical standpoint.

I am glad that Mr. Williams sees but little objection to the use of $\frac{P_w}{\Delta^{\frac{7}{6}}}$ etc. for the representation of power. Similarly I am pleased to find he considers that the Froude method of depicting a model's geometrical properties can hardly be improved upon because the Froude and the Relative systems are for all practical purposes identical in this respect.

Reply to Mr. Tutin.—I am pleased to have so favourable an opinion

The Author

of the Relative system from Mr. Tutin. Mr. Tutin points out an apparent lack of generality in my proofs of the Froude and Rayleigh laws. The more general method of expressing the resistance proposed by Mr. Tutin is undoubtedly initially more rigorous than the method I adopted. However, my method is quite rigorous, for whatever complex form the constituent expressions of speed, length, etc., may have, the dimensional relation must be satisfied. The exponent c which is involved in my proof may be quite a complex quantity and it is only because its complexity is not sufficiently self-evident that the alternative frictional presentation mentioned by Mr. J. Lockwood Taylor and hinted at by Mr. Tutin is sometimes preferred.

Mr. Tutin's deduction from Appendix 3 of the Paper is very interesting, and incidentally, from the point of view of the "weight equation," very useful. I gather that Mr. Tutin suggests that n can be determined from this relation. This is possibly correct but M and N must be known in order to do so. By the experimental method which I propose the determination of n is the most fundamental process. Once n is determined M and N can be simply obtained by means of the relation suggested by Mr. Tutin. Taking the Gebers n value of 0·125 the equation for frictional power pointed out by Mr. Tutin becomes—

$$P_f = K \,.\, \triangle^{\cdot 625}\, V^{2\cdot 875}.$$

Reply to Mr. Hinchliffe.—Mr. Hinchliffe brings out very clearly and forcibly what may be called the tyranny of being used to a thing. In the absence of Mr. Hinchliffe's actual support and adoption of such a system as I recommend, I appreciate the good wishes for its ultimate success that he extends to me.

I am pleased that Mr. Hinchliffe has pointed out the necessity for some agreement in the definition of a ship's length. This necessity is very important when the speed variable $\frac{V}{\sqrt{L}}$ is employed in an attempt to compare approximately similar forms, but I think that this importance is somewhat diminished when the relative velocity expression $\frac{V}{\triangle^{\frac{1}{6}}}$ herein recommended is adopted. Personally, however, I think length on load line has much to commend it. Uniformity in this respect is greatly complicated through the use of differing definitions of length in other branches of shipbuilding, *e.g.*, those used in connexion with the Bulkhead convention, tonnage, freeboard and the classification societies' regulations, and above all a particular firm's practice in the calculation of displacement. When using the data of a particular model, care must be taken in developing the design that the relative length specified by the experimenter is understood. If a profile and lines plan are provided with the data, the laxity of length definition becomes unimportant.

Mr. Hinchliffe's use of a graph index for the classification of model data is very interesting. The problem of including a third dimension, which he sets me, is also interesting. This third dimension I would

prefer to be the relative draught rather than the relative beam. A vessel most often has a stipulated draught and deadweight (and hence displacement) imposed upon it in the initial stages of a design. From these stipulations the relative draught is immediately determined and thus is more often likely to be of use in tracing suitable models than the corresponding relative beam. So far as I understand Mr. Hinchliffe's index I think that there is no need for a three-dimensional chart. Above the base line of prismatic coefficient the relative lengths are placed, each spot being appropriately numbered and below it the corresponding relative draughts with the same identifying numbers. The identifying number eliminates the necessity for spatial co-ordinates. The Author

Mr. Hinchliffe suggests that I extend the scope of this Paper to include propeller and propulsion data. Several other contributors to the discussion have expressed the same wish. So far as the problem is concerned with experiment data it is probable that the subject is in a sufficiently advanced state. However, both marine and aeronautical engineers must be consulted in such a task, the magnitude of which would undoubtedly require the services of a committee rather than the somewhat promiscuous attention of an individual.

Reply to Mr. Whiting.—Mr. Whiting regrets, chiefly on patriotic grounds I take it, that I have recommended the adoption of the metric system. In pure science, automobile engineering and electrical engineering the adoption of the metric system in this country is firmly established. But be this as it may I think the system I propose, composed as it is of the units V, $\triangle$ and P which are for all practical purposes identical in both the British and metric systems, and the expression $\frac{L}{\triangle^{\frac{1}{3}}}$ where L is in metres and which is for all practical purposes the Froude (M), is as much a British system as it is a metric. To be quite precise I had to define my expressions either one way or the other and in view of probable international agreement, however remote, I chose the metric units.

Mr. Whiting points out that actually the expressions that are usually adopted are not in their final practical form dimensionless. This, of course, is quite correct and probably they would be better known as pseudo-dimensionless expressions.

I agree with Mr. Whiting that research workers should be untrammelled in their pursuit of truth by having no particular mode of presenting their final results imposed upon them. However, I cannot agree that from the practitioner's standpoint resistance cannot be dispensed with in favour of power and that a displacement-speed basis is not as valid as a length-speed basis. Model data is used for the powering of ships and incidentally a particular model's data must only be used for powering an identical ship.

If by "juggling with" Mr. Whiting means departing from the

The Author

actual model data to an approximately similar vessel, then I maintain that no speed basis, whether displacement or length, can supply information which does not exist.

Reply to Mr. Liddell.—Mr. Liddell has raised several interesting points. The first of these with regard to the use of (L), $\frac{V}{\sqrt{L}}$, and (P) I have already dealt with in my reply to Prof. T. B. Abell and Dr. Kempf. The plotting of total in preference to constituent relative powers I have dealt with in my reply to Mr. Baker.

The changing ratio, with absolute size, of relative wave-making to relative frictional power mentioned by Mr. Liddell can hardly be expected to be brought out in "Relative" co-ordinates, for the primary aim of the presentation is to eliminate absolute size. The alignment chart accompanying the Paper will, as Mr. Liddell agrees, bring out this changing relation.

Mr. Liddell's suggestion to make use of the ratio $\frac{P_f(\text{equivalent plank})}{P_e}$ is novel, but I doubt whether the expression can really be said to measure the "form efficiency" or "quality" of the model. To express efficiency, P_f should be the absolute minimum for a given displacement and length. However, it is well known that P_f varies with say the beam-draft ratio as does of course P_w. If two models of different beam-draft ratio are compared by Mr. Liddell's suggested method, the one which is more inefficient from the frictional power standpoint, *i.e.*, that with the greater wetted surface, will actually have a higher quality than the more efficient. As the purpose of any criterion such as the one proposed is to be of service over a whole range of model type, I do not think that Mr. Liddell's suggestion possesses the generality and freedom from qualification that would be necessary to justify its adoption in practice.

Reply to Mr. Fletcher.—Mr. Fletcher draws attention to the difference which exists between the Froude, Baker and Gebers calculated values of frictional resistance. In an article published in the *Marine Engineer and Naval Architect* in October last year, I reproduced a diagram previously published by Dr. Gebers, which brought out very clearly the difference between Gebers' and Froude's work. In view of the improved apparatus that Dr. Gebers has at his disposal at the Vienna Tank, and the fact that the experiment data which he has published are the result of repeatedly checked and not isolated experiments, and further that as Dr. Gebers took every precaution to eliminate form effect, then there is good reason for accepting the accuracy of the Gebers work in preference to that of Froude. However, the discrepancy between the results of these experimenters, although large, is not revolutionary. In the article already referred to, I showed the error in the estimate of the speed of a full-size ship occasioned by a given error in the splitting up of the constituent resistances of its similar model. A low estimate of a model's

frictional resistance produces overpowering in the ship because of the consequent high estimate of the model's residuary resistance, and *vice versa*. An appreciable error can occur in the assessment of the model's frictional resistance without the error being observable say, on a trial trip. The Froude coefficients, although they possess a small form effect, are undoubtedly too high for lengths greater than 50 feet. The form effect they do possess is not sufficient for a ship-shaped body and thus their use has probably produced too high an estimate of the model's residuary resistance and thus, together with the high estimate of the ship's frictional resistance, has probably produced too high an estimate of the ship's total resistance. This high estimate of resistance has actually been used in some otherwise responsible quarters as an argument against the validity of the Froude law! The Author

The confusion that exists at present as to the magnitude of frictional resistance should be largely eliminated by obtaining the form factor based on Gebers' plank minimum, by the method I suggest in Appendix 3. This done, the relative frictional power curve is plotted, and from this curve, the frictional power of any similar ship can be obtained without reference to wetted surface, tabulated data, or form effect. The calculation of frictional power becomes thus as precise as that of wave-making power, having a simplicity that should appeal to designers and encourage their more frequent and less sceptical use of model-experiment data.

Reply to Mr. R. E. Froude.—I greatly appreciate Mr. Froude's remarks on my Paper. I regret that through a possible ambiguity of statement I have conveyed the impression to Mr. Froude that I considered that (C) was specifically designed chiefly to procure the simple fairing of experiment-data curves. On page 224 I gave as the chief reason for the adoption of (C) the flattening of the power curve which it effects. The immediate and obvious corollary to this flattening, *i.e.*, the reduction of the power range, I did not, unfortunately, state specifically, but brought it out more or less in the nature of antithesis, when I stated that "there seems to be no important justification in practical work for not making the higher powers that are required at the higher speeds immediately visible to the eye of the designer." The ease in fairing which I suggested as a possible attribute of (C) I did not consider so obvious, and thus specifically drew attention to it.

However, Mr. Froude's own clear exposition of the reasons which led to his adoption of (C) will adequately dispose of any misapprehension which my own explanation may have occasioned.

Mr. Froude is undeniably correct when he asserts that (C) is now quite indispensable at Haslar. Similar assertions could, however, be made for the indispensability of $\frac{R}{\Delta}$ at the Washington and Grenelle tanks merely from the point of view of consistency of presentation. All

The Author

individual systems used by various experimenters must generally of necessity be retained; but I am certain Mr. Froude will agree with me that when a system can easily be devised which is immediately and not revolutionarily international, and in which, in addition to their own, experimenters might be persuaded to present their results for the immediate convenience of the practitioner, and ultimately for their own interchange of ideas, then there is something to be said for its adoption.

Reply to General Rota.—I greatly appreciate General Rota's kind remarks on my paper and his references to my other published writings. The basic conception of the Relative system is by no means new to General Rota, and I am pleased to find that the further development that I suggest, following the earlier work of Tideman and Scribanti, is also acceptable to General Rota. The Froude tank at Spezia is one of the strongholds of the (C) − (K) notation, but because of General Rota's early work in connexion with the practical assimilation of tank data and his advocating of the "Quadro Calcolatore" device in Italian designing practice, his ready acceptance of my proposals of relative wave-making and frictional power is easily understood. General Rota's criticism of the relative frictional power term, in so far as this latter involves the continuance of the equivalent-plank assumption, is directed at the very foundations of our subject. Granting the validity of the assumption—which years of experience would seem to justify—and only considering the comparative work of Gebers and Froude, I think that General Rota will agree that the Gebers results are, in the present state of knowledge, to be preferred. The further work of Dr. Gebers, upon which he is at present engaged and which is based upon the method I propose, should definitely decide whether the equivalent-plank assumption can be dispensed with.

General Rota stresses the importance of towing experiments on full-size ships. Were these experiments carried out our scientific knowledge of the subject would undoubtedly be greatly increased.

General Rota's references to his "Quadro Calcolatore" are very interesting. This device, whilst evidently in common use in Italy is not—to the best of my knowledge—greatly used in this country. Mr. Hök, in discussing Mr. Denholm Young's 1893 paper, referred to by General Rota, pointed out one defect of this device—that of the distortion of the power curve. With the Relative system this distortion is entirely absent. The "Quadro Calcolatore" is essentially a graphical means of computation; and if the subject of graphical computation is reviewed, it will be found to have passed through three distinct stages, these being the Cartesian, the Logarithmic and the Nomographic. At the time of Mr. Denholm Young's paper the logarithmic was in the ascendancy, but since that time Professor d'Ocagne has developed the subject of nomography, and once its possibilities are realized there

is no doubt that it will play a very important part in the applied naval architecture of the future. I therefore look upon the alignment chart which I propose as the logical development of the "Quadro Calcolatore," progressing as it does from logarithmic to nomographic methods of calculation. The superiority of the nomogram is recognized in several branches of pure and applied sciences, and I feel sure that General Rota will also eventually decide in its favour for use in the practical manipulation of model-experiment data. The Author

Reply to Admiral Taylor.—Admiral Taylor in his opening remarks happily introduces himself as one of the earliest offenders to depart from the classical Froude system. As a matter of history, I have discovered, since writing my Paper, that Admiral Taylor, in departing from the Froude system, actually reverted to what is probably a still more classical system. I have recently obtained a copy of Tideman's Report of his early experiment work, and actually in 1876,* 12 years prior to Froude's publication of his "Constant" system, Tideman proposed and adopted a system that has the one-ton model as its basis, in which the residuary resistance is expressed in kilograms per ton, speed as the speed in metres of the one-ton model (*i.e.* $0{\cdot}5144 \frac{V}{\Delta^{\frac{1}{6}}}$ where V is in knots), and in which the calculation of frictional resistance is calculated in precisely the same manner as in Admiral Taylor's method. To characterize the dimensions of the model the actual dimensions (in metres) of the one-ton model are used. Thus, with the exception of the speed unit, the displacement-length ratio and the metric units used by Tideman, the Tideman and Taylor systems are identical in principle.

I am glad to have so eminent an authority as Admiral Taylor to endorse my opinion that the presentation of model experiment data should be moulded for the convenience of the practitioner rather than the research worker. Admiral Taylor's remarks as to the policy of the Washington Tank, based on this principle, will add greatly to the value of our *Transactions.*

Admiral Taylor considers that $\frac{V}{\sqrt{L}}$ is probably to be preferred to $\frac{V}{\Delta^{\frac{1}{6}}}$. When the use of model data is restricted to the powering of a precisely similar ship the use of $\frac{V}{\Delta^{\frac{1}{6}}}$ can be more convenient than of $\frac{V}{\sqrt{L}}$. Admiral Taylor's well-known "Standard Series" resistance contours can be simply converted to the system I propose without any loss of accuracy, no more auxiliary diagrams for tracing the effect of length being necessary in this system than in Admiral Taylor's system.

Admiral Taylor's preliminary reference to my proposed disregarding of resistance, I take it, especially refers to the standpoint of the

* See Bibliography.

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experimenter. From that of the practitioner, power is the thing which is, and has to be measured, and I am pleased to have Admiral Taylor's qualified opinion that the expression $\frac{\text{E.H.P.}}{\triangle^{\frac{7}{6}}}$ "is almost the ideal one for the purpose."

I am sorry that Admiral Taylor considers that it is probably simpler to continue calculating frictional resistance or power as at present rather than to adopt the analogous method to the Froude law which we have now at our disposal in the approximate form of the Rayleigh law. Admiral Taylor's point of view in this connexion, he states, is that of simplicity for the practitioner. In the method I propose in which relative wave-making power is placed above the base line of relative speed, and relative frictional power below it, the practitioner has always in front of him the respective magnitudes of the two main components of power. To convert these relative powers into actual powers the processes are, by means of the alignment chart that I propose, mechanically identical and unquestionably simpler than existing methods.

Admiral Taylor's suggestion that the relative-power expressions $\frac{P_w}{\triangle^{\frac{7}{6}}}$, $\frac{P_f}{\triangle^{\frac{53}{48}}}$ be multiplied by some convenient power of ten in order to secure ease of handling, is a very important one. I am not quite sure, however, that ease of practical manipulation would be secured by this step. However, ease of discussion might be secured. The expressions as they stand at present represent the actual respective horse-powers of the one-ton model; and any alteration to the expressions rather detracts from the value of the consistent use of the one-ton model basis. The use of the alignment chart which I recommend will largely eliminate the practical necessity for a more convenient magnitude for the relative-power expressions; and in the light of these considerations I should prefer to retain the expressions proposed.

Admiral Taylor, together with several others who have contributed to this discussion, emphasizes the necessity for the unification of propeller-data presentations. There can be no doubt of the urgency of such a step, although I am afraid it could hardly be attempted as an immediate extension of the present Paper.

Reply to Mr. Baker.—From such an acknowledged champion of the (C) and (P) system as Mr. Baker I had expected a vigorous attack upon that which I have had the temerity to propose. In the Paper I did not deal in detail with the shortcomings of each particular system. Immediate internationality and simplicity were my objects and my standpoint was that of the practitioner more than that of the research worker.

In reply to Mr. Baker I should like to consider the utility and

simplicity of (C) a little further. In the first place I should like to quote from his reply to the discussion on his 1914 I.N.A. Paper (*Transactions*, Institution of Naval Architects, vol. 56, p. 64). The Author

"Passing to Professor Welch's remarks, I deliberately left out the correction curves for lengths over 400 feet, because it would make the diagrams so complicated that, unless you were fairly conversant with the "constant" method of presenting results, a good many people would get tired of trying to understand the Paper and give it up altogether, and I think it is better to understand a little, than to try and understand a lot and not understand anything. I will try and see whether I can give some sort of average figures which could be used, but I do not propose to complicate the diagrams by putting in other skin friction curves. I will give some sort of average figure which you must regard simply as there or thereabouts, and not in any way very accurate."

Parallel with this quotation I should like to quote from Mr. Baker's remarks on my Paper.

"We had in mind this also, that every man *trained* to naval architecture understands these constants, and if he does not it is better for him not to meddle with model data at all."

Mr. Baker admits the difficulty in getting used to the correct manipulation of the (C) system. So much so that he chooses somewhat to neglect the accuracy of the results rather than that the attention of anyone wishing to abstract information therefrom should be alienated. Mr. Baker in the second quotation adopts the rather unsympathetic attitude that because a trained naval architect can eventually understand the "constant" system, those who cannot should cease to meddle with model data at all. In this connexion I think that it will be admitted that an expertness and facility in the manipulation of a method, brought about by practice, are no proof of the method's inherent simplicity. The true test of a method's simplicity and utility is found in the way in which a man makes his first approach to the method, and not by the expertness which he possesses when he has got used to it. A thorough training in the technique of naval architecture, even to-day, is not necessarily within the grasp of everyone who will eventually take their part in ship design and construction; and thus it is the duty of those who have had this training to simplify their methods and make it possible for the humbler, first to understand and then to utilize.

One advantage claimed by Mr. Baker and others for (C) is that the total effective horse-power can be immediately obtained, only a length correction being necessary to correct for size. From the first of the above quotations it may be gathered that this length correction is not such a simple matter as hitherto supposed. In fact when this correction is actually made the obtaining of the total power is precisely analogous to that involved in the Taylor method. R. E. Froude in his 1888 I.N.A. Paper showed how to effect this correction, and in addition showed that when only one model's result was being portrayed the correct total effective power could be simply obtained for different absolute lengths by means of an appropriately modified base line. In Froude's 1904 Paper the same artifice was employed in a diagram containing the results of a whole series of models. In this particular case the artifice must be

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recognized as approximate, for the (C) correction depends upon the value of (S) as well as on the actual length of the model or ship. The above-mentioned quotation from Mr. Baker's 1914 I.N.A. Paper and the references to Froude's 1904 I.N.A. Paper show that a slight approximation has crept into the public presentation of work of those experimenters who favour the use of (C). The correction chart given by Mr. Baker in his 1914 Paper only allowed for length by subtracting or adding to the (C) value at the standard length of 400 feet. This subtraction, etc., is erroneous in principle because the necessary modification involves a knowledge of the frictional component of (C) at the standard length, and any correction factor must be of the form of a multiple or fraction of the frictional component of (C) at the standard length. All this, of course, is well known to experimenters and in Mr. Kent's 1915 I.N.A. Paper, Plate 15, Fig. 9, a convenient chart is given which for a given length and $\frac{V}{\sqrt{L}}$ value, enables the factor to be obtained which, multiplied by (S), produces the frictional component of (C).

Thus, as previously mentioned, to use the (C) system correctly the wave-making and frictional components will always need to be distinguished by the practitioner whether the experimenter is kind enough to do so or not. Unfortunately none of the experimenters who use (C) with the exception of R. E. Froude and General Rota, have published sufficient information to enable their work to be utilized correctly. For instance, none of the published work of Baker, Kent, Semple or Robertson gives the values of (S) of the models experimented on by them. It is thus necessary to use approximate formulæ in order to determine the value of (S) with their models—a fact, together with the knowledge that the preponderating resistance in merchant vessels is frictional, detracts considerably from the immediate value of the published work of those experimenters.

As it is desirable, from the point of view of the experimenter and the practitioner, to know the respective wave-making and frictional components of power, there seems to be no good reason why the present practice of (C) with its consequent awkwardness of manipulation should be maintained. From this standpoint it will be interesting to examine how the use of (C) lends itself to similar treatment as in the proposed Relative system.

Let the total effective power of the model be split up into its two components thus :—

$$P = P_f + P_w$$

From Appendix 3 the frictional power based on Gebers' experiments, and allowing for a form factor K may be written in the usual British units.

$$P_f = \frac{\cdot 01352K}{326} \frac{S.V^{2\cdot 875}}{L^{\cdot 125}}$$

Substituting this expression in the above equation and multiplying through- The Author
out by $\frac{427 \cdot 1}{\Delta^{\frac{2}{3}} V^3}$ we have

$$\frac{427 \cdot 1\ P_e}{\Delta^{\frac{2}{3}} V^3} = K.\ \frac{427 \cdot 1}{326 L^{\cdot 125}} \times \cdot 01352 \cdot \frac{S}{\Delta^{\frac{2}{3}}} \times V^{-\cdot 125} + \frac{427 \cdot 1\ P_w}{\Delta^{\frac{2}{3}} V^3}$$

but $(S) = \cdot 09346 \frac{S}{\Delta^{\frac{2}{3}}}$ and calling the frictional and wave-making components of (C), $(C)_f$ and $(C)_w$ we have

$$(C) = \frac{\cdot 1892 K}{L^{\cdot 125}} (S) V^{-\cdot 125} + (C)_w$$

or expressing $(C)_f$ in terms of $\frac{V}{\sqrt{L}}$,

$$(C)_f = \frac{\cdot 1892 K}{L^{\frac{3}{16}}} (S) \left(\frac{V}{\sqrt{L}}\right)^{-\frac{1}{8}}$$

The term K is a form factor which represents the ratio of the model's frictional power to that absorbed by the equivalent plank. Following Mr. Baker's usual practice of adopting a standard length of 400 feet, the value of $(C)_f$ could be evaluated for each model, the resulting curve on a base of $\sqrt{L}$ being plotted below the base and the value of $(C)_w$ plotted above the base. A correction chart for length based on the expression $\left(\frac{400}{L}\right)^{\frac{3}{16}}$ will then enable the value of (C) to be most simply and accurately obtained.

I have deliberately dealt at some length with the above question of the (C) presentation merely to show that even in itself it offers considerable scope for simplification and improvement.

Mr. Baker's remarks as to the advantage of the (C) system apply with equal force to all existing systems of presentation.

Mr. Baker states that Robinson of the Washington Tank has adopted (C) for the presentation of that Tank's model data. Judging by the date which Mr. Baker gives in this connexion, 1920 (*i.e.*, 1920 *Trans. Am. Soc. M.E.* and *N.A.*?) I think he is really thinking of Mr. A. J. C. Robertson, sometime connected with the Michigan University Tank and who is definitely advocating (C) in America. I merely mention this possible oversight on Mr. Baker's part for had Mr. Baker's statement been true the conversion of the stronghold of "pounds per ton" would indeed have been a triumph for the advocates of (C). Admiral Taylor's contribution to this present discussion strengthens my belief in Mr. Baker's oversight. Mr. Baker points out that there is a vast amount of data expressed in the form of (C). If international agreement is arrived at with regard to the skin-resistance assessment then there can be no doubt that much of this vast amount of data will have to be overhauled. Apart from this question of skin friction the invalidity of slow-

The Author

speed work on small models is not too often recognized by some experimenters, and a considerable amount of past data will in the future have to be discarded. The milk bacteria analogy suggested by Mr. Baker has a further extension in this connexion. Ship data, like milk, should have an impartial "Ministry of Health" to testify as to its purity and its being fit for professional consumption.

Mr. Baker in discussing the Relative system suggests that it has two units of length. The velocity term which he says contains feet, equally well contains metres; in fact the derivation of the knot and the metre are closely allied. The knot used in the Relative system I should have qualified as that used in metric countries and which equals 1,852 metres per hour as against the British knot of 1,853 metres per hour.

Mr. Baker's remarks with regard to my proposed use of the metric ton are rather strange. I was under the impression that the term metric ton was immediately recognizable as, and the generally accepted terminology for, a ton of 1,000 kilograms. Apart from technical work where its use is common, it is also quite well known to practical shipbuilders who have engaged in ship construction for Continental owners.

The inherent complication of the frictional-resistance problem makes it impossible to avoid all awkward arithmetic. However, the use of an alignment chart for the solution of $P_{rw} \triangle^{\frac{7}{6}}$ and $P_{rf} \triangle^{\frac{53}{48}}$ will probably reduce the process of evaluation to a minimum. Even when the suggested modifications to the (C) system have been effected the evaluation of power from (C) will be more complicated than in the "Relative" system.

Mr. Baker's reference to the *Daily Mail* hat is illuminating. Mr. Baker considers that model-power data cannot successfully be expressed in one form owing to the necessity of bringing out other features, and, I would add, respecting various requirements. Mr. Baker uses the hat analogy to dispose of $\frac{P_w}{\triangle^{\frac{7}{6}}}$. But surely the same argument also disposes of his consistent use of (C)! Mr. Baker admits that the replacing of V by $\triangle^{\frac{1}{6}}$ is dimensionally correct but suggests that any resulting expression is liable to be useless and may be used inaccurately. When model data is used for the powering of absolutely similar forms there can be no possible doubt as to the accuracy and utility of any basic expression that is dimensionally correct. None of the expressions that I have proposed, with the exception of P_{rf}, is altogether new; they have appeared in other systems.

With regard to the proof of the Froude law given in Appendix I. of the Paper this is quite different from that given in Mr. Baker's book. Although both are based upon the comparison of physical dimensions I consider that the method which I give and which I believe is also due to the late Lord Rayleigh is more general in its "attack" than that published by Mr. Baker. Mr. Baker is correct in stating that Lord Rayleigh is deducing the law of dynamical similarity prefaced his deduction by "*If* the frictional resistance," etc. . . .

The English authors mentioned by Mr. Baker were not included in my bibliography because I had not directly drawn upon their work. The Author

The method which I gave in Appendix 3 and which I believed to be original, has I find been used by Dr. Gebers as long ago as 1908. If the two models are in different physical localities of a VL range the method must fail for n is admittedly different for laminar and turbulent flow. If, however, a minimum value of $VL = 5m^2/\text{sec.}$ were used, both models would be in the turbulent range and thus n should be sensibly constant. In this connexion I would refer Mr. Baker to Dr. Gebers' contribution to the present discussion.

In criticizing Dr. Gebers' n value of 0·125 Mr. Baker is probably under the impression that I am referring to Dr. Gebers' Dresden experiments carried out in 1907-8. The value I quote was that given by Dr. Gebers in 1921 and was deduced by him from the results of undoubtedly the most thorough, reliable and comprehensive experiments that have yet been carried out on the resistance of thin planks. The experiments which were carried out during and after the war by Dr. Gebers in the new Vienna Tank are exemplary in their presentation. Reference to Dr. Gebers' Vienna experiments is made in my Bibliography.

Finally I am sorry that Mr. Baker has not made any mention of the undoubtedly different points of view of the experimenter and practitioner. There can be no doubt as to the existence of this difference of outlook, and on Mr. Baker's *Daily Mail* hat theory, this difference must be catered for.

Reply to Dr. Gebers.—Dr. Gebers congratulates me as an Englishman for partially adopting the metric system, and asks me to go still further. Dr. Gebers will see from the discussion on my paper that British naval architects are not of one accord as to the desirability of the adoption of the metric system. My task was to usher in a masked metric system in order to secure a peaceful and evolutionary rather than revolutionary transition stage. I had to show that each expression that I proposed was really British. For this reason I chose the knot as the unit of velocity rather than the m/sec., and although I openly used the metre in expressing the length of the one-ton model I covered myself by pointing out that the resulting relative length expression was almost precisely numerically identical with that of Froude's (M).

Dr. Gebers presents a very strong case for the adoption of metres per second as the unit of velocity, and personally I would endorse its adoption. Of course, British naval architects will think in knots for a long time to come, but as they have been thinking in terms of $\frac{V}{\sqrt{L}}$, (L), (K) and (P), there should be no difficulty in persuading them to think in terms of the quantity $0{\cdot}5144\frac{V}{\triangle^{\frac{1}{6}}}$ where V is in knots, but which actually represents the velocity in metres per second of the one-ton model and actually was the only relative velocity expression used by

The Author

Tideman. Incidentally, it is much nearer the Froude (K) *i.e.* $0{\cdot}5834 \frac{V}{\Delta^{\frac{1}{6}}}$ than the expression I proposed, and thus Britishers who think in terms of (K) would soon adapt themselves to $0{\cdot}5144 \frac{V}{\Delta^{\frac{1}{6}}}$, and have the added satisfaction that they were actually thinking of the one-ton model's velocity in metres per second. However, if the form $\frac{V}{\Delta^{\frac{1}{6}}}$ is agreed upon the best solution will be to use a double scale of knots and metres per second. Such an artifice should secure the immediate satisfaction of British and Continental workers and will assist in the ultimate adoption of the metres per second basis.

Dr. Gebers points out that the one-ton model would be more accurately defined as the m^3-model. This I pointed out in my paper on page 222, but unfortunately the qualification of weight displacement as tons fresh water was inadvertently omitted from the printed proof.

Dr. Gebers considers that Resistance is more important than horse-power from—I take it—the standpoint of the practitioner. Dr. Gebers mentions in support of this statement the cases of propeller design and the towing efficiency of tugs. However, propellers can be equally well designed to develop a given power as to develop a given thrust; in fact, the former base is the more usual, at least in this country. The towing efficiency of tugs instead of being expressed as kilograms per indicated or brake horse-power—an expression which, although in common use in Germany, is not dimensionless—can be better expressed as the ratio of the indicated power when towing at a certain speed to that developed when moving alone at the same speed. This expression is dimensionless and holds good for similar tugs at the corresponding speed. Another expression used in Germany, and which expresses towing efficiency as the ratio of the effective power of the towed barge to the brake power of the tug when towing the barge, whilst certainly dimensionless, does not seem so direct as the one proposed.

Dr. Gebers' criticism of the British, French, German and Italian practice of characterizing the power required to tow the naked hull as the effective horse-power, is very interesting. However, I do not think that the possible ambiguity to which he alludes is very real—at least amongst English speaking people. The significance of effective horse-power has been with us since the beginning of the subject, and I think the effective horse-power of the ship will always be realized as being quite different from that of the machinery. Of course we have in English the expression tow-rope horse-power which corresponds to that proposed by Dr. Gebers, but it is but very rarely used. However, I think, the present terminology e.h.p. or P_e can be safely retained as Dr. Gebers' P_t would probably be confused with thrust horse-power in propeller design.

I am pleased to have Dr. Gebers' statement that the method I proposed in Appendix III. has actually been used by him. I have

not seen Dr. Gebers' 1908 paper, although I have read his Werft u. Reederei article. In this latter article the frictional resistance at corresponding speeds was certainly shown to be proportional to $\lambda^{2 \cdot 8125}$, but the isolation of frictional and wave-making resistance by the solution of a simultaneous equation—which is the chief point of my method—was not mentioned in this article. The Author

I trust that Dr. Gebers will soon publish his further work on this subject. This work is absolutely essential before international agreement on the frictional-resistance correction of model data can be obtained.

Reply to Dr. Kempf.—I am glad that Dr. Kempf intimates the willingness of the Hamburg Tank to co-operate with other tanks in the publication of the work in some common system. As the Hamburg Tank is easily the largest and most modern in the world, such a proposal should carry considerable weight. Dr. Kempf, however, prefers the expression $\frac{V}{\sqrt{L}}$ for relative velocity rather than the expression I propose, *i.e.*, $\frac{V}{\triangle^{\frac{1}{6}}}$. Dr. Kempf refers to the *definite* relation between the length of produced wave and length of ship, and puts this forward as the most preferable interpretation of relative velocity. The length of the produced wave can hardly be said to bear a *definite* relation to the length of the ship: rather, I should say, it bears an indefinite relation. This relation is of course the same for similar ships at corresponding speeds, yet for ships of the same length and speed but of different fullness, the relation between length of wave and length of ship is quite different. Thus the "wave" physical significance of relative velocity is something which demands considerable power of visualization, that is, of course, provided that this physical significance is to be recalled on each occasion the expression is used. I think Dr. Kempf wishes this to be so and that I agree with him naturally follows from requirement 3 of the Paper. But I qualify in this connexion physical significance as an *obvious* physical significance and I would suggest to Dr. Kempf that $\frac{V}{\triangle^{\frac{1}{6}}}$ being the actual speed of a one-ton model has a much more obvious significance than the involved relation of length of produced wave and length of ship. In any case R. E. Froude deduced his (K) *i.e.*, $\cdot 5834 \frac{V}{\triangle^{\frac{1}{6}}}$ from precisely the same argument as that used by Dr. Kempf and thus the expression $\frac{V}{\triangle^{\frac{1}{6}}}$, apart from its obvious significance, also has the same further significance as that of $\frac{V}{\sqrt{L}}$. From the designer's standpoint, as the proposed relative velocity connects the two most fundamental quantities—speed and displacement—I think its adoption, as Dr. Kempf admits, for the economic comparison of similar ships, is to be desired in a system especially moulded for the practitioner.

The Author

I am glad that Dr. Kempf more or less endorses the provisional use of $\Delta^{\frac{5}{4}\frac{3}{8}}$ for the comparison of the frictional power.

Dr. Kempf's remarks with regard to the work of Herr Bruckhoff on the frictional resistance of ship-shaped solids, are very interesting. I have seen some of Herr Bruckhoff's work in *Schiffbau* and "Werft u. Reederei" but was under the impression that he was endeavouring to substantiate his claims for the fundamental accuracy of the "Bruckhoffische Zahl," *i.e.*, the accuracy of the use of the 2·8853900 power of the scale ratio to express the ratio of the total resistances of two similar models at corresponding speeds. However, I am interested to learn from Dr. Kempf that the method that I proposed for estimation of the frictional resistance of a solid body was also made use of by Herr Bruckhoff. In Herr Bruckhoff's article in Werft u. Reederei (1920, Vol. I., p. 350), he gives data for two models of a torpedo boat of lengths 1 and 4·5 metres respectively. I endeavoured to apply my method to this case but found as Dr. Kempf has mentioned that n was by no means constant.

However there can be no doubt that the flow round these models is not similar. The highest speed for the smaller model was 2 metres per second and thus the product VL, *i.e.*, 2 m^2/sec. shows that the flow would be predominantly laminar. The corresponding speed of the larger model would be 4·24 m/sec. with a resulting VL value of 19·1 m^2/sec., and thus well within the range of turbulent flow. If a minimum value of VL of 5 m^2/sec. as recommended by Dr. Gebers as the lowest reliable speed-length-product or ordinary experiments, is respected, then it is very probable that a constant value of n will be obtained. I trust that when Dr. Kempf extends his research to larger comparisons, this will be found to be the case and that he will communicate his results to the scientific world.

Reply to Professors Sadler and Bragg.—I am pleased to find that Professor Sadler and Professor Bragg endorse the more practical attributes which I suggested that a system of recording experiment data should possess.

With regard to the fact that a particular model is generally restricted to a very limited range of ships there is yet something to be said in favour of the elimination of a particular basis such as 400 feet and the adoption of the one-ton model basis which I advocate. With this latter basis all branches of naval-architectural practice can be brought together and compared. Types giving exceptionally good performances will be evident to all, and it is even possible that established types of form may ultimately be dispensed with in favour of a form produced in an entirely different field. However, Professors Sadler and Bragg only particularly object to the elimination of absolute size, when the presentation becomes unduly complicated. I do not therefore construe their remarks as a criticism of the "Relative system."

The Author

Professors Sadler and Bragg emphasize the necessity for uniformity of friction allowance amongst the various experimenters and state that the diversity at present existing may easily obliterate the effect of form in a comparison of two models. This is a very important statement; and uniformity of frictional-resistance allowance should certainly be obtained before the adoption of any international system of presentation is secured. However, Dr. Gebers' work on the frictional resistance of solid bodies based upon a method similar to or identical with that proposed in Appendix 3, should, when published, go a long way towards attaining this desirable state of affairs.

With regard to the physical significance of the relative-speed expression, I quite agree with Professors Sadler and Bragg that it is speed and length that jointly provide the picture of what actually occurs. This picture is provided by the expressions adopted in the Relative system for $\frac{V}{\triangle^{\frac{1}{6}}}$ gives us the actual speed of the one-ton model whose length is $\frac{L}{\triangle^{\frac{1}{3}}}$ and thus the interference phenomena along the length of the one-ton model moving at this speed can be directly visualized. However, this visualization of wave interference is in any system rather difficult to accomplish and the significance of relative speed as the speed of the one-ton model is probably to be preferred because of its simplicity.

Professors Sadler and Bragg further suggest in this connexion that the length of a ship in the design stage is more likely to be fixed than its displacement, and hence the relative velocity expression should be $\frac{V}{\sqrt{L}}$. Admittedly, of course, this may be so, yet in merchant-ship design, because of a known or anticipated volume of trade, as it is generally the custom of a shipowner to specify speed and deadweight, the first step of the naval architect should be to convert these conditions to speed and displacement. Speed and displacement at once determine the relative speed $\frac{V}{\triangle^{\frac{1}{6}}}$ and thus the selection of the Optimum dimensions is facilitated, since $\frac{V}{\triangle^{\frac{1}{6}}}$ is constant over the range of type examined. The problem of the same vessel at different drafts is fundamentally one of different vessels, and is treated as such in the Relative system. A vessel has to be powered for its maximum draft and thus the powers at smaller drafts play but a small part in fixing the form of a particular vessel.

If model-experiment data is to be made use of at the only time when it can be scientifically serviceable, *i.e.*, in the *plastic* stage in order to produce the optimum form, I would suggest that the expression $\frac{V}{\triangle^{\frac{1}{6}}}$ which I have adopted, following Tideman, Froude, Rota, and others, is the best for the purpose.

Professors Sadler and Bragg agree more or less with the relative-

The Author

power and length expressions that I propose; and I trust that the above amplification of the advantages and physical significance of $\frac{V}{\triangle^{\frac{1}{6}}}$ with its one-ton model basis, will also ultimately receive their favourable consideration.

Reply to Mr. Mumford.—I am pleased to find that Mr. Mumford is in more or less complete agreement as to the practical utility of the "Relative" system. I agree with him that its ultimate international adoption will only be made possible through an international conference of technical societies. The formation of such a conference is urgently required to consolidate much of our knowledge of the ship-resistance problem and I trust it is not too sanguine to hope that the day is not far distant when such a conference will take place.

Mr. Mumford suggests the construction of curves for $\triangle^{\frac{7}{6}}$ and $\triangle^{\frac{53}{48}}$ to facilitate their practical calculation. I think Mr. Mumford will find that the alignment chart which accompanies the Paper is a much more convenient practical method of deriving actual powers from particular relative powers for any given displacement.

Reply to Professor Abell.—I agree with Professor Abell's suggested rearrangement of the requirements of a system of notation and to a certain extent also with the elimination of requirement 5. However, this requirement was especially inserted to point out that the adoption of a common practical system need not imply the elimination of all other systems.

Professor Abell endorses my suggestion that all expressions adopted should have an obvious physical meaning but does not stress with me the importance of numerical internationality. This latter, from the practical standpoint, means the elimination of unnecessary labour which, of course, is always to be welcomed; but from the scientific standpoint it is also justified. There can be no doubt that many valuable contributions to the discussions of papers dealing with the powers of ships are definitely withheld because naval architects eminently qualified to discuss the paper are unable to do so in the short time at their disposal owing, undoubtedly, to the diversity of presentation.

This diversity can be equally well achieved either by fundamental difference in the expression or merely numerical differences in otherwise identical expressions. The physical-significance attribute is necessary to give a qualitative value to an expression; the numerical internationality is equally necessary to bestow upon the expression a definite quantitative value.

Professor Abell prefers Mr. Bakers (P) for the expression of relative velocity. The obvious as apposed to the basic physical significance of (P) is rather involved as a concept. Bereft of its particular constant 0·746 it really gives the speed of the one-ton model divided by the square root

of its mean length. Or again it may be said to be the actual speed of a model whose mean length is unity. However, from the purely scientific point of view, it seems more rational to portray any modification of a dependent variable—due to the existence of some phenomena whose origin can only be traced to a complex source—by simple portrayal on a curve rather than by attempting to modify at the same time the independent variable in order to unravel approximately the complexity of the resulting behaviour of the dependent variable. This complexity in a phenomenon of several variables can only be understood by the progressive isolation of one of the variables. The method adopted by R. E. Froude in his *Iso-*(K) diagrams agrees precisely with the above reasoning. The aim of (P) whilst of interest can at the best only claim to be an approximate solution of the "interference" problem. For vessels of the same length and prismatic coefficient but, say, different water-plane-area coefficients, the interference phenomena must necessarily be different. When, moreover, it is remembered that the interference phenomena obtained with naked models may be materially influenced by the presence of the propeller at the very point of the interference in self-propelled experiments and full-size ships, the practical and even scientific utility of (P) may be largely diminished. The Author

Apart from this objection to (P) I think from the practitioner's standpoint it is advisable to construct the dependent variable, relative velocity, in terms of units which are fundamental and not dependent. The majority of merchant-ship designs invariably involve the specification of a given deadweight and a given speed. The designer's first step is to estimate displacement, and at this point he *should* be in a position to refer to experiment data for preliminary assistance in choice of proportions and coefficients. Throughout the whole range of possible proportions the value of the relative velocity remains constant and a designer's procedure is restricted to the selection of the best one-ton model at the imposed value of relative velocity. The search for the best one-ton model is simplified when the draught of the actual ship is restricted and thus specified. The corresponding draught of the one-ton model is thus specified and all models having materially different draughts can be at once rejected. The fixing of actual dimensions is simplified by use of the one-ton-model conception because, once the best model has been found, it is merely necessary to bring up the model to the desired displacement and thus the model dimensions are multiplied $\Delta^{\frac{1}{3}}$ and the ship dimensions are immediately obtained. Admittedly the resulting dimensions are subject to revision from stability and weight considerations for compromise can never be entirely eliminated from design. With the adoption of such a speed constant as $\frac{V}{\sqrt{L}}$ or (P) the freedom of design is hindered in the former by change in speed-length ratio when different lengths are tried.

The Author

Moreover, as pointed out by Mr. Hinchliffe, considerable diversity of opinion exists amongst designers and experimenters as to the particular length to be used. The use of $\frac{V}{\Delta^{\frac{1}{6}}}$ gets round this diversity. The use of (P) in the design stage introduces another unknown into the problem with the prismatic coefficient. Professor Abell stresses the uniformity of diagram obtained with the use of (P) as abscissa. However, even with $\frac{V}{\Delta^{\frac{1}{6}}}$ there is still uniformity although it is dislocated. When a series of relative-wave-making-power curves for constant relative lengths is plotted upon a base of relative speed $\frac{V}{\Delta^{\frac{1}{6}}}$, this diagram brings out very clearly the economy of increased relative length. The ship designer's problem is unquestionably the most economical disposition of a given displacement from the point of view of resistance. Thus the stipulation of $\frac{V}{\Delta^{\frac{1}{6}}}$ in the majority of cases in practice ought to be an *ipso facto* recommendation for the use of this speed variable in a practical system. The lessons brought out by the use of other speed variables by the various experimenters will, of course, be appreciated by designers, but for the convenience of the designer and to secure immediate internationality most diplomatically, the relative speed $\frac{V}{\Delta^{\frac{1}{6}}}$ seems to my mind to be absolutely essential.

I am pleased that Professor Abell considers the suggested expression for relative power suitable for a practical system and I quite concede that (C) may be desirable from the point of view of the experimenter whose speed and power ranges are generally enormously greater than those ever likely to be met with in practice.

I agree with Professor Abell that the combined proof of the Froude and Rayleigh laws which he prefers is a more fundamental exposition of ship-resistance similarity than are the distinct proofs included in the Paper.

I am aware of Professor Abell's discussion of Mr. Baker's 1916 I.N.A. paper with regard to his suggested splitting up of a model's resistance into its component parts by making use of a simultaneous solution of the Froude and Rayleigh laws. Professor Abell will be interested to learn, with me, that the method which I propose in Appendix 3 and which incidentally was largely the outcome of his suggestion, was used as long ago as 1908 by Dr. Gebers. The publication of the results of the further work on this subject to which Dr. Gebers has been good enough to refer in his contribution to this discussion will indicate whether the method is now absolutely practical. Naval architects will await the appearance of these results with interest.

Reply to Mr. Ainslie.—Mr. Ainslie possibly misunderstands me in assuming that I want experimenters to scrap their individual systems

and adopt some common system. This step is neither essential nor desirable. I merely propose the adoption of a common system in addition to each particular favourite of an experimenter—a very different thing. Mr. Ainslie brings forward a good point when he suggests that experimenters should publish the actual total results of the model. This is very rarely done, although Schaffran in Germany generally publishes a total model resistance curve without, however, the actual spots. I agree with Mr. Ainslie in that towing experiments on full-size vessels are urgently required. The Author

Reply to Mr. Alexander.—Mr. Alexander considers that I would have presented a strong case for the "Relative" system if I had stated more clearly its comparative advantages. As a matter of fact, it was not the aim of the Paper to "push" this "Relative" system *per se*, but to show that once the conditions required by a practical system are respected, one is more or less led to the Relative system by a process of *reductio ad absurdum.*

I am glad that Mr. Alexander emphasizes the necessity for the publication of actual experimental results. Experimenters should realize that the greater the number of investigators there are dealing with the ship-resistance problems the more rapid will be scientific progress in the subject. The non-publication of actual tank results coupled with the almost invariable publication of faired curves is something which designers are at a loss to understand. Sometimes they dismiss it as an oversight; at other times they feel inclined to consider it a policy.

Reply to Mr. Hök.—Mr. Hök stresses the superiority of a length basis over a displacement basis, in the comparison of trial and, I presume, model-experiment data. Mr. Hök is presumably referring to the 100-ft.-model basis which he used throughout his 1893 Paper. He suggests that most experimenters are still adhering to the displacement basis. This I hardly think is quite the case because Taylor has closely followed Mr. Hök in his method of comparison, using the 100-ft. model and the resistance per ton displacement as his basis. Resistance per ton (residuary resistance), of course, refers just as much to the 100-ft. model as to any other similar model at the corresponding speed. Taylor's speed-length ratio is, however, not the speed of the 100-ft. model but that of a one-foot model. Baker, Kent, and Semple, in bringing their results up to a length of 400 ft., are really respecting Mr. Hök's length basis. So far as I understand the subject, the length basis is purely one of convenience and not a fundamental necessity. The use of model-experiment data must be restricted to the powering of similar ships and thus the simplest and most homogeneous basis should be adopted. Mr. Hök's method is certainly homogeneous because his power and speed expressions are directly referred to his basis 100-ft. model. However, this basis is not so easily visualized and does not seem to bring out the length-displacement relation of a model so clearly as does the Froude one-cubic-foot

The Author

model, basis or the one-ton-model basis used by Tideman, Scribanti and others—and also in this Paper. For this reason and others given in the course of my replies to other contributors to this discussion, together with the fact that the subject of frictional resistance has been placed upon a much less empirical foundation since 1893, I think that the Relative system should be preferred to that suggested by Mr. Hök. Naturally, of course, Mr. Hök will recommend the use of his system, as I, of course, do mine. However, I think that the subject is not really one upon which individual opinion should be the deciding factor, but one for the consideration of which, together with several cognate problems, the appointment of some kind of international conference is most essential.

Reply to Mr. W. J. Lovett.—Mr. Lovett's remarks are very interesting. He refers to the respective tonnages owned by America and England and Continental countries. I am not quite certain whether this is the best comparison. I think tonnage contemplated or under construction bears a closer relation to the necessity for scientific research; and because of this basis one will find that the research output of Continental investigators, and especially German, is quite equal to that of our own. Ease of assimilation will thus be brought about not through paucity of research but by the adoption of some international system of presentation.

With regard to Mr. Lovett's suggestion of uniformity amongst the users of the metric and non-metric system themselves I think that he will agree that when a system, such as that which I propose, combines both systems without noticeable sacrifice of individuality, it is immediately preferable.

Mr. Lovett possibly misunderstands me when he suggests that with the "Relative" system two distinct model experiments will always be necessary. I point out in Appendix 3 that this duplication is probably only advisable in pure research. In commercial research the relative frictional power will be calculated from the usual equivalent-plank assumption, this latter being modified by an appropriate form augment factor obtained from the pure research with duplicate models of varying fullness.

Mr. Lovett does not like the relative speed basis $\frac{V}{\triangle^{\frac{1}{6}}}$ but prefers $\frac{V}{\sqrt{L}}$. In practice it will be found that $\frac{V}{\triangle^{\frac{1}{6}}}$ will always be associated with the relative length L_r and between them the wave-making properties of a particular form are quite clearly characterized.

Reply to Mr. McAlister.—In defending the Froude constant ⓒ Mr. McAlister claims that it is simpler in application than the alternative proposed by Taylor. In this connexion I would refer Mr. McAlister to my reply to Mr. Baker's contribution to this discussion.

Mr. McAlister prefers the use of (C) because of its flattened nature and its consequent accentuation of interference and other phenomena. Whilst I concede this accentuation property of (C) it is essential for a designer to crystallize his visualizing of power curves. The ship which he evolves has a definite power curve. The visualization of this should be immediately transferable to his model power curves. This could of course be accomplished either by having both ship and model curves expressed in terms of (C) and (I) or in terms of indicated power and relative power. The alternative is not optional; the designer and shipbuilder have to convince the marine superintendent and there can be no doubt that this gentleman will have to be convinced by the conventional power curve, or in the design stage by a curve of the same nature, *i.e.*, a relative power curve. The more a designer is acquainted with this latter type of curve the sounder is his immediate judgment on an indicated power curve. The Author

Mr. McAlister, with others, emphasizes the necessity for uniformity amongst experimenters in the calculation of frictional resistance. There can be no doubt that this is urgently necessary.

Reply to Mr. Perring.—Whilst appreciating the difference of outlook between the practitioner and the experimenter, Mr. Perring admits the desirability of an unflattened power curve for the former but unfortunately considers that I class the designer in the same category as the experimenter. Surely a designer is essentially a practitioner!

Mr. Perring enumerates certain advantages of (C). These I quite concede, for some justification can always be found for most systems. For a more detailed criticism of (C) I would refer Mr. Perring to my reply to Mr. Baker. Mr. Perring considers that the adoption of $\frac{P_w}{\triangle^{\frac{7}{6}}}$ will obscure the phenomena accentuated by (C). I have already mentioned that the system that I have proposed is primarily intended for the practitioner, but I would also suggest to Mr. Perring for his consideration that perturbations that are not evident in an ordinary power curve are likely to be somewhat visually overestimated in importance from their appearance on a (C) curve.

Mr. Perring also criticizes the practicability of the method I proposed in Appendix 3 for the determination of the frictional resistance of a ship-formed solid. I have already defended this method in my reply to Mr. Baker. I think the minimum value of VL suggested by Mr. Perring, *i.e.*, $16 \times 13 = 208$ ft.^{2}/sec. is distinctly on the high side. Constancy of n really strikes at the root of all model experiments, for to be valid at all the model and the ship must both be in the turbulent range of fluid motion. To secure this validity Baker has suggested a minimum value of VL of 40 ft.^{2}/sec. whilst Gebers has shown that the value should be higher than this and at least equal to 55 ft.^{2}/sec. Gebers' opinion is

The Author

substantiated by Semple's 1919 experiments in which instability was observed on 16·67-ft. models at a $\frac{V}{\sqrt{L}}$ ratio of 0·65. Mr. Perring's suggested requisite models are therefore unnecessarily large and of too high a speed. Mr. Perring is in error in stating a speed of 18·4 ft. per sec. with a 32-ft. model corresponds to 1 knot in a 400-ft. boat. 18·4 ft. per sec. is roughly 10·9 knots, the $\frac{V}{\sqrt{L}}$ ratio of the model is 1·93 and the corresponding speed of the 400-ft. ship is 38·6 knots. Mr. Perring's resulting observations on the futility of the proposed method are therefore untenable.

Reply to Mr. Lockwood Taylor.—Mr. Taylor suggests that I have not proved my case for the adoption of $\frac{P_w}{\Delta^{\frac{7}{6}}}$, etc., instead of the Froude expression $\frac{P_e}{\Delta^{\frac{2}{3}} \cdot V^3}$. My reasons for the adoption of the expressions $\frac{P_w}{\Delta^{\frac{7}{6}}}$ and $\frac{P_f}{\Delta^{\frac{23}{48}}}$ were briefly as follows : -

(1) They give the actual wave and frictional horse-powers of the one-ton model, and are hence tangible quantities.

(2) They lend themselves to simple and consistent nomographic evaluation.

(3) They are consistent with the speed and length bases adopted.

(4) The resulting relative-power curve simulates the indicated power curve of the ship and should hence appeal to the practitioner.

For a detailed criticism of the Froude expression (C) from the standpoint of the practitioner I would refer Mr. Taylor to my rely to Mr. Baker.

Mr. Taylor's remarks on the work contained in the appendices to the Paper are very interesting. His suggestion to express Equations 8 and 18 is a general functional form has the advantage that the resulting expressions are more obviously general than those that I proposed. However, as the index c in these expressions is in itself quite general, the alternative form proposed by Mr. Taylor is fundamentally no more accurate than the form that I adopted. In the empirical solution of the Rayleigh law when the functional form of $\left(\frac{vl}{\nu}\right)$ is found from experiment the usual presentation is of the form I make use of, *i.e.*, $\left(\frac{vl}{\nu}\right)^c$. It is the actual experimental determination of the nature of this function which I call approximate and not the expression $\left(\frac{vl}{\nu}\right)^c$ itself as implied by Mr. Taylor.

Mr. Taylor's remarks on Appendix 3 I regard as a valuable addition to that section. Mr. Taylor will be interested to learn from Dr. Gebers' discussion of my Paper that the method proposed in

Appendix 3 has been used by Dr. Gebers since 1908. Mr. Taylor's analysis will be useful to check the reliability of the proposed method. However, I think that if both models are in the turbulent VL range and if the maximum scale ratio is made use of, each model being tried at very small intervals of speed, the method will be quite practical. All spots should be plotted on one diagram, the resistances of the larger model being plotted on the corresponding speed abcissa of the smaller model. If necessary the spots of both should be adjusted and the best curve drawn through them. In rejecting any spots the relative position of the corresponding spot on the other model's curve should be carefully considered so as to differentiate between departures which may be due to experimental inaccuracies and those possibly due to hydrodynamic phenomena. The Author

General Reply.—If any conclusion may be drawn from the discussion —which I presume, can safely be considered representative—it is that international agreement is urgently necessary on many branches of model-experiment science. Principal amongst these is the question of frictional resistance. In Appendix 3, I endeavoured to show how the frictional resistance of a model could be most accurately determined. Following upon this, Dr. Gebers' announcement that he has already carried out considerable research by this method, the early solution of the frictional resistance diversity problem seems to be quite possible. Other questions such as minimum size of models, correlation of "open," "joint" and automatic hull and propeller experiment results require discussion. Towing experiments on full-sized vessels are also desirable. Finally, agreement in presentation as outlined in my Paper is also essential. The representative and extensive nature of the discussion which my Paper has produced indicates that an international consideration of the whole subject in the form of a symposium would be not only as above mentioned, desirable, but also quite practicable and possible. I would therefore suggest that this Institution should either organize; or lend its weight to the formation of, an international symposium on the question of model-experiment science. The value of the findings of such a symposium, to the practitioner, the shipowner, and the experimenter would be inestimable.

REVIEW OF DIFFERENT TYPES OF MARINE INTERNAL-COMBUSTION ENGINES.

(Read in Newcastle-upon-Tyne on the 12th January, and Discussed on the 26th January, 1923).

SPECIFICATION.

Contributors of Papers to the Review were required by the Institu tion to fulfil the undermentioned conditions :—

Vessel to be in accordance with the accompanying diagram.*

Note :—The space available for the machinery is shown by figure dimensions and is more than sufficient to obtain the 32 per cent deduction from gross tonnage. This deduction would be obtained if th length of the engine room were reduced to 40 feet.

A further diagram (Fig. 1) is supplied showing the estimated leas b.h.p. required at different revolutions of the propeller in order t develop the thrust horsepower required to maintain $10\frac{3}{4}$ knots on tria The estimate is based on R. E. Froude's published experiment dat for four-bladed bronze propellers. At each number of revolutions th b.h.p. shown is that which would be required to drive a propeller c the given type having a diameter and pitch ratio giving the bes efficiency, subject to the condition that the diameter must not excee 15 feet 9 inches, the maximum allowable by the size of the scre aperture. While the actual b.h.p.'s shown by this curve are, of course open to dispute, it is thought that the proportionate increase of powe required to drive the ship with faster-running engines will be generall admitted.

Boilers.—The boiler (shown on the plan) is intended to generat sufficient steam for the following purposes, which may require stea simultaneously :—

(*a*) Steam windlass, $9\frac{1}{2}$ inches by 12 inches.

(*b*) 9 Steam winches, 7 inches by 10 inches.

(*c*) 1 Steam winch, 7 inches by 12 inches.

(*d*) 1 8 kw. electric generating set.

(*e*) Steam heating of accommodation.

At sea only (*d*) and (*e*) will be required and the surplus boiler powe can be used for steam steering (which would require an $8\frac{1}{2}$ inch b

* This Diagram is reproduced by the majority of the contributors to the Review.

The principal dimensions of the vessel specified were as follows :—

Length overall	371′ 0″
Length between perpendiculars	360′ 0″
Breadth, extreme over shell plating	51′ 0″
Breadth, moulded	50′ 9″
Depth, moulded	28′ 0″
Draught of water (summer)	23′ 0″
Displacement on 23′ 0″ draught	9,220 tons
Estimated dead weight on 23′ 0″ draft including fresh water, stores and spare gear, and allowing 430 tons for weight of propelling machinery ...	6,570 tons
Speed on trial, fully laden	$10\frac{3}{4}$ knots

8½ inch engine) bilge pumps, or any engine auxiliaries which the main engine designer desires to be driven by steam.

The engine designer should instal a boiler of sufficient size to provide for the whole of the auxiliaries mentioned above, together with any others which he intends to be steam driven.

On the other hand, the main engine designer may, if he prefers fit no boiler at all subject to the provision of equivalent power in electric generators driven by internal-combustion engines for the auxiliary purposes mentioned above, although it may be necessary for the vessel to be fitted with one smallish boiler if only for accommodation heating purposes.

Papers to give the following information :—

1. Total b.h.p. of the propelling engines, stating whether it can be maintained continuously, and, if not, for what length of time.

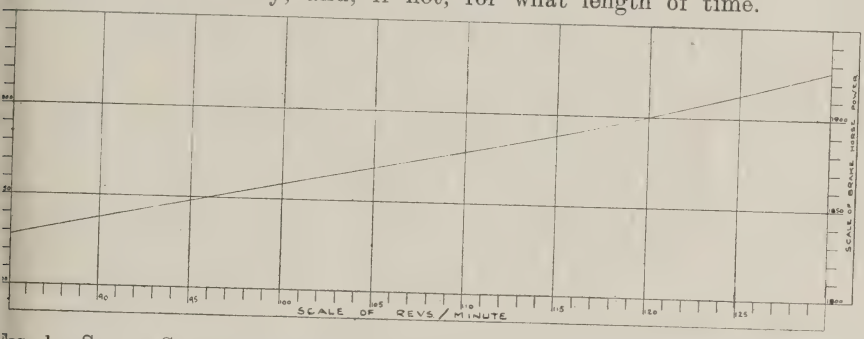


FIG. 1.—SINGLE SCREW MOTOR DRIVEN CARGO VESSEL. CURVE SHEWING ESTIMATED B.H.P. FOR A SPEED OF 10¾ KNOTS ON TRIAL WITH BEST PROPELLORS AVAILABLE FOR DIFFERENT REVOLUTIONS.

2. Overload obtainable in b.h.p. above trial conditions.

3. Mechanical efficiency of the engine, stating the basis upon which the figure has been arrived at.

4. Consumption of fuel per b.h.p. when the ship is steaming at the designed speed :—

(*a*) For main engines only, and

(*b*) For all purposes.

5. The brand of fuel assumed and its calorific value. What fuels would be recommended for continuous running, and whether the engines would run satisfactorily on the heavier boiler fuel oils for a continuous period.

6. The total weight of the main propelling engines and auxiliaries in engine room.*

* Subsequent to the meeting held on the 12th January, when the Review Papers were read, it was decided that weight of machinery should include everything from propeller to funnel.

It was also decided at the same time that contributors of papers to the Review would include only the following items in the cost per ton-mile included in their proposals :—

(1) Fuel Oil. Price, 70/- per ton Calorific value 18,000 B.T.U. with electric auxiliaries only.
(2) Boiler Fuel, at 60/- per ton.
(3) Lubricating Oil, at 3/- per gallon.

The relevant figures in the following Review Papers now comply with these conditions.

7. Cost per ton-mile.*

Authors to be prepared to guarantee their horse-power figures, required to do so.

Length of Papers:—Not over 1,000 words.

Diagrams:—Not more than three, *i.e.*, profile, plan and midsh section.

The following questions were asked by one of the contributors Papers to the Review, and the answers indicated were supplied by t Institution:—

1.—*Q*. What fully-laden sea speed is required?

A. It is not desired to specify a sea speed for the vessel becau so many indefinite conditions unconnected with the pe formance of the engines are involved, in the relati between trial speed and sea speed. It is considered th the contributor would best define the practical efficien of the engine which he describes by giving the followi information:—

(*a*) The b.h.p. which the contributor is providing in t engine to enable a speed of $10\frac{3}{4}$ knots to be mai tained on a four-hours' trial.

(*b*) What b.h.p. (or percentage of the trial b.h.p.) c be maintained by the proposed engines on a 3,00 mile run under ordinary sea conditions.

2.—*Q*. Is it necessary that the steam power of the boiler should such that the windlass and winches are to be operat simultaneously? (Presumably, when the winches a working the steam windlass will not be in use).

A. It is considered that for the purpose of securing uniformi of conditions under which the contributions are submitte the boiler should be of sufficient capacity to supply t cargo winches and the windlass simultaneously if both a steam driven as laid down by the specification issued the 10th June, 1922. It is, of course, open to the co tributor to provide for one or both of these services to electrically driven if the steam boiler power saved in th way is replaced by the equivalent in electric generators

3.—*Q*. Maximum space available for machinery. We presume th means the minimum space.

A. No limit is set on the length of machinery space which m be taken by the engine designer, but it is requested th contributions will show the difference between the length machinery space taken by them and that shown on t standard drawing issued with the latter inviting the co tribution, and that they will state the volume in cubic f thus added to or subtracted from the available cargo spa

Q. Total cost per ton-mile. We take it this will be the c only of fuel and lubricating oil, for the machinery.

* See footnote on previous page.

A. If the contributor is prepared to give an estimate of the total average cost per ton-mile of carrying cargo in the ship, specifying the assumptions on which the estimate is based, members of the Institution will, no doubt, be pleased to consider and discuss the figures. Inasmuch, however, as the Papers are expected to show the effect of design and type of engine on the cost of cargo carrying it is not desired to lay down the requirement that any estimate beyond the cost per ton-mile of fuel and lubricating oil and number of engine-room staff should be given. For purposes of comparison the speed of transport on which this estimate is based should be that obtained *under trial conditions* when developing the b.h.p. which can be developed by the engines continuously on a voyage of 3,000 miles.

Armstrong-Sulzer Diesel Engine.

By Sir W. G. Armstrong, Whitworth & Co., Ltd.

Proposal (A) Steam Driven Auxiliaries, Plate II.

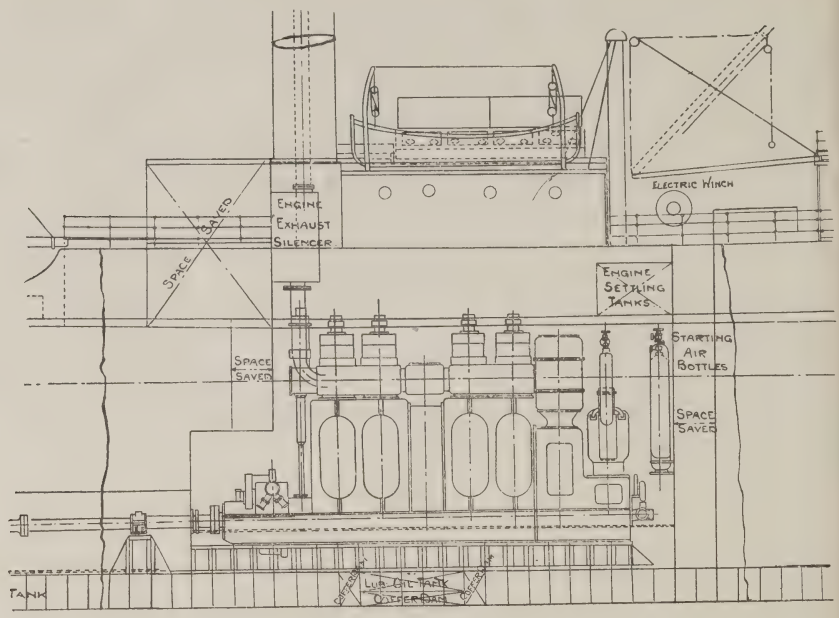
Proposal (B) Electrical Auxiliaries, Figure 2.

We will first consider proposal (A).

In this ship the nearest standard size of engine of the Armstrong-Sulzer two-stroke cycle type is one having four cylinders—680 mm. (26·77-inch) bore, 1,200 mm. (47·24-inch) stroke which will develop 2,000 b.h.p. continuously at sea at 100 revs., and for 6 hours trial would develop an overload of 10 per cent., viz.: 2,200 b.h.p. which could be increased to 18 per cent. for short periods, but with the latter overload the high resulting pressures are not to be recommended. To meet the specified requirements the engine would be run at 92 revs. at which speed it will develop 1,840 b.h.p. continuously at sea.

The mechanical efficiency of the engine, making the necessary allowance for the power absorbed in friction of the engine together with the power required for driving the separate turbo-blower, air compressor, cooling water and lubricating-oil pumps, is approximately 73 per cent. to 75 per cent.

The consumption of fuel is 0·386 lb. per b.h.p. for the main engine only, and for all purposes under sea-going conditions approximately 0·512 b. per b.h.p. which includes the fuel for the donkey boiler. The consumption of fuel for the main engines is on the assumption that the fuel oil has a nett calorific value of 18,000 B.T.U.'s.



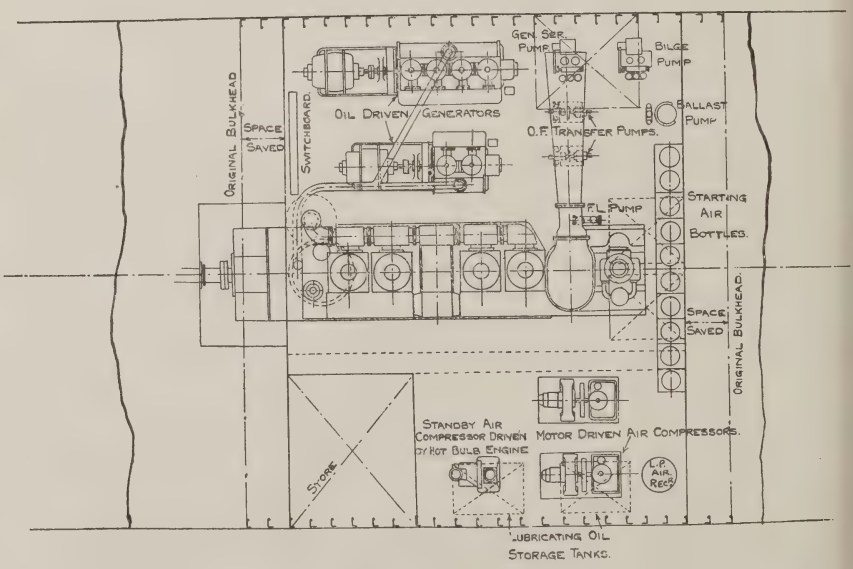


Fig. 2.—Proposal B. Diesel Driven Cargo Vessel. Armstrong-Sulzer Engines and Electric Driven Auxiliaries.

The engine will run satisfactorily on the heavier boiler fuel oils for a continuous period if the content of sulphur is not more than 3 per cent. and the content of asphaltum not more than 15 per cent. With such fuel oils the engine will, of course, require more frequent overhauls than if run on any of the usual Diesel oils.

The air compressor, bilge and lubricating-oil pumps are driven off the main engine. The scavenge air and cooling-water pumps are driven by steam turbines which exhaust into the winch condenser. The auxiliary air compressors and the remaining auxiliaries are of the ordinary steam reciprocating type.

There are two oil-fired Cochran boilers as shown, one of which will be used at sea and two in port. The arrangement shows the two Cochran donkey boilers in the engine room as it is thought advisable to have the installation under the supervision of the engineer on watch and the natural draught with this arrangement is also improved.

The overall length of the engine should be noted. This is obtained by the introduction of an independent steam-turbine-driven scavenge blower, which supplies scavenge air to the cylinders.

When comparing the merits or demerits of an oil engine the factor that predominates over all others in reliability. Simplicity of design ensures reliability, and the details in this engine, are remarkably simple, as for instance, the cylinder cover and the reversing gear, etc. The liner is perhaps a little more complicated than the four-stroke type, but it must be remembered that the temperature of the exhaust gases is only 460° F., the bars between the ports are efficiently cooled and hundreds of liners of this type have been subjected to working conditions over many years without trouble.

The valve-controlled super-charge port in the Armstrong-Sulzer engine is one of its special features. The introduction of a blast of pure air into the cylinder as in this engine is well worth the few additional details.

The piston head is cooled by water through telescopic pipes arranged in such a manner that the gland is never subjected to water pressure. In the Armstrong-Sulzer cylinder cover, there is only one housing carrying the fuel spray injection valve and the starting-air valve; hence a very simple design of cylinder cover is obtained. The fuel pump, governor and the rotary scavenge valves are driven off the vertical shaft at the aft end of the crank-shaft, each cylinder being supplied with fuel oil from its own plunger in the fuel pump. The fuel pump supply is controlled by hand and by the governor.

The starting and manœuvring engine consists of two air-driven inverted cylinders set at about 60° to each other with a very neat arrangement of interlocking gear. The starting is effected by first putting compressed air on all cylinders then two on air and two on oil then all four on oil. During this operation the fuel pump, lubricating-oil supply, etc., are all automatically brought into operation in their proper sequence.

The lubricating-oil pump on the engine discharges the oil to the gravity tank which supplies the main bearings and the bottom ends. The cross-head lubrication is supplied by two small pumps at a suitable pressure. The cylinders, air compressors, and the rotary valves are lubricated by small sight feed pumps driven from the cam-shaft. The air compressor, which is driven off the crank-shaft, is of the three-stage, stepped-piston type. The valves are of the automatic plate-ring type, the l.p. and i.p. valves being identical to reduce the amount of spare gear carried.

The capacity of the compressor is sufficient for spraying air injection with a margin when necessary for the charging of the starting-air bottles, the quantity of suction air to the compressor being under hand control.

The total weight of the main propelling machinery, auxiliaries, etc., in proposal (A) is approximately 405 tons. The cost per ton-mile, (nautical miles), assuming 6,500 tons of cargo, in proposal (A) is approximately 0·00533 pence. This figure includes oil and lubricating oil.

The amount of cargo space saved in the proposed ship is 14,640 cubic feet in proposal (A) and 11,020 cubic feet in proposal (B) which follows.

Proposal (B) shows an Armstrong-Sulzer engine with electric auxiliaries, and in this proposal the scavenge-air pump is arranged on the main engine to avoid the necessity of fitting a high-speed-motor-driven scavenge blower as at present there is some difficulty in obtaining suitable electric motors in this country.

In this arrangement, the bilge, sanitary, piston cooling, cooling water, and lubricating pumps are driven off the main engine. The separate auxiliaries to consist of the following motor-driven pumps:—Ballast, bilge, general service, fuel transfer pump; a suitable air-driven lubricating pump in case of emergency, two Diesel-driven generators, two motor-driven auxiliary compressors and a small stand-by hot-bulb engine are also provided.

The total weight of the main and auxiliary machinery under proposal (B) is approximately 414 tons, and the cost per ton-mile assuming 6,500 tons of cargo, is approximately 0·0047 pence. This figure includes fuel oil and lubricating oil.

In conclusion, mention should be made of the spare gear. The Armstrong-Sulzer four-cylinder engine has only twelve valves in communication with the heat of combustion and therefore very few spare parts are necessary for the valve gear.

Proposal (B) is put forward simply as a matter of interest to the members of the Institution who wish to compare the all-electric proposition with the steam-auxiliary proposal.

Camellaird-Fullagar Marine Oil Engine.

By Palmers Shipbuilding and Iron Co., Ltd., and Cammell, Laird & Co., Ltd.

The type of machinery put forward for the proposed motor cargo hip is representative of that being manufactured by the following firms of shipbuilders and engineers in this country :—

Cammell, Laird & Co., Ltd., Birkenhead.
John Brown & Co., Ltd., Clydebank.
Palmers Shipbuilding & Iron Co., Ltd., Jarrow.
Smith's Dock Co., Ltd., Middlesbrough.
David Rowan & Co., Ltd., Glasgow.
Dunsmuir & Jackson, Ltd., Glasgow.

The main features of the engine, which has been developed by Cammell, Laird and Co., Ltd., Birkenhead, are now well known, but a hort résumé of its characteristics will not be out of place.

The engine is of the opposed-piston type, the cylinder liner being an open-ended tube with exhaust ports at one end and scavenging ports at the other which are uncovered by the pistons, so that a straight-hrough scavenge is accomplished. The elimination of the cylinder cover and exhaust valves are useful objects obtained by this feature of the lesign, and with the large air-port area available, the scavenging pressure for the lower-speed engines is only 1 to 1·5 lb. per square inch. Owing to the manner in which the upper and lower pistons are cross-onnected, the three cranks and connecting rods required by the most amiliar type of opposed-piston engine per cylinder are reduced to one f each per cylinder. Other outstanding features are the high degree f balance, which relieves the riveted engine seating of a good deal of ibration, and the balance of the piston loads, which frees the engine raming of all direct stresses. The combination of these features in ne engine has resulted in the production of a highly efficient machine hich has given fuel consumptions down to 0·39 lb. per b.h.p. per hour, nd, with the inclusion of further improvements in later designs, it s anticipated that this figure will be reduced.

The absence of exhaust valves makes the engine very suitable for urning lower grades of fuel than those in common use, and one engine as recently completed a satisfactory run of one week on Mexican boiler uel oil, having a specific gravity of 0·95. Particulars of this have lready been published In addition, two engines have made runs at ea of about 600 hours each on Borneo fuel oil having a specific gravity f 0·937.

The push-and-pull applied to each crank of the engine comes in seful when slow speeds are desired for manœuvring. Professor Watkin-on, of Liverpool University, conducted some tests on a four-cylinder

25-inch-stroke engine, and certified it to run steadily without misfirin at 16½ r.p.m., which corresponds to a piston speed of 68·7 feet pe minute—the lowest piston speed, so far as is known, at which an internal combustion engine has successfully operated continuously. From thi performance it will be evident that with longer strokes and six-cylinde engines, lower revolutions can be obtained if desired.

The engine chosen for the present ship, Plate III., is a four-cylinde one having cylinders of 24 inches diameter by 36 inches piston-stroke running at a speed of 85 r.p.m., or the lowest speed permitted by th chart accompanying the invitation of the Institution. Electrically driven auxiliaries are assumed, this being the most modern, convenien and economical system.

A smaller and higher speed engine could be put forward, but as th engine room is already down to the minimum allowed by the tonnag laws, there would be no gain in cargo space, and the slower-speed, mor efficient engine makes the most attractive proposal.

At the revolutions selected the b.h.p. required is 1,828, and the mea indicated pressure in the cylinders would be the moderate one of 91 lb per square inch, or, if referred to b.h.p., approximately 65 lb. pe square inch, and this rating can be guaranteed as a continuous one The piston speed is only 510 feet per minute, and the effect of this o overhauling and reliability need not be enlarged upon. Opposed-pisto engines having brake mean pressures of 80 to 90 lb. have been satis factorily at work for long periods, so that an overload capacity up t 2,300 b.h.p. at 92 r.p.m., which is approximately the speed according to the propeller law (an overload of 25 per cent.) would be easil available. The apparently low continuous rating adopted is convenien in many ways, as it provides a very large overhead factor that can b drawn upon in cases of emergency.

The main engine drives all its own circulating pumps, compressor scavenging pumps, etc., the object being to render the operation of th engine independent of all auxiliaries, as stoppage of the auxiliary engin driving the circulating pumps, etc., would necessitate stopping the mai engine. It is also the most economical system, as the continuousl running pumps are driven by a high-efficiency prime mover and th double electrical losses are avoided. Incidentally there is anothe important advantage in this arrangement. It is well known that the firs start of a Diesel engine is as a rule the most difficult as the engine i cold. With separately-driven circulating pumps the engine is coole down as soon as it stops, so that each start is a cold one unless a attendant is told off to manœuvre the pumps.

The mechanical efficiency of the engine would be about 73 per cent. and is simply represented by the b.h.p. divided by the indicated wor developed by the working cylinders only. This figure would be somewha higher if, as is the case with most marine oil engines, the cooling pump were driven separately.

When developing the full rated power of 1,828 b.h.p., the main engine only would consume 0·4 lb. per b.h.p. per hour. The consumption with boiler oil at 60s. per ton would be 0·42 lb. With one auxiliary engine running for driving the steering gear, ship's pumps that may be working intermittently and for lighting and heating, about 50 b.h.p. would be required. This figure is based on actual data from ship's running, and will obviously vary slightly according to climatic conditions. If 0·25 tons per day is allowed for this item, the daily fuel consumption will be approximately 7·9 tons.

The fuel used would have a gross calorific value of 18,000 B.T.U. per lb., and, as already stated, the engine would operate for long periods on many boiler fuels, but at a slightly higher consumption as in addition to the inferior heating value the combustion properties of such oils are not so good.

The total weight of machinery in the engine room would be approximately 430 tons, but if higher piston speeds common to other engines were adopted this figure could be considerably reduced. As it is, it represents a saving in weight of about 70 tons over similar steam-engine installations. The weight given includes main engine, auxiliary Diesel engines, pumps used as stand-by to those on the engine, all ship's pumps, fuel tanks and all systems in connexion with the main and auxiliary engines.

For actual performances of Camellaird-Fullagar engines at sea, the fuel cost would be 0·00432 pence per ton-mile with Diesel fuel oil at 70s. per ton. With boiler fuel oil at 60s. per ton, the cost would come down to 0·00392 pence per ton-mile.

Doxford Opposed-Piston Marine Oil Engine.

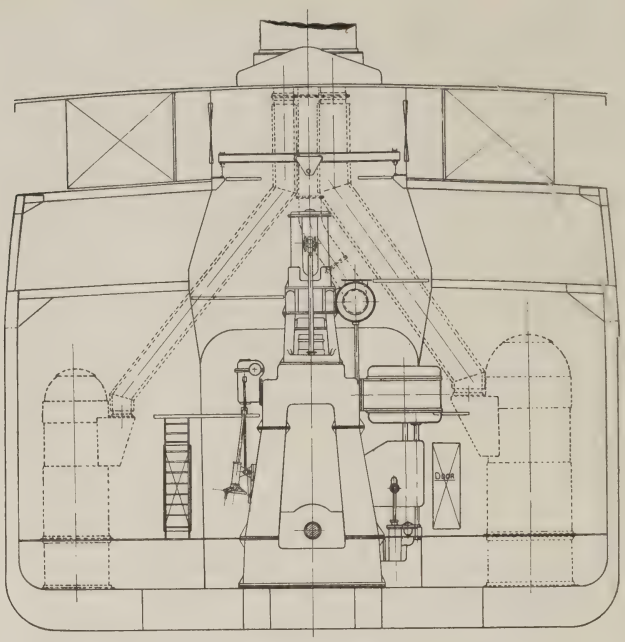
By Messrs. William Doxford and Sons, Ltd.

Following the invitation of the Council of the Institution to submit a proposal for a marine oil-engine installation based upon a given specification, my firm, Messrs. Wm. Doxford & Sons, Ltd., Sunderland, have pleasure in submitting the following plans (Plate IV. and Figs. 3 and 4) and description of machinery arrangement for discussion.

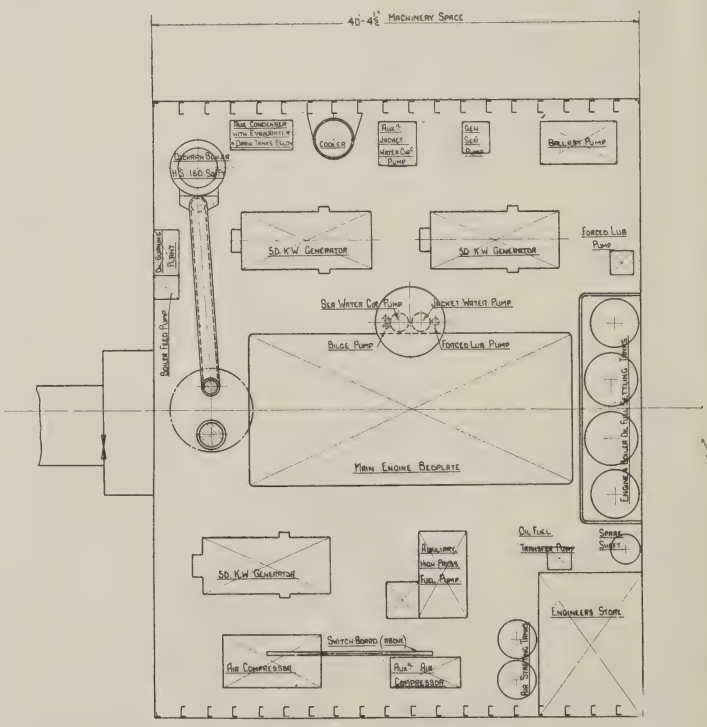
The installation consists of a single-screw three-cylinder unit of 540 mm. (21¼ inches) diameter and 2 by 1,080 mm. (42½ inches) stroke.

The engine is of the Doxford opposed-piston two-stroke type operating on solid injection.

The double-acting scavenging pump and the necessary auxiliary pumps for making the engine self-contained at sea are driven by levers from the centre cross-head of No. 2 cylinder.



Section Thro Engine Room
Looking Aft



Plan shewing electrically Driven Auxiliaries

Fig. 3.—Proposed Single Screw Arrangement with Doxford Opposed Piston Oil Engine.

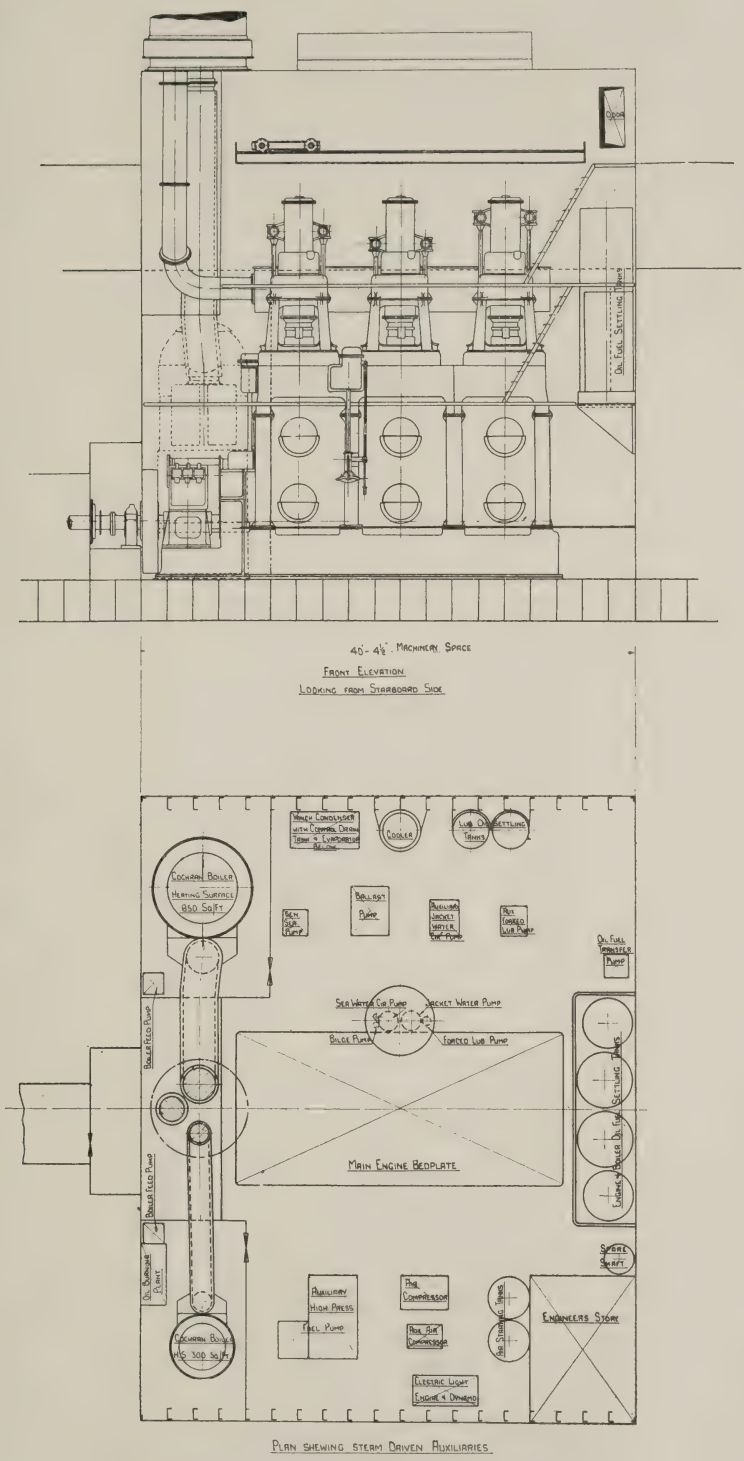


FIG. 4.—PROPOSED SINGLE SCREW ARRANGEMENT WITH DOXFORD OPPOSED PISTON OIL ENGINE.

Aft of the engine are arranged on a combined baseplate, which is bolted direct to the main engine bedplate, a Michell thrust bearing, a three-throw high-pressure fuel-oil pump and a fly-wheel.

There are two cam-shafts, one in front of the engine for going ahead and astern, and one at the back of the engine which is operative only when going ahead. Both cam-shafts are driven by spur and bevel gearing from the thrust shaft.

The crank-shaft is of the built-up type, and so arranged that the shaft for each cylinder can be removed without having to dismantle other parts of the engine. In case of need the crank-shaft for any one cylinder can be lowered into the crank pit and replaced by a straight length of shaft. In this case only two cylinders would be operative.

Forced lubrication is applied to main bearings, connecting-rod bearings and cross-head guides, while each piston is so lubricated at six points around the cylinder.

Each cylinder is provided with two fuel-injection valves, one air-starting non-return, and one relief valve.

Fresh-water cooling is used for cylinders, pistons, and cylinder valves, whilst sea water is used for the cross-head guides and for cooling the fresh water in a tubular cooler.

Two machinery arrangements are submitted, one with steam auxiliaries and one with electrical auxiliaries. In each case the minimum length of engine room of 40 feet fixed for this vessel, to come within the 13 per cent. for light and air space has been adopted.

Steam Auxiliaries.—The plan of the ship proposed for this review shows the donkey boiler on deck. This arrangement has in numerous cases been the practice with steam propelling machinery, but it should be borne in mind that it was only on rare occasions that the donkey boiler was called upon to operate. The case is different with a motor installation, where the donkey boiler virtually becomes the main boiler, and as such its proper place is in the engine room and preferably accessible from the engine-room floor.

This arrangement shows the boiler split up into two units, a larger and a smaller one, the former serving for harbour use while the latter will serve at sea for supplying steam for accommodation, steering gear, electric-lighting set and whistle.

Electric Auxiliaries.—Three electric generating sets are proposed, each of 50 kw., D.C., 220 V. direct coupled to a two-cylinder oil engine. Two of these would be required for harbour use to drive electric winches, and one would serve at sea for electric light and steering gear. An 8 kw. converter is required for the electric light service to reduce the voltage from 220 to 110. A syren worked by compressed air from the starting-air bottles would take the place of the steam whistle. A small motor-driven compressor, which would come well within the capacity of one generating set, would serve to press up the air bottles periodically.

A small donkey boiler would serve to heat accommodation and fuel oil (if necessary) and also to warm up the main engine prior to starting up.

In reply to numbered questions contained in the Institution's Specification :—

1. *Total b.h.p. of the propelling machinery.*
For $10\frac{3}{4}$ knots, 1,850 s.h.p. at 96 revs.

2. *Overload obtainable in b.h.p. above trial conditions :*
$11\frac{1}{2}$ knots, 2,270 s.h.p. at 103 revs.

3. *Mechanical efficiency of the engine, stating the basis upon which the figure has been arrived at.*

82 per cent. based upon test-bed records with four cylinders, 3,000 i.h.p. marine engines, which have given 85 to 86 per cent. mechanical efficiency when working against Heenan & Froude water brake.

4. *Consumption of fuel per b.h.p. when the ship is steaming at the designed speed.*

Diesel Oil	(*a*)	8 tons per day for engine only.
0·4 lb. b.h.p. hour.	(*b*)	$8\frac{1}{4}$ tons for engine and electric auxiliaries.
$10\frac{3}{4}$ knots.	(*c*)	9 tons engine and boiler at sea.
Boiler Oil	(*a*)	$8\frac{1}{2}$ tons per day for engine only.
0·42 lb. b.h.p. hour	(*b*)	$8\frac{3}{4}$ tons for engine and electric auxiliaries.
	(*c*)	$9\frac{1}{2}$ tons engine and boiler at sea.
Diesel oil	(*d*)	$9\frac{3}{4}$ tons per day engine only.
0·4 lb. b.h.p. hour.	(*e*)	10 tons per day engine and electrical auxiliaries.
$11\frac{1}{2}$ knots.	(*f*)	$10\frac{3}{4}$ tons engine and boiler at sea.
Boiler oil	(*d*)	$10\frac{1}{2}$ tons per day engine only.
0·42 lb. b.h.p. hour	(*e*)	$10\frac{3}{4}$ tons per day engine and electric auxiliaries.
	(*f*)	$11\frac{1}{4}$ tons per day engine and boiler at sea.

5. *The brand of fuel assumed and its calorific value. What fuel would be recommended for continuous running, and whether the engines would run satisfactorily on the heavier boiler fuel oils for a continuous period.*

Diesel oil, 0·895 s.g.—19,200 B.T.U. lb. higher value ; 18,000 B.T.U., lower value.

Boiler oil, 0·945 s.g.—18,300 B.T.U. lb. higher value ; 17,200 B.T.U. lb. lower value.

We recommend the use of Diesel oils as these have generally a lower percentage of ash, and consequently the wear on cylinder liners and piston rings is reasonable. Boiler fuel oils can be used satisfactorily and continuously and without any lighter fuel oils as a standby, but extra wear on cylinder liners and piston rings would have to be taken into the bargain on account of the hard particles of ash which some of the heavier boiler oils contain.

6. *Total weight of the main propelling engines and auxiliaries in engine room.*

430 Tons with steam auxiliaries and steam up. 450 Tons with electrical auxiliaries ready for sea.

7. *Cost per ton-mile in pence and* 6,570 *tons d.w.*

Oil in accordance with Institution Specification.

	Steam Auxiliaries. Diesel Oil.	Steam Auxiliaries. Boiler Oil.	Electrical Auxiliaries. Diesel Oil.	Electrical Auxiliaries. Boiler Oil.
$10\frac{3}{4}$ Knot Voyage—				
$8\frac{1}{4}$ tons fuel per day	....	....	0·0041	....
$8\frac{3}{4}$,, ,, ,, ...	....	....	....	0·0037
9 ,, ,, ,, ...	0·0044	....	....	....
$9\frac{1}{2}$,, ,, ,, ...	....	0·00405	....	....
11 gallons per day lubrication	0·0002	same.	same.	same.
$11\frac{1}{2}$ Knot Voyage—				
10 tons fuel per day	....	....	0·00466	....
$10\frac{3}{4}$,, ,, ,, ...	0·00495	....	....	....
$10\frac{3}{4}$,, ,, ,, ...	....	....	....	0·0043
$11\frac{1}{4}$,, ,, ,, ...	....	0·0045	....	....
13 gallons per day lubrication	0·0002	same.	same.	same.

8. *Engine-room crew.* 3 Engineers. 3 Greasers.

Sizes and Capacities of Pumps and Auxiliaries in Engine Room.

Lever Driven Pumps on Main Engine.

1 Sea-water circulating pump	100 tons per hour.
1 Jacket-water circulating pump	70 ,, ,,
1 Forced-lubrication pump	25 ,, ,,
2 Bilge pumps, each	25 ,, ,,

Auxiliaries.—Steam-driven.

1 Ballast pump	200 tons per hour.
1 Jacket-water circulating pump	70 ,, ,,
1 Forced-lubrication pump	25 ,, ,,
1 General-service pump	25 ,, ,,
2 Boiler-feed pumps, each	7 ,, ,,
1 Oil-fuel-transfer pump	10 ,, ,,
1 Air compressor	120 cubic feet.
1 ,, ,, ...	50 ,, ,,
1 Electric-lighting set	8 kw.
1 Auxiliary high-pressure fuel pump driven by	7″ × 6″ steam engine.
1 Winch condenser	300 sq. ft. surface.
1 Evaporator	10 tons per day.
1 Jacket-water cooler	1,000 sq. ft. urface.
1 Cochran boiler —100 lb. per sq. in.	850 sq. ft. sheating surface.
1 Cochran boiler —working pressure	300 ,, ,, ,,
1 Oil-burning unit for above.	
2 Settling tanks, each	5 tons capacity.
2 Air-storage tanks, each	110 cubic feet capacity.
2 Lubricating-oil settling tanks, each	400 gallons capacity.

Auxilaries.—Electrically-driven.

3 Electric generating sets	50 kw. each.
1 Ballast pump	200 tons per hour.
1 Jacket-water circulating pump	70 ,, ,,
1 Forced-lubrication pump	25 ,, ,,
1 General-service pump	25 ,, ,,
1 Oil-fuel-transfer pump	10 ,, ,,
1 Air-compressor	120 cubic feet.
1 ,, ,, ...	50 cubic feet.
1 Converter for electric light	8 kw.
1 Auxiliary high-pressure fuel pump	15 h.p.
1 Jacket-water cooler	1,000 sq. ft. surface.
1 Cochran boiler, 100 lb. per sq. in. working press	200 sq. ft. heating surface.
1 Oil-burning unit for above.	
2 Fuel-oil settling tanks, each	5 tons capacity.
2 Air storage tanks, each	110 cubic feet capacity.
2 Lubricating-oil settling tanks, each	400 gallons capacity.

NEPTUNE MARINE OIL ENGINE.

By Messrs. Swan, Hunter, & Wigham Richardson, Ltd.

The engine proposed will fulfil the following conditions:—

B.H.P.—Will maintain 1,840 b.h.p. at 90 r.p.m. during a six hours' trial.

Overload.—At least 10 per cent. would be obtainable above the trial b.h.p., but 25 per cent. has been obtained on a bench test.

Mechanical Efficiency.—Will not be less than 73 per cent. (deduced from bench tests of previous engines).

Fuel Oil Consumption.—The consumption of fuel oil at the designed service power will be:—

(*a*) 0·43 lb./b.h.p./hour, main engines only.

(*b*) 0·48 lb./b.h.p./hour for all purposes with steam-driven auxiliaries.

(*c*) 0·45 lb./b.h.p./hour for all purposes with electrically-driven auxiliaries.

The usual 5 per cent. should be added to the above figures for guarantee purposes.

Fuel Oil.—The above figures are based on "Diesel Oil" with a net calorific value of not less than 18,500 B.Th.U.

The engine will run satisfactorily and continuously on boiler fuel oil reasonably free from ash, the only appreciable effect being a greater accumulation of deposit in the cylinders, etc., involving more frequent

opening up for cleaning. The factor having most influence on the deposit appears to be the percentage of hard asphalt, and oils containing high percentages of this should be avoided.

The cost per ton-mile using oil as specified by the Institution would be :—

Fuel oil, 0·00488 pence.

Boiler oil, 0·00441 pence.

Total weight of Machinery Installation—430 tons.

Outline Description of Engines and Auxiliaries.

Main Engine.—Neptune Marine Oil Engine of the reversible two-cycle single-acting, port scavenging type, having four cylinders 26 inches diameter by 52 inches stroke.

Main Compressor.—Of the three-stage type, fitted at the forward end of the engine and driven from an extension of the crankshaft.

Pumps.—The following pumps are arranged on the back of the engine and driven by levers from the piston crossheads :—

2 double-acting scavenge air pumps.
2 jacket water circulating pumps.
1 piston water circulating pump.
1 lubricating oil pump.
2 bilge pumps.

Crankshaft.—Of the built-up type, in two interchangeable parts.

Cylinders.—To consist of cast iron liners of special composition fitted into a cast iron entablature. The water jacket for the main cylinder liners to be formed by means of light castings surrounding the liners but not forming a portion of the main entablature, water joints in way of the scavenge and exhaust ports thus being avoided.

Each cylinder cover consists of a symmetrical conical shaped loose liner of special composition, such liner forming the combustion space, an outer casing, and a small cover closing the small open end of the loose liner, and so arranged as to allow free expansion of the loose liner.

The small covers carry the fuel and air starting valves and connections for relief valves and indicators are provided.

Pistons are of cast iron and are fitted with a separate cast iron skirt. Piston rings are of Ramsbottom type.

The pistons are cooled by sea water supplied through telescopic tubes, which are attached to the piston and work through a specially designed stuffing box having a lantern gland and wiper ring fitted. The gear is so arranged that there are no moving water joints under pressure.

Piston rods, connecting rods, bedplate and main bearings are in accordance with usual marine practice for steam engines.

Forced lubrication is employed throughout, some appliance for filtering and purifying the oil being fitted.

Cam shaft is driven from the main engine crankshaft through eccentrics, rods and suitable gearing. An air engine is fitted for moving the cam shaft horizontally as required for ahead or astern running. Interlocking gear is provided to prevent the cam shaft from being moved horizontally while the engine is either being started up on air or running on fuel, and also to prevent the fuel cams from being in operation while the starting cams are in action or *vice versa.*

A steam turning gear is provided for turning the engine.

One fuel pump is provided for each cylinder.

The speed of the engine is controlled by hand gear connected to the suction valves of the fuel pump.

The lift of the suction valves can be regulated separately for equalizing the power in the cylinders.

A governor is provided which operates on the suction valves of the fuel pump.

A pyrometer set is provided for indicating exhaust gas temperature at each cylinder.

Thrust Block is of the Michell type.

Shafting of forged ingot steel.

Stern Tube of cast iron with a brass bush lined with lignum vitæ at the outer end.

Propeller is of cast iron.

Air Bottles.—Injection air bottles capacity, 16 cubic feet. Starting air tanks capacity, 460 cubic feet.

Auxiliary Machinery—

(*a*) With steam deck machinery as specified by the Institution :—

1 ballast pump, 9 inches by 10 inches by 10 inches, Duplex type; acts also as standby to main engine jacket water circulating pump.

1 general service pump, 7 inches by 7 inches by 8 inches, Duplex type; acts also as standby to piston water circulating pump.

2 boiler feed pumps, 6 inches by $4\frac{1}{4}$ inches by 10 inches, Simplex type.

1 condenser, 500 square feet surface.

1 evaporator, 10 ton capacity.

1 auxiliary compressor, capacity 140 cubic feet of free air per minute to 1,000 lbs. per square inch, driven by a compound steam engine.

1 fuel oil transfer pump, 4 inches by 4 inches by 6 inches, Simplex type.

1 standby lubricating oil pump, 5 inches by $4\frac{1}{2}$ inches by 12 inches.

1 donkey boiler, 12 feet diameter by 10 feet 6 inches long, with oil-burning installation. 100 lbs. per square inch working pressure.

1 waste heat boiler (100 lbs. per square inch working pressure) of special design, through which the exhaust gases from the main engine are passed, and sufficient steam will thus be

raised to provide for normal requirements at sea. An auxiliary oil burner is fitted to this boiler for use when the steam supply is insufficient.

(*b*) In the event of the adoption of electrical deck machinery the following motor-driven pumps would be provided :—

1 ballast pump.
1 general service pump.
1 lubricating oil pump.
1 fuel oil transfer pump.
1 auxiliary compressor.

also—

3 65 kw. Diesel-driven generating sets.
1 waste heat boiler for heating, etc.

The Scott-Still Marine Oil Engine.

By Scotts' Shipbuilding and Engineering Co. Ltd.

General Description.—The main propelling engine (Plate V.) consists of one set of two-stroke-cycle Scott-Still marine oil engines, with four cylinders 25 inches diameter by 45 inches stroke. Forced lubrication is supplied to the main bearings, connecting-rod bearings, and guides.

Airless injection of fuel to the cylinders is adopted, and the fuel valves are automatic in action, being arranged to open under the action of the fuel pumps. One fuel pump is provided for each cylinder, worked by a cam fixed to the engine crank-shaft. Arrangements are made for running the main engine ahead and astern on oil.

Operation of the steam inlet and exhaust valves is controlled by distributors driven by gearing from the crank-shaft at engine speed, and reversing is accomplished by the movement of piston valves in the distributors. A pump of Hele-Shaw type, with automatic variable-delivery gear, supplies oil at 400 lb. per square inch to the distributors for the operation of the steam valves. This pump is driven off the condenser circulating pump, which is arranged to suit this duty.

The scavenging air is supplied by a turbo-blower driven by exhaust steam from the main engine. The turbine is of 80 b.h.p. with a normal output of 7,000 cubic feet per minute, and the air discharge is led to the engine bed-plate for distribution to the cylinders. The air is drawn from a cowl on deck in the usual manner and a reservoir or tank, suitably drained, is interposed to arrest water or grit.

The engine has a fly-wheel of cast iron with teeth on the rim to take the worm on the turning engine. The thrust block is of Michell type, arranged for forced lubrication.

A regenerator or boiler provides steam generating surface of 1,150 square feet, to raise steam at a pressure of 150 lb. per square inch, for starting and manœuvring the main engine, oil being supplied to the

burners for these purposes. When the engine is "under way" this regenerator (the burners having been shut off) serves the purpose of a steam and water reservoir in which steam is raised, from the heat in the exhaust gases and cylinder jackets, and utilized in the main-engine cylinders for the production of power. Three oil-fuel burners are fitted and the burning plant is of Wallsend-Howden type, an extra pump being provided as a stand-by. The gases from the regenerator pass through a feed heater and thence up the funnel. Advantage is taken of the steam for the purpose of cooling the main-engine pistons.

A group of pumps is driven by levers from the forward end of the main-engine crank-shaft, comprising the following:—Air pump, feed pump, forced-lubrication pump, bilge pump, sanitary pump, and regenerator circulating pump. The duty of the last-named pump is to draw water from the regenerator and return it thereto through a pressure filter with the object of cleansing the water. A stand-by feed pump is provided of Weir's vertical type, 4 inches diameter by 6 inches by 12 inches stroke. A pump of similar type is fitted, 6 inches diameter by $13\frac{1}{2}$ inches by 15 inches stroke, to take the duties, on emergency, of either the engine-driven forced-lubrication pump or the valve-gear oil operating pump.

A condenser designed for a vacuum of 28 inches is provided, having 600 square feet cooling surface. The condensate is effectively filtered before entering the feed-water heater. The circulating pump, already referred to, is of centrifugal type, 7 inches bore, running at 420 r.p.m., and discharges 800 gallons per minute.

The other auxiliaries included are:—Two general-service vertical duplex pumps, 7 inches diameter by 9 inches by 8 inches stroke; one ballast vertical duplex pump, 9 inches diameter by 10 inches by 10 inches stroke; one oil-fuel-transfer pump, 5 inches diameter by $4\frac{1}{2}$ inches by 10 inches stroke; one electric generator giving 8 kw. at 110 volts, and a steering engine of "Wilson-Pirrie" type (not shown on the arrangement of machinery), with cylinders $8\frac{1}{2}$ inches diameter by $8\frac{1}{2}$ inches stroke. All the auxiliaries are steam driven. In connexion with the feed "make up" a quantity of fresh water is carried in the ship's tanks amounting to 40 tons. In addition, an evaporator of 10 tons capacity per day is fitted. The winches and windlass are operated by steam supplied by the regenerator, which for this purpose fulfils the function of a donkey boiler. For this duty oil fuel is employed in the burners.

Statement embodying the information required in connection with the Scott-Still propelling machinery, the item numbers given corresponding with those in the Institution's Specification.

1. (*a*) The total b.h.p. of the propelling engines to enable a speed of $10\frac{3}{4}$ knots to be maintained on a 4 hours' trial is 1,870.

(*b*) The b.h.p. that can be maintained on a run of 3,000 nautical miles under ordinary sea conditions is 1,725.

2. An overload in b.h.p. above trial conditions of 35 per cent. can be obtained.

3. The mechanical efficiency of the engine is 88 per cent.

The bases upon which this figure is arrived at are as follows:—The mechanical friction of the engine is equivalent to 11 lb. per square inch of piston area (from data taken from extensive trials of a similar engine). The mean pressure equivalent to 1,870 b.h.p. at 106 revolutions per minute is 79 lb. per square inch. The engine efficiency is therefore $\frac{79}{90}$ or 88 per cent.

4. (*b*) The consumption of fuel per b.h.p. for "all purposes" is estimated at 0·37 lb.

This is the only significant figure in the particular proposal put forward, where all the auxiliaries, including the scavenging blower, are driven by steam produced from the waste heat of the main engine.

5. The brand of fuel assumed is Anglo-American Diesel Oil, and its calorific value is 18,000 B.T.U. net. From examinations which have taken place on various occasions after trials, admittedly of short duration (8 hours), of a similar engine, with the heavier boiler fuel oils, there is nothing to suggest that these oils cannot be used satisfactorily in the engine.

6. The total weight of the main propelling engines and auxiliaries in engine room is estimated at 375 tons. This does not include the weights of steering engine, winches or deck pipes.

7. (*a*) The cost per ton-mile of fuel and lubricating oil is 0·0037 pence (taking fuel oil at 70s. per ton, lubricating oil at 3s. per gallon, and a speed of 10·45 knots).

(*b*) The engine-room complement is:—

1 Chief.
3 Watch engineers.
1 Stand-by engineer.
6 Greaser class.
1 Storeman.

12 Total.

The engine-room complement is a matter for the decision of the owners' superintendent. The number stated above may be looked upon as a maximum, and could be substantially reduced depending upon the point of view.

8. The arrangement of machinery shows that the length of engine room claimed is 31 feet 10½ inches, while the length shown on the standard drawing is 48 feet 10½ inches.

The volume of the machinery space claimed is 31,800 cubic feet less than that shown on the standard drawing.

It is further noted that the capacity of the machinery space claimed is 17,300 cubic feet less than that based upon the stated minimum length of engine room (40 feet 4½ inches) to give the 32 per cent. deduction. This result shows that the agitation in certain quarters for a revision of the tonnage rules is not without reason.

Vickers Internal-Combustion Engine.

By Vickers, Ltd.

As alternative arrangements have not been invited, the proposal submitted is one in which a compromise between a low first cost and economy of running has been sought. No attempt has been made, however, to cut down the installation below the margin indicated by sea experience.

The nature of the installation and the disposition of the important items are indicated on the accompanying machinery arrangement (Plate VI.). It will be observed that the total engine room length is 42 feet 6 inches, this affording ample space for all the plant required.

The main engine has been primarily designed to give continuous running at a sea speed of $10\frac{1}{2}$ knots without fear of excessive wear or replacements. Attention is drawn to the fact that the mean indicated pressure in these conditions is no more than 96 lb. per square inch, the mechanical efficiency of the engine being taken at 78 per cent., as judged by dynamometer tests of similar engines.

The four-hours trial at $10\frac{3}{4}$ knots is easily within the capacity of the engine.

The main engine has six four-stroke cylinders, 28 inches diameter by 45 inches piston-stroke, and will develop 1,760 brake-horsepower at 112 revolutions per minute, giving the sea speed of $10\frac{1}{2}$ knots.

It is of the solid-injection type, no injection air compressors being required, thus greatly simplifying the engine and reducing the cost of upkeep. The saving of weight affords an opportunity of increasing the rigidity of the engine and allowing of ample bearing surfaces.

The aim has been to produce an engine of robust proportions throughout, easily manufactured without the use of special tools and having all parts readily accessible for examination or replacement. No attempt has been made to reduce weight or space occupied at the expense of these features.

The bedplate is flat-bottomed and directly attachable to tank top, so saving expense to shipbuilders for seatings.

Forced lubrication is used throughout.

Salt water is used for the cooling of the jackets, covers and pistons, the circulating pumps as well as those for the lubricating oil being driven by the main engine.

These pumps are of slow-speed type to ensure durability.

A diaphragm is fitted between the crank case and the cylinders to prevent any possible contamination of the bearing oil, thus economizing lubricating oil.

The exhaust valves in the cylinder covers are water-cooled. This type of valve has proved very successful in the "Vickers" engine, and it has been recorded that during 18 months' running of a twin-screw installation comprising 16 cylinders, only one exhaust valve was opened up, and this only to ease the spindle.

Judging from trials carried out in Vickers-built motor vessels, the exhaust gases from the engine would, if passed through the boiler, generate sufficient steam for all auxiliary purposes at sea; but as there is insufficient margin to cover slow-speed running conditions, the dynamo is driven by a semi-Diesel engine.

The boiler is designed to cover the maximum requirements of steam-driven deck auxiliaries and for either exhaust-gas or oil firing, the furnace fronts being arranged for rapid conversion.

For extensive manœuvring a steam-driven compressor is fitted, a semi-Diesel-driven auxiliary being provided for emergency or sea use. Generous compressed-air storage capacity is provided.

The oil engines for the dynamo and auxiliary air compressor are arranged in line so that there is a spare engine for the dynamo available at short notice.

The propeller is right handed and of the built-up type. The boss is of cast steel and the blades of manganese bronze. It is considered that the superior efficiency of this type of propeller would soon repay the extra cost over one of cast iron.

The thrust block is of the "Michell" patent single collar type.

The shafting generally, in common with all other parts of the installation, is proportioned to conform to the requirements of Lloyd's Register, and the weights given are calculated on that basis.

The fuel consumption for the main engine is estimated at 0·42 lb. per brake-horsepower per hour, and for all purposes at 0·43 lb. per brake-horsepower per hour. The calorific value assumed is in the neighbourhood of 18,000 B.T.U.'s. The engine could be adjusted, if desired, to function on the more difficult fuels, but such are not recommended, as entailing greater mechanical wear on any engine and greater upkeep. Fuels of Texas quality, light Mexican, and similar fuels not containing large quantities of impurity—especially hard asphalt—are eminently suitable when Diesel fuel is not available.

The weights of the machinery proposed are given as an appendix.

The cost of running per ton-mile, on a deadweight tonnage basis of 6,540 tons, using Fuel oil and lubricating oil as specified by the Institution, would be 0·00437 pence.

Approximate Machinery Weights.

	Tons.
Main engine complete with engine driven pumps, flywheel, electrically driven turning gear, all piping and connections attached, also holding down bolts and chocks	270·0
Air reservoirs, exhaust silencer, lubricating oil settling tank, cooler, and strainers, fuel oil ready-use tank mountings and strainers	20·5
Air compressors and oil driven dynamo	13·5
Shafting complete with all accessories	48·0
Ladders and gratings, floor plates and bearers, lifting gear, sundry bolts, nuts, joints, clips, hangers, engine room ventilators, telegraph gear, spanners, special tools and outfit	28·0

	Tons.
Piping and connections clear of engine, including strainers and sundry fittings	27·5
One single ended cylindrical boiler 10 feet diameter by 11 feet long, with steam driven auxiliaries for ballast, general service, fuel oil transfer, etc., and with uptakes, funnel and steam and exhaust pipes in machinery space	32·5
Water in engine, boiler and pipes affected	20·0
Total	460·0

The Werkspoor Engine.

By R. & W. Hawthorn, Leslie & Co., Ltd.

The following particulars are given regarding a machinery installation embodying the Werkspoor heavy marine oil engine suitable for propelling the Review ship under the conditions laid down by the Institution.

The engine selected in conjunction with the Review ship is moderately rated, and in fact is able to maintain trial-trip conditions continuously at sea.

The engine is of the six-cylinder four-stroke single-acting type, having a cylinder diameter of 760 mm. and a stroke of 1,400 mm., together with a speed of revolutions of 95 at maximum output of 2,000 b.h.p. on brake. At a speed of revolutions of 95 per minute, and using a propeller in accordance with the "revolutions power curve" issued with the plans of the ship. the b.h.p. necessary is 1,848, when propelling the ship at 10¾ knots under trial-trip conditions of draft. The engine would develop this power with a mean indicated pressure of 90 lb. per square inch, and would, therefore, be running under very safe conditions. The auxiliary machinery is electrically driven, consisting of three Diesel generating sets of 88 kw. each and one semi-Diesel generating set of 10 kw. One electrically operated auxiliary compressor is fitted, equal in capacity to the air compressor driven off the main engine. At sea one Diesel generating set will be sufficient, whilst, when manœuvring, two sets will be required for providing starting air. In port the maximum load will require two sets in operation, thus leaving at all times one stand-by set available in case of emergency.

A small Cochran boiler of about 80 square feet of heating surface is fitted for accommodation heating and also for the heating of oil fuel in the double bottoms and in the daily service tanks when required.

The following answers are given to the questions selected by the Institution :—

1.—The engine will develop 1,848 b.h.p. when the ship is travelling at 10¾ knots during a four-hours' trial.

2.—The percentage of the trial b.h.p. which can be maintained by the proposed engine on a 3,000-mile run under ordinary sea

conditions is 100 per cent., in as much as the engine is capable of developing 2,000 b.h.p. if required.

3.—About 73 per cent. is the mechanical efficiency of the engine, which drives its own air compressor, bilge and sanitary pumps, together with the high-pressure fuel pumps.

4.—The fuel consumption of the main engine per b.h.p. when the ship is travelling at the designed speed is 0·41 lb. per b.h.p. per hour, or 8·1 tons of oil fuel per 24 hours. The total consumption for all purposes, under the same conditions, is 0·45 lb. per b.h.p. per hour, or 8·9 tons of oil fuel per 24 hours.

5.—The brand of fuel assumed is that regularly supplied for marine oil engines, and has the following specification, viz. :—Specific gravity, 0·86 to 0·93 at 60° F. calorific value 17,800 B.T.U. per lb. low value. Flash point 160° F. (Abel Pensky close test). Viscosity 155 seconds Redwood at 120° F. Sulphur content and ash content 1 per cent. maximum by weight respectively. Oils conforming to the above specification are suitable in every way for continuous running.

The grades of boiler oils which have been used recently for Diesel engines can be burnt successfully, and other less refined boiler oils can be also used, but due to their ash and hard asphaltum content, together with earthy matter, are not to be recommended on account of the abrasive and clogging action which takes place within the cylinder, together with pitting of the exhaust valves, all of which make for more frequent and more expensive repairs, greatly reducing any economy that might have been anticipated by using the cheaper grades of oil fuel.

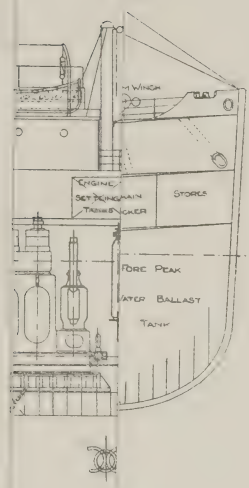
6.—The weight of the main engine, including intermediate and propeller shafting, and propeller, is about 350 tons. The total weight of machinery, which includes generating sets, auxiliary pumps, pipes, valves, platforms, gratings, silencers, spares, etc., is, approximately, 550 tons.

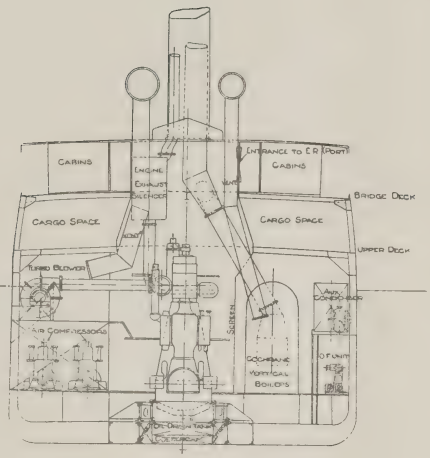
7.—The length of engine room required in 46 feet 9 inches. The saving in cargo space is therefore 2,600 cubic feet over that given in the plan of the Review ship. A small thrust recess has been added to the original engine-room.

8.—The cost of fuel and lubricating oil per ton-mile is based on an average price of 70s. per ton, and 3s. per gallon respectively. With a speed of $10\frac{3}{4}$ knots at full load, the distance travelled per day is 258 nautical miles, the estimated fuel consumption being 8·9 tons per day and the estimated lubricating-oil consumption 12 gallons per day. For comparative purposes, the deadweight carried is 6,450 tons, corrected for machinery weight; therefore the fuel and lubricating-oil cost per ton-mile is 0·00476 pence. Using boiler oil at 60s. per ton the cost per ton-mile is 0·00428 pence.

The number of the engine-room staff is eleven, including four engineers, one electrician, and six assistants.

Types of





SECTION THRO' ENGINE ROOM
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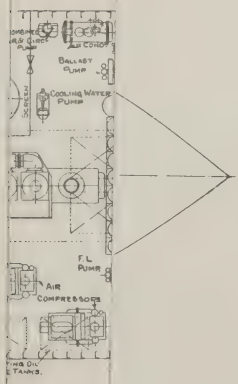
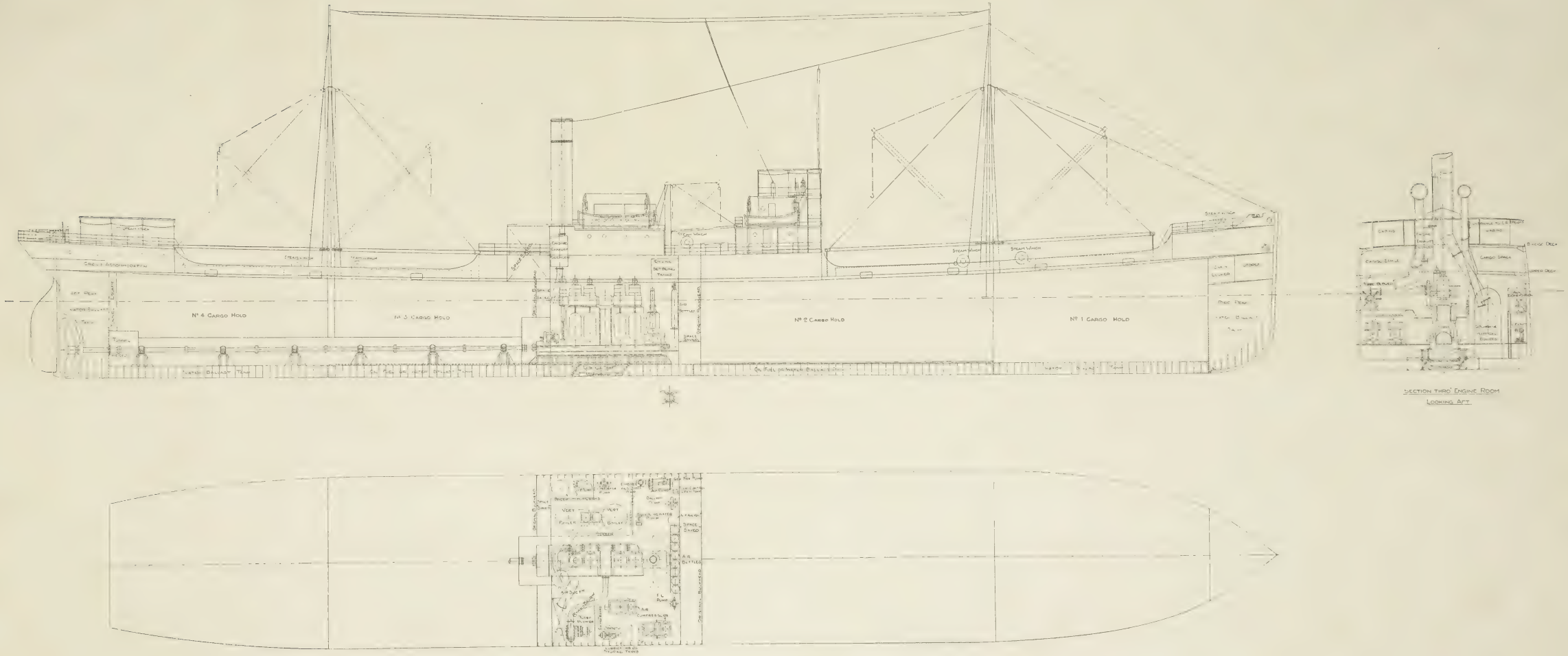


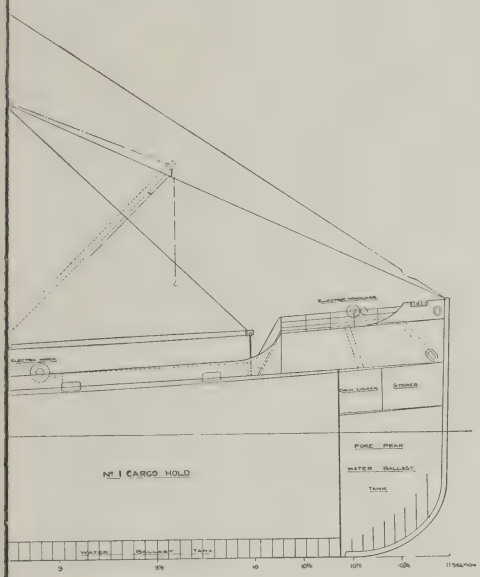
DRIVEN

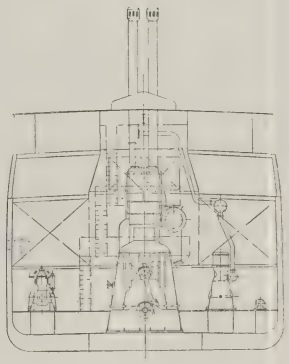
Illustrating "Review of Different Types of Marine Internal-Combustion Engines." (The Armstrong-Sulzer Engine).



PROPOSAL "A." DIESEL DRIVEN CARGO VESSEL. ARMSTRONG-SULZER ENGINES AND STEAM DRIVEN AUXILIARIES.

e Camellaird-Fullagar Engine).





PRINCIPAL DIMENSIONS :—

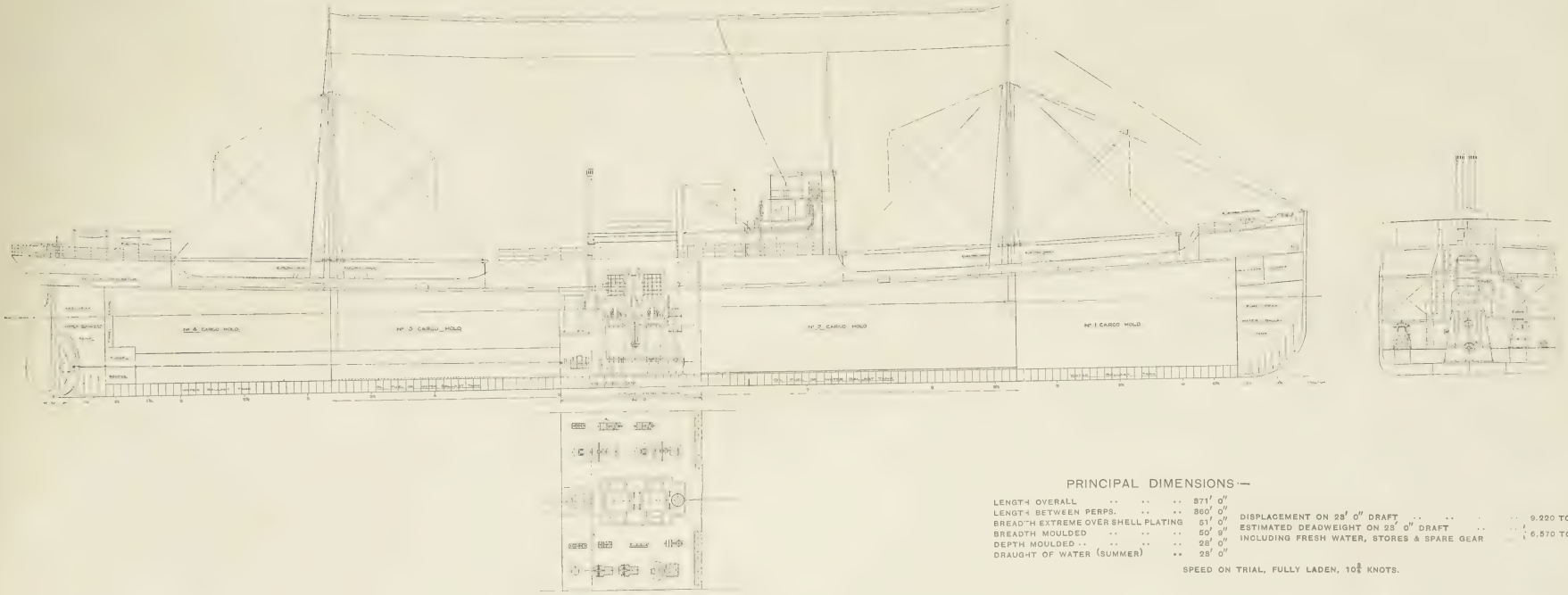
H OVERALL	371′ 0″
H BETWEEN PERPS.	360′ 0″
TH EXTREME OVER SHELL PLATING	51′ 0″
TH MOULDED	50′ 9″
MOULDED	28′ 0″
HT OF WATER (SUMMER) ..	23′ 0″

DISPLACEMENT ON 23′ 0″ DRAFT	9.220 TONS
ESTIMATED DEADWEIGHT ON 23′ 0″ DRAFT	6,570 TONS
INCLUDING FRESH WATER, STORES & SPARE GEAR ..	

SPEED ON TRIAL, FULLY LADEN, 10¾ KNOTS.

NE PROFILE.

Illustrating "Review of Different Types of Marine Internal-Combustion Engines." (The Camellaird-Fullagar Engine).



PRINCIPAL DIMENSIONS:—

LENGTH OVERALL	371′ 0″
LENGTH BETWEEN PERPS.	360′ 0″
BREADTH EXTREME OVER SHELL PLATING	51′ 0″
BREADTH MOULDED	50′ 9″
DEPTH MOULDED	28′ 0″
DRAUGHT OF WATER (SUMMER) ..	25′ 0″

DISPLACEMENT ON 28′ 0″ DRAFT	9,220 TONS
ESTIMATED DEADWEIGHT ON 25′ 0″ DRAFT .. .. INCLUDING FRESH WATER, STORES & SPARE GEAR	6,570 TONS

SPEED ON TRIAL, FULLY LADEN, $10\frac{3}{4}$ KNOTS.

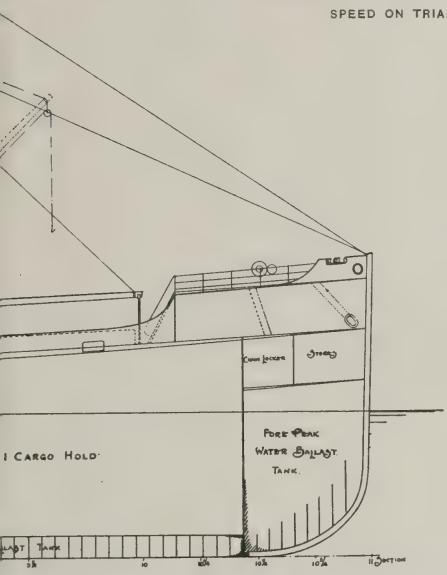
DESIGN FOR A SINGLE SCREW MOTOR DRIVEN CARGO VESSEL. OUTLINE PROFILE.

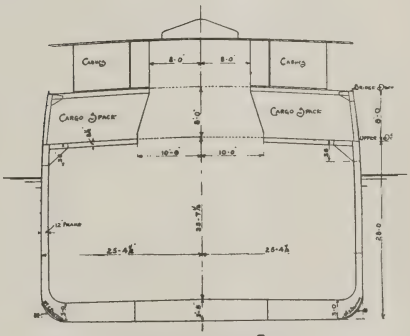
ed Piston Engine).

PRINCIPAL DIMENSIONS:—

LENGTH OVERALL	371′ 0″	DISPLACEMENT ON 23′ 0″ DRAFT	9,220 TONS
LENGTH BETWEEN PERPS.	360′ 0″	ESTIMATED DEADWEIGHT ON 23′ 0″ DRAFT .. INCLUDING FRESH WATER, STORES & SPARE GEAR	6,570 TONS
BREADTH EXTREME OVER SHELL PLATING	51′ 0″		
BREADTH MOULDED	50′ 9″	NOTE—430 TONS HAS BEEN ALLOWED FOR IN THE ABOVE FIGURE FOR WEIGHT OF PROPELLING MACHINERY.	
DEPTH MOULDED..	28′ 0″		
DRAUGHT OF WATER (SUMMER) ..	23′ 0″		

SPEED ON TRIAL, FULLY LADEN, 10¾ KNOTS.





Section thro Engine Room

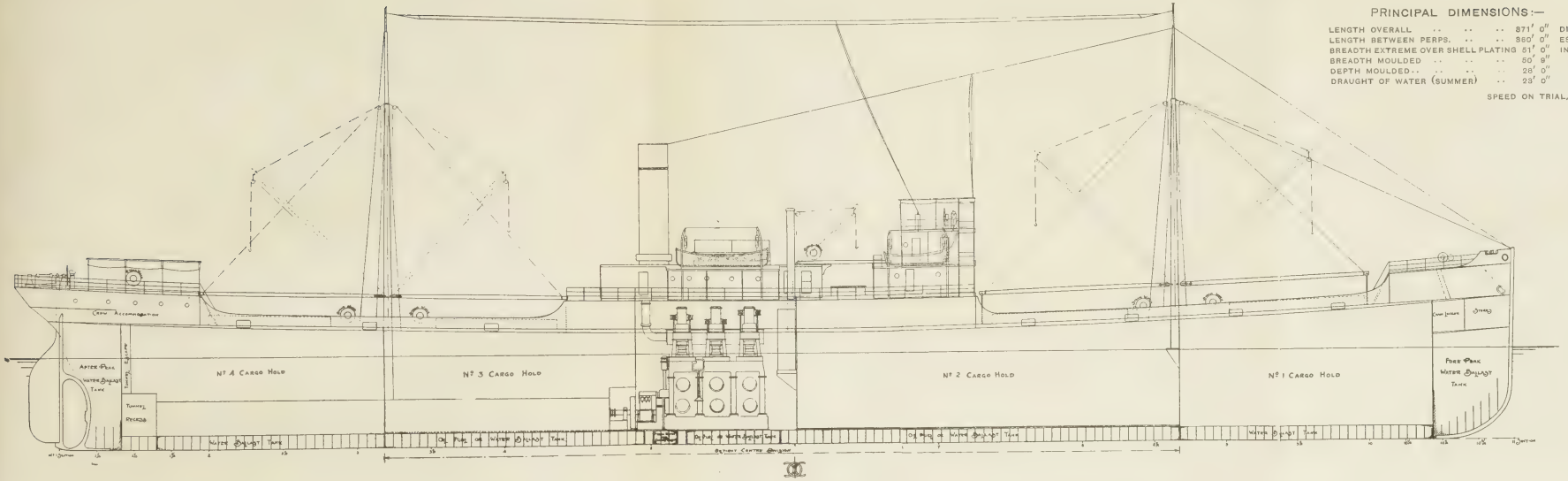
Illustrating "Review of Different Types of Marine Internal-Combustion Engines." (Doxford Opposed Piston Engine).

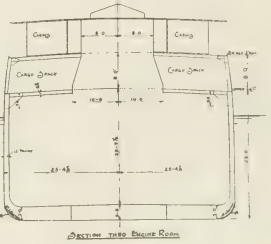
PRINCIPAL DIMENSIONS:—

LENGTH OVERALL	371′ 0″	DISPLACEMENT ON 23′ 0″ DRAFT	9,220 TONS
LENGTH BETWEEN PERPS.	360′ 0″	ESTIMATED DEADWEIGHT ON 23′ 0″ DRAFT INCLUDING FRESH WATER, STORES & SPARE GEAR	6,570 TONS
BREADTH EXTREME OVER SHELL PLATING	51′ 0″		
BREADTH MOULDED	50′ 9″		
DEPTH MOULDED	28′ 0″		
DRAUGHT OF WATER (SUMMER)	23′ 0″		

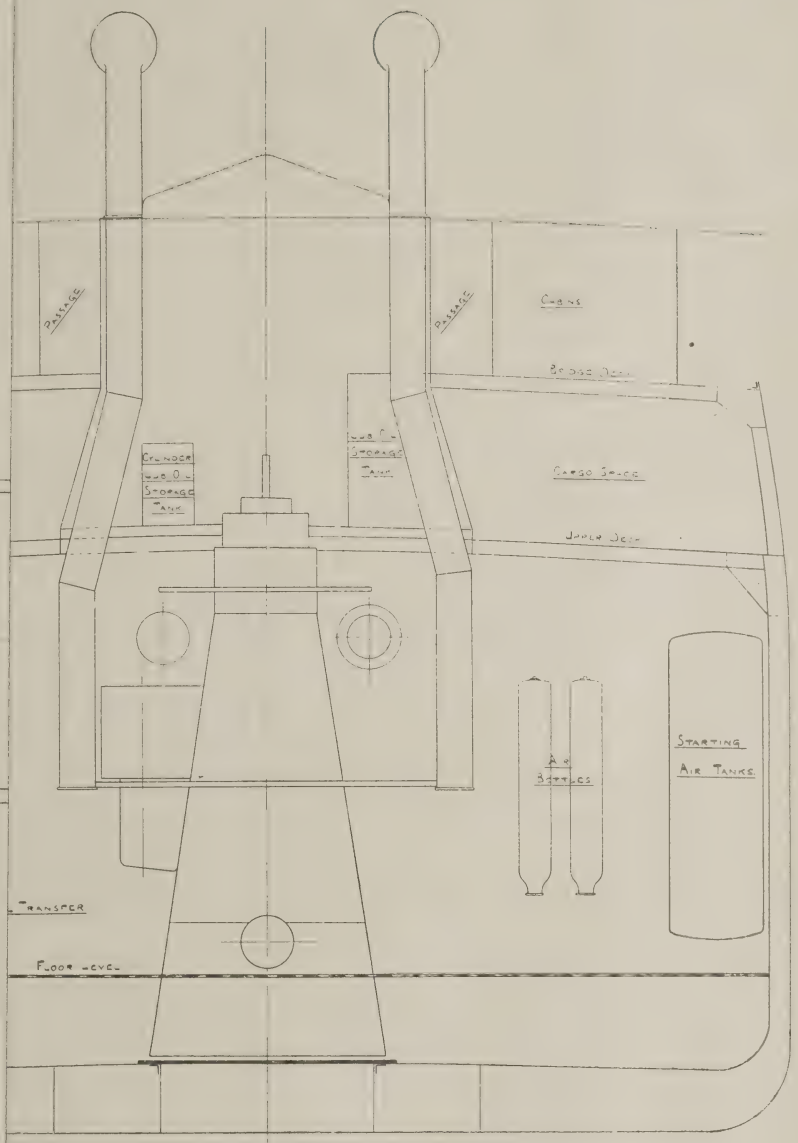
NOTE—430 TONS HAS BEEN ALLOWED FOR IN THE ABOVE FIGURE FOR WEIGHT OF PROPELLING MACHINERY.

SPEED ON TRIAL, FULLY LADEN, 10¾ KNOTS.



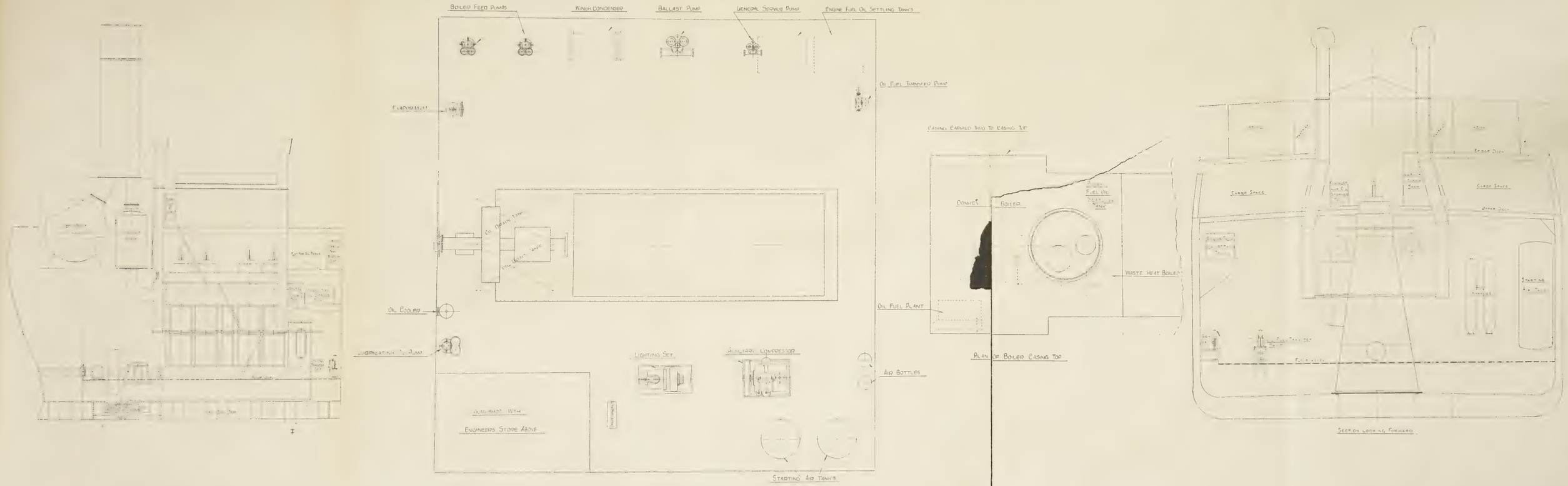


DESIGN FOR A SINGLE SCREW MOTOR DRIVEN CARGO VESSEL. OUTLINE PROFILE.



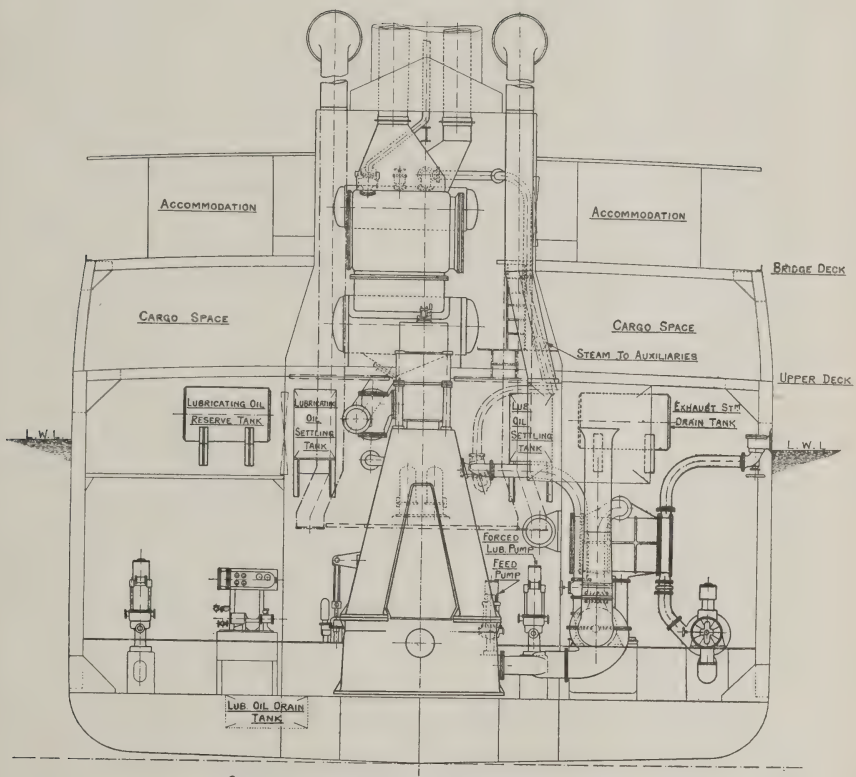
SECTION LOOKING FORWARD

Illustrating "Review of Marine Internal-Combustion Engines." (Swan, Hunter, & Wigham Richardson, Ltd. "Neptune" Engine).



PROPOSED ARRANGEMENT OF PROPELLING MACHINERY FOR 371 FT. MOTOR DRIVEN CARGO VESSEL

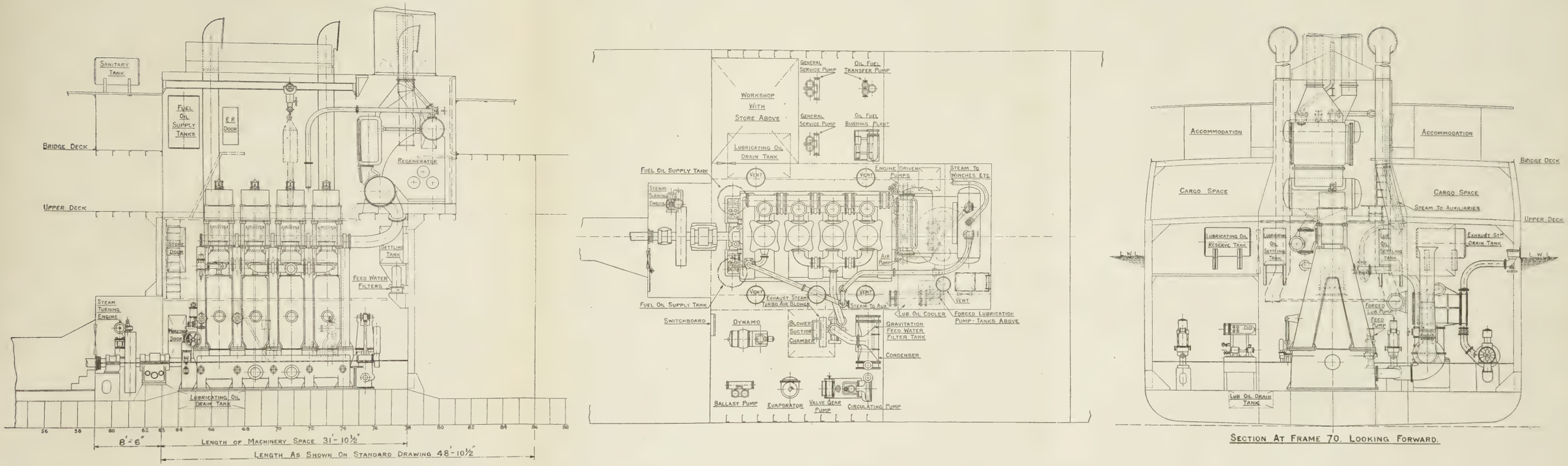
l Marine Oil Engine).



SECTION AT FRAME 70. LOOKING FORWARD.

ES.

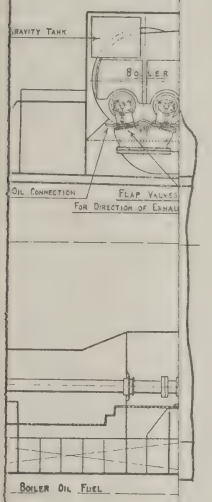
Illustrating "Review of Different Types of Marine Internal-Combustion Engines." (The Scott-Still Marine Oil Engine).

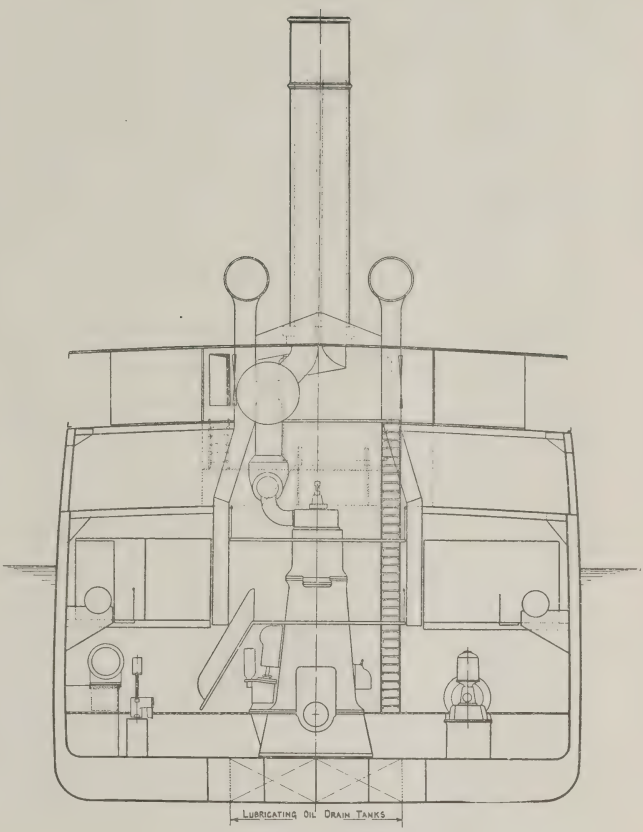


Section At Frame 70. Looking Forward.

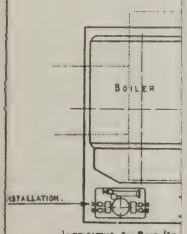
SINGLE SCREW CARGO VESSEL. ARRANGEMENT OF MACHINERY WITH SCOTT-STILL OIL ENGINES.

ne Internal-





SECTION THRO' ENGINE ROOM.
LOOKING AFT.



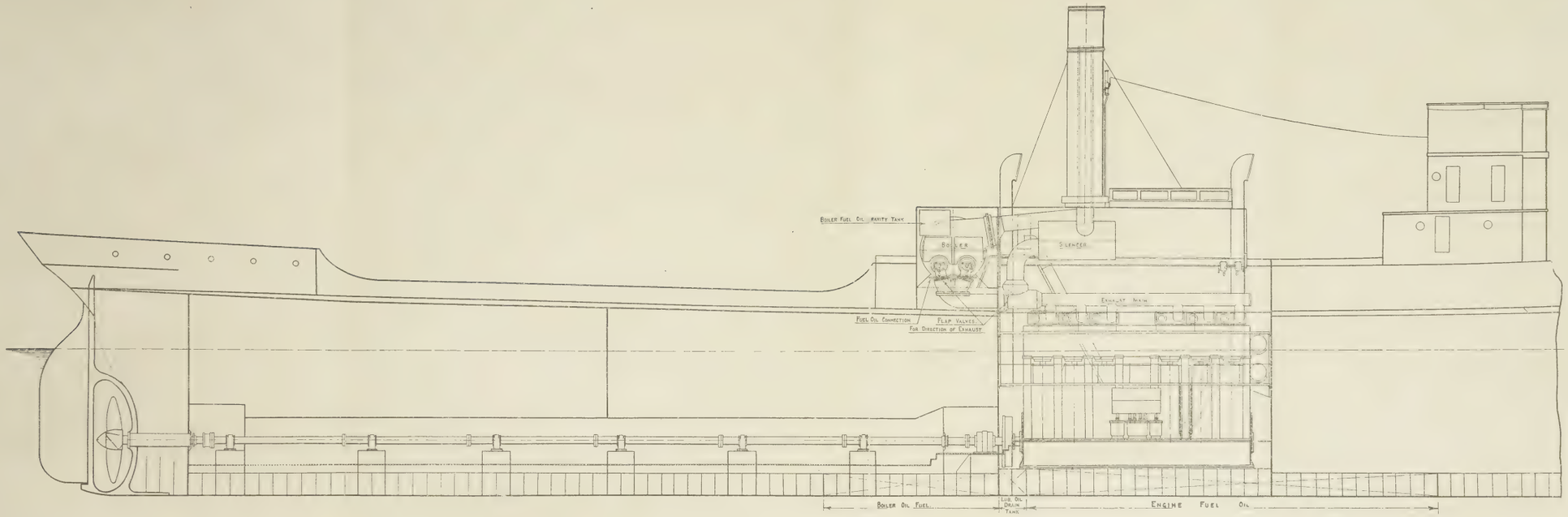
LUBRICATING OIL PUMP (STA
STEAM - DRIVEN.

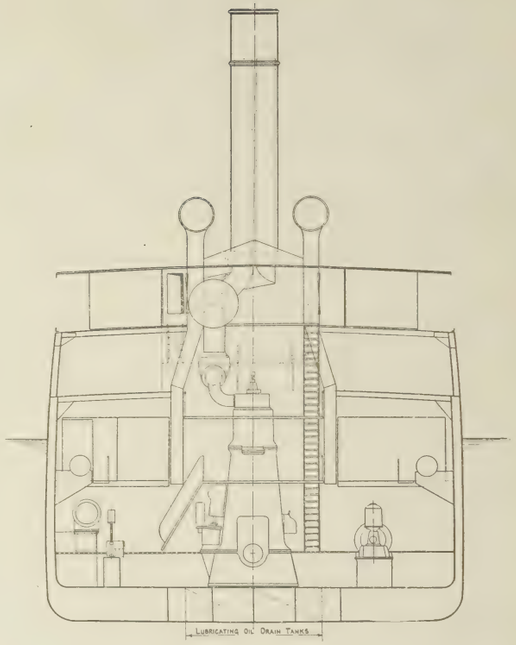
LUBRICATING OIL COOLER.

FUEL OIL TRANSFER PUMP
STEAM - DRIVEN.

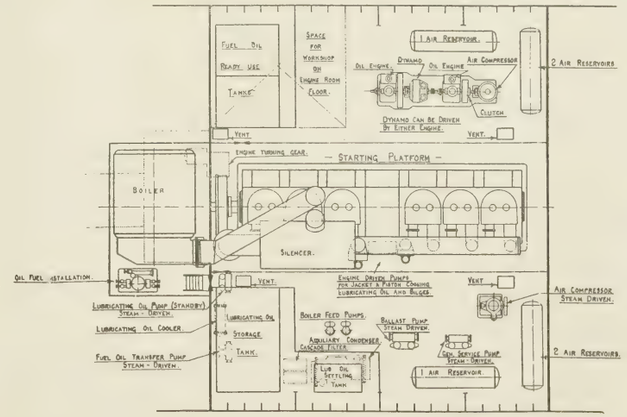
B.H.P. FOR A S

Illustrating "Review of Different Types of Marine Internal-Combustion Engines." (The Vickers Internal-Combustion Engine).





Section Thro' Engine Room.
Looking Aft.



Principal Dimensions.

Length Overall	371'-0"
Length Between Perps	360'-0"
Breadth, Extreme Over Shell Plating	51'-0"
Breadth Moulded	50'-9"
Depth Moulded	28'-0"
Draught of Water (Summer)	23'-0"

Displacement on 23'-0" Draft	9220 Tons
Estimated Deadweight on 23'-0" Draft Including Fresh Water, Stores & Spare Gear	6540 Tons

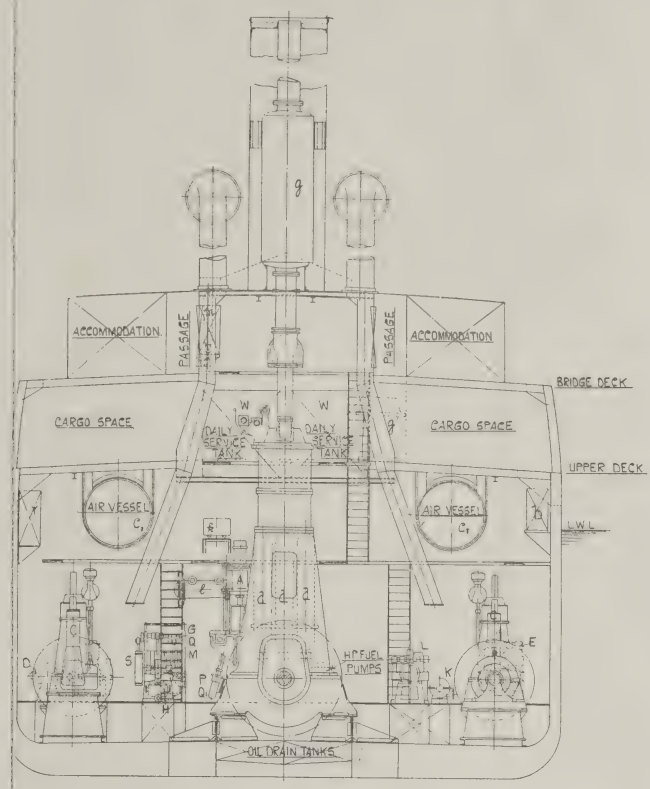
Note:- 460 Tons has been allowed for in above figures for weight of propelling machinery.

Length of Machinery Space is 3 Frame-Spaces less than on Guidance Plan, representing a gain of approx. 6,000 Cub. Ft. of Cargo Space.

Speed on Trial - Fully Laden 10¾ Knots.

ARRANGEMENT OF MACHINERY — 1890 B.H.P. FOR A SINGLE SCREW MOTOR DRIVEN CARGO VESSEL. VICKERS, LTD

ion Engines." (The Hawthorn-Werkspoor Engine).



LOOKING FORWARD

LIST OF AUXILIARY MACHINERY ETC.			
MARK	Nº OFF	DESCRIPTION	SIZE
A	ONE	MAIN ENGINE AIR COMPRESSOR	450 CU.FT. PER MIN.
B	ONE	AUX. AIR COMPRESSOR (MOTOR DRIVE)	450 " " "
C	3	MAIN GENERATING SETS	88 KWs. EACH
D	ONE	HOT BULB GENERATING SET	10 KWs.
E	ONE	BALLAST PUMP (PLUNGER)	150 TONS/HOUR
F	ONE	LUBRICATING PUMP (PLUNGER)	30 TONS/HOUR
G	ONE	" " (SPARE)	30 " "
H	ONE	DAILY SERVICE O.F. PUMP (PLUNGER)	9 " "
J	ONE	H.P. O.F. PUMP (DRIVEN OFF TURNING MOTOR)	9 " "
K	ONE	CYLINDER COOLING PUMP (CENTRIFᵍ)	150 " "
L	ONE	PISTON COOLING PUMP (PLUNGER)	60 " "
M	ONE	" " " (SPARE)	60 " "
N	ONE	PISTON COOLING DISCᵃ & BILGE PUMP	100 " "
P	ONE	SANITARY PUMP (DRIVEN OFF MAIN ENGᵉ)	35 " "
Q	ONE	BILGE PUMP (SPARE)	35 " "
Q1	ONE	BILGE PUMP (DRIVEN OFF MAIN ENGᵉ)	35 " "
R	3	AIR COMPRESSORS ON MAIN GENᵉ SETS	——
S	ONE	AIR COMPRESSOR (EMERGENCY)	50 CU.FT. PER MIN.
T	ONE	COCHRANE BOILER	80 ⌀ H.S.

LIST OF AUXILIARY MACHINERY ETC.			
MARK	Nº OFF	DESCRIPTION	SIZE
U	ONE	FEED PUMPS	——
V	ONE	DRAIN TANK FOR HEATING SYSTEM	130 GALLS.
W	2	DAILY SERVICE OIL FUEL TANKS	1250 GALLS EACH
X	ONE	PISTON COOLING WATER TANK	380 GALLS.
Y	ONE	LUBRICATING OIL SETTLING TANK	250 "
Z	ONE	" " RESERVE TANK	400 "
a	ONE	" " GRAVITY TANK	250 "
b	6	TANKS FOR CYL. OIL PARAFFIN ETC	80 GALLS. EACH
C_1	4	STARTING AIR RECEIVERS	450 CU.FT. EACH
d	3	RESERVE BLAST AIR BOTTLES	21 CU.FT. EACH
e	ONE	MAIN BLAST AIR BOTTLE	16 CU.FT.
f	2	AUXILIARY BLAST AIR BOTTLES	3.7 CU.FT. EACH
g	ONE	MAIN SILENCER	226 CU.FT.
h	ONE	AUX. SILENCER	30 CU.FT.
i	ONE	SPARE MAIN ENGINE CYLINDER	——
k	ONE	" " " PISTON	——
l	ONE	LUBRICATING OIL COOLER	100 ⌀ C.S.
m	ONE	FLOATING O.F. VESSEL	——

PARTICULARS OF VESSEL

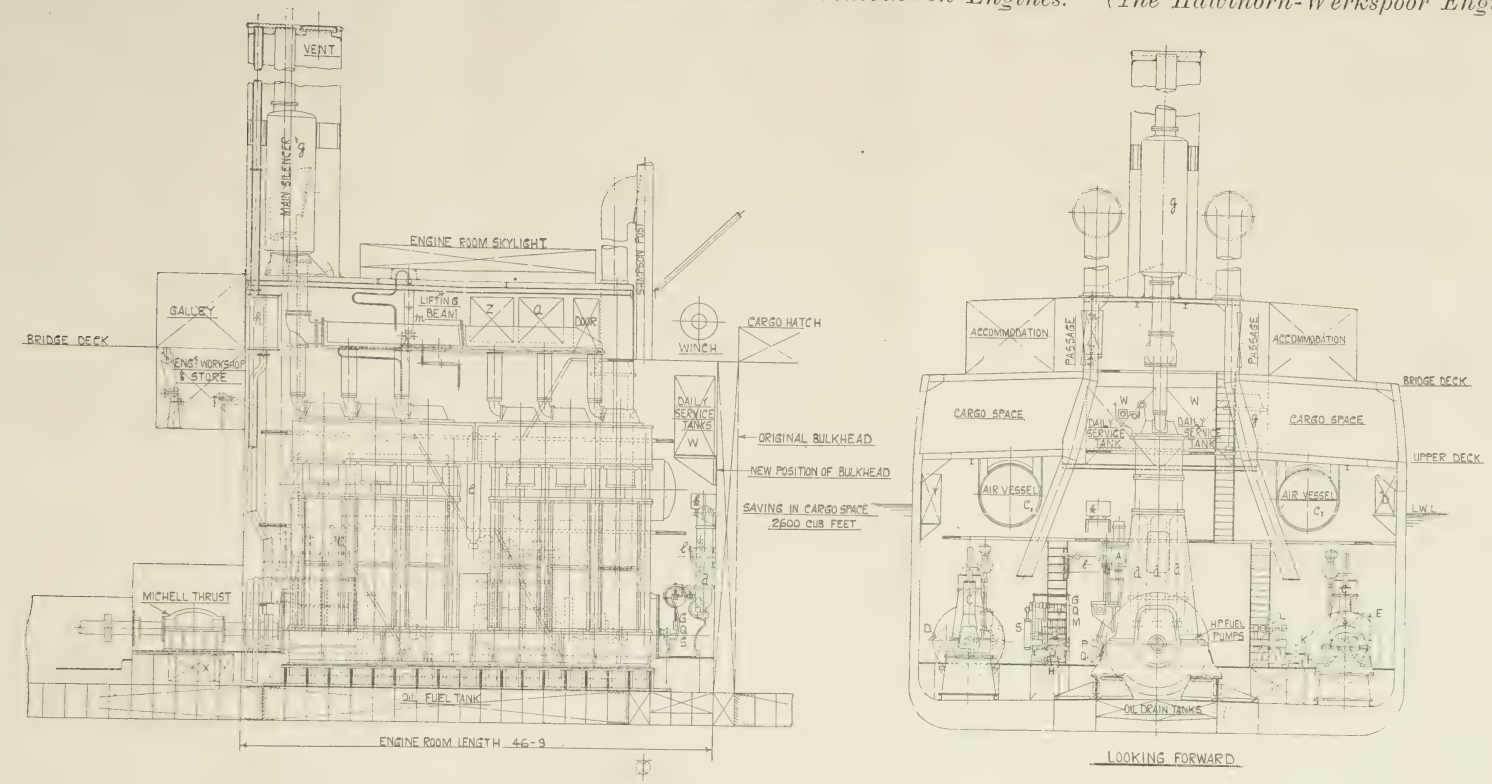
LENGTH BETWEEN PERPENDICULARS ------- 360'—0"
BREADTH MOULDED ---------- 50—9
DEPTH MOULDED ---------- 28'—0"
DISPLACEMENT ---------- 9220 TONS
DRAUGHT FULLY LADEN ---------- 23'—0"
DEADWEIGHT INCLUDING FRESHWATER STORES & SPARE GEAR } 6450 TONS
SPEED ON TRIAL FULLY LADEN --------- 10¾ KNOTS

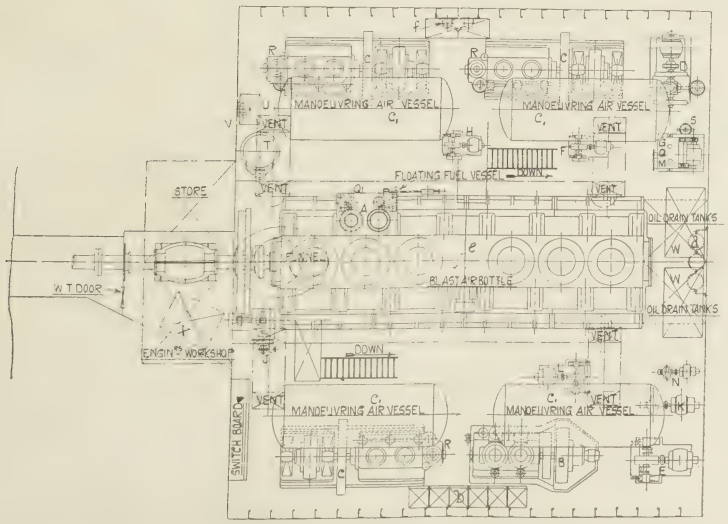
PARTICULARS OF ENGINE

TYPE - WERKSPOOR FOUR CYCLE SINGLE ACTING
No. OF CYLINDERS ---------- SIX
DIAMETER OF CYLINDERS ---------- 760 m/m
STROKE OF ENGINE ---------- 1400 m/m
REVOLUTIONS ON TRIAL ---------- 95
POWER ON TRIAL ---------- 1848 B.H.P.
POWER (MAXIMUM) ---------- 2000 B.H.P.
WT. OF ENGINE WITH SHAFTING & PROPELLER 350 TONS
TOTAL WEIGHT OF MACHINERY 550 TONS (APPROX.)

SINGLE SCREW MOTOR DRIVEN CARGO VESSEL.

Illustrating "Review of Different Types of Marine Internal-Combustion Engines." (The Hawthorn-Werkspoor Engine).





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PARTICULARS OF VESSEL

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No. OF CYLINDERS	SIX
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STROKE OF ENGINE	1400 M/M
REVOLUTIONS ON TRIAL	95
POWER ON TRIAL	1848 B.H.P.
POWER (MAXIMUM)	2000 B.H.P.
WT OF ENGINE WITH SHAFTING & PROPELLER	350 TONS
TOTAL WEIGHT OF MACHINERY	550 TONS (APPROX)

CLASS NO. 718B.

ARRANGEMENT OF MACHINERY WITH ELECTRIC AUXILIARIES FOR SINGLE SCREW MOTOR DRIVEN CARGO VESSEL.

DISCUSSION (*12th January, 1923*).

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E., introducing the representatives of the Companies contributing Papers to the Review: I think you will agree with me that in some respects this may be regarded as a red-letter day in the history of the Institution. For some considerable time by a series of papers on the developments of the marine internal-combustion engine we have kept abreast of the times; we have also not so very long ago had comparative papers on the individual merits of the various forms of propulsion, and we are now to have a comparison of the suggested installations of various types of internal-combustion engines, both four-stroke and two-stroke, to meet the requirements of a specification which has been agreed upon at our Reading Committee's suggestion by the Council of the Institution. It should be understood that in the early stages of development of any engine or motive power—and this is especially applicable to internal-combustion engines for marine purposes—manufacturers have neither the designs nor the patterns suitable to meet any and every requirement of power and speed, and, therefore, whilst I think in all cases the gentlemen who have been good enough on behalf of their firms to read papers to-night have met the requirements and put forward engines which will give the necessary power and speed, they have quite possibly in some instances chosen the nearest approximate existing design. One can imagine certain shipowners eagerly searching through the *Transactions* to read the relative prices of the types which are being put forward to-night, but obviously this would be quite out of place for such a technical institution. I hope that every engine that is described to-night will get its fair trial at the hands of the shipowners, and that the papers will evoke a discussion worthy of our Institution and worthy of the very important subject that is to come under discussion to-night and in a fortnight's time.

Naturally, the question arose as to the order in which the papers should be read, and to avoid acting partially at all I have decided that they shall be read in alphabetical order of the names of the engines, not necessarily of the builders, and, therefore, they will be read in the following sequence: The Armstrong-Sulzer by Mr. T. H. Matthews, the Camellaird-Fullagar by Mr. D. M. Shannon, the Doxford opposed-piston by Mr. K. O. Keller, the Neptune marine oil engine by Mr. F. Rowntree, the Scott-Still engine by Mr. C. R. Bruce, the Vickers by Mr. D. M. Meikle, and the Werkspoor (Hawthorn Leslie) by Mr. A. D. Bruce.

The President then read the Institution specification and stated the General Dimensions of the ship there specified (p. 300).

Mr. Keller

MR. K. O. KELLER, after reading the Paper on the Doxford opposed-piston engine suggested, with the President's permission, that the contributors of papers to the Review should state more definitely what their crank-shafts were made of and also state the bearing pressures of their engines on bottom-end top knots and main bearings.

Mr. Meikle

MR. D. M. MEIKLE, after reading the Paper on the Vickers engine said: In the design we have put forward the engine is shown with a flat bedplate bolted direct to the tank top. I think it would be interesting to have an expression of opinion as to the best form of seating for a Diesel engine. We ourselves have built engines both with flat-bottom bedplates bolted direct to the tank top and naval type bedplates carried on girders. If a flat-bottom type is used it would be interesting to have the opinion of shipbuilders as to any difficulty of construction, especially in connexion with carrying oil fuel or fresh water directly underneath the engine.

Engr.-Commdr. Hawkes

ENGR.-COMMDR. C. J. HAWKES, R.N. (ret.), *Member of Council*: In reading the Review one is struck by the fact that of the seven Papers five are concerned with engines of the two-stroke type. Except in the case of the Still engine there is not a great deal of difference in the machinery weights of the two-stroke and four-stroke proposals, although the two-stroke proposals are, as one would expect, somewhat lighter than the four-stroke—especially when one takes into account the proposed revolutions of the engines.

The fuel consumptions of the opposed-piston engines are lower than the figures quoted for other types, both two- and four-stroke, with the exception of the Scott-Still engine; and the fuel consumptions of the single-piston two-stroke engines are somewhat higher than the four-stroke engines. It is noticed that in the Vickers proposal the consumption per b.h.p. hour is 0·42 for the main engine and 0·43 for "all purposes," whereas the corresponding figures for the Hawthorn-Werkspoor proposal are 0·41 and 0·45 respectively—the auxiliaries in the former being steam driven and in the latter electrically driven. Mr. Meikle has made it clear that this difference is due largely to the use of steam generated by the heat of the exhaust gases in the Vickers proposal.

It is interesting to note that the consensus of opinion is against the general use of heavy boiler fuel-oil—especially that containing a high percentage of hard asphaltum.

The consumption of fuel per i.h.p. is, of course, only an approximate figure. It is noted, however, that the consumptions per i.h.p. are practically the same in all the solid-injection engines; but the consumption in the case of the Scott-Still engine is based on a mechanical efficiency of 88 per cent., and this takes into account the power developed by the steam side of the engine. The total mean pressure for the combustion and steam sides of the pistons is 90 lb. per square inch. How much of this is attributed to the steam? It will be seen that if we consider only the *combustion* i.h.p., *i.e.*, the i.h.p. due to the combustion of the oil on the combustion sides of the pistons, the fuel consumption per combustion i.h.p. is appreciably higher than in the Vickers or Doxford proposals. Is there any reason for this? Then again the *combustion* mean pressure is appreciably lower in the Still engine than in the other engines. It is probable that at the mean pressure adopted

in the Scott-Still proposal the lowest consumption per b.h.p. is obtained, but are there any other reasons for adopting such a low mean pressure? Engr.-Commdr. Hawkes

The proposed engine-room complement has not been given in all cases. It is difficult, as Mr. Bruce of Messrs. Scotts pointed out, to fix on the engine-room complement for any particular proposal. It is noted that where the information is given the proposed engine-room staffs differ appreciably, and should these figures be approximately correct it would have the effect of nullifying any advantages derived from a low fuel consumption.

There are a few points upon which further information would be appreciated :—

Armstrong-Sulzer engine.—Separate pumps are proposed for the lubrication of cross-head bearings. At what pressure is the oil supplied to these bearings, and is there any real need for the use of high pressure or for a separate lubricating system? It is stated that the engine will run satisfactorily on boiler fuel oils having a sulphur content not exceeding 3 per cent. It is realized that sulphur is undesirable, owing to its low calorific value, but how was the 3 per cent. limit determined? Is there any experience with oils containing more than 3 per cent. of sulphur and, if so, in what way was the sulphur found deleterious? Is the 15 per cent. of asphaltum quoted what is known as hard asphaltum or does it represent the total of hard and soft asphaltums?

Camellaird-Fullagar engine.—There is no doubt that opposed-piston engines run very satisfactorily at slow speed. I have seen similar results obtained with the Doxford engine. It is stated that from a performance of the four-cylinder, 25-inch-stroke, Camellaird-Fullagar engine it is evident that with longer strokes and six-cylinder engines lower revolutions can be obtained, if desired. It is presumed that this statement refers to engines of the same total power in four-cylinders. If not, what are considered to be the principal points which affect the slow-speed running of the engine? Also, was the 25-inch-stroke engine firing in all cylinders when running at 16·5 r.p.m.?

Doxford engine.—Spur and bevel wheels are proposed for driving the camshafts. What is the experience obtained with Doxford engines fitted with skew-gear drives?

Scott-Still engine.—Why is it necessary to draw water from the regenerator and pass it through a pressure filter in view of the provision for filtering the condensate before it enters the feed-water heater?

It is presumed that the fuel consumption quoted is intended to apply to the continuous sea speed and not to the trial speed. Does this figure include the fuel equivalent of the make-up feed and what is the anticipated loss of feed water per day in the proposal outlined?

Vickers engine.—The experience with exhaust valves is interesting. There is no doubt in my mind that it is an advantage to cool the exhaust valves in large marine engines, and in fast-running engines, on the lines adopted by Messrs. Vickers.

Engr.-Commdr. Hawkes

It is noticed that the revolutions of the proposed engine are appreciably higher than the other proposals except, perhaps, the Scott-Still. Was there any reason, apart from first cost, for adopting 112 r.p.m. at the continuous sea speed?

Werkspoor engine.—The maximum sulphur content of a fuel-oil is given as 1 per cent. How was this limit determined and what has been the experience with oils containing a higher sulphur content than that quoted?

VOTE OF THANKS.

COMMANDER HAWKES continuing: The Papers are representative of the various types of oil engines, and they are most interesting and valuable. The thanks of the Institution are due to the firms which have taken so much trouble in contributing to the Review. It gives me the greatest pleasure in proposing a very hearty vote of thanks to the gentlemen who have presented the various contributions this evening.

The vote was carried with great acclamation.

DISCUSSION (*26th January, 1923*).

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E., stated that, with the view of placing the figures given in the seven Papers forming the Review so far as possible on a common basis, a circular letter had been issued to the contributors since last meeting asking them to give their figures for weight of machinery and cost per ton-mile on the basis specified in the footnote to page 301 of the *Transactions*.

The Authors' figures on that basis were shown on the blackboard at the meeting, and also on schedules distributed among the audience.

The President further stated that the attempt made by means of the Review to create an interest in the different designs of marine internal-combustion engines had been more than justified by the extraordinary response which it had received.

Mr. Rennie

MR. ARCHIBALD RENNIE: There are many points of interest to be noted in the Papers, but I wish to refer to a few of them only, and these mostly in connexion with the Scott-Still proposal.

As a matter of general importance it is worthy of note, I think, that apart from all questions of type of engine and method of fuel injection adopted in the proposals before the Institution, all the makers prefer to burn Diesel-engine oil, although it is claimed that the engines will work satisfactorily with a boiler fuel oil if certain disadvantages, such as loss of efficiency, more frequent opening up for cleaning and overhaul, and greater wear and tear of the rubbing parts, are accepted by the owners. I think most engineers will endorse the makers' preference, and will agree that it is neither good policy nor good practice to use boiler fuel oil in place of the more refined Diesel oil when the latter can be obtained.

Mr Rennie

Another matter of general importance is the question of engine seating. This, it appears to me, should be much stronger than is required by the ordinary reciprocating steam engine. This question appears to have received some attention from Messrs. Scotts' and Messrs. Vickers, who propose to increase the depth of the ship's floors, and fit their engines directly on the tank top. Messrs. Armstrong Whitworth and Messrs. Hawthorn Leslie also seem to have considered the matter, and fit longitudinal girders on the ordinary depth of floor, on which to build their engines. The other builders apparently have not considered the question, or think that no special provision is necessary. The fitting of longitudinal girders without doubt makes a lighter job both for ship and machinery, though more expensive, and may be dictated in some cases by questions of engine design; but the deepening of the ship's floors is to my mind a sounder and more straightforward job even at the sacrifice of some weight.

Referring now to the Scott-Still proposal, it has been claimed for this engine that it gives a lower fuel consumption per b.h.p., has a lighter weight per unit of power and occupies less space in the ship than any other known type of heavy-oil engine. In the proposals before the Institution, where a specific case is dealt with, it will be seen that all these claims are fully substantiated, for it must be assumed that each maker has put forward what he considers to be the most suitable proposal to fulfil the specified conditions.

There appears to be at the present time a feeling among shipowners and others in favour of the retention of steam-driven auxiliaries even in ships driven by internal-combustion engines, and in this connexion I should like to call attention to the perfectly rational way in which the Scott-Still engine forms a natural alliance with steam-driven auxiliaries. In the first place, the Scott-Still engine in normal sea service produces within itself all the steam required by the auxiliaries and that without the expenditure of even an ounce of additional fuel. In the case before the Institution, one of the main-engine cylinders will function on its steam side as a high-pressure cylinder and will exhaust to the steam side of the other three cylinders which will function as low-pressure cylinders. About 5,000 lb. of steam at a pressure of 150 lb. per square inch, will be available per hour, the larger portion of which will be fed to those auxiliaries taking high-pressure steam, and will be exhausted by them into the low-pressure receiver of the main engine. The remainder will pass to the high-pressure cylinder for piston-cooling and power purposes, and will also be exhausted to the low-pressure receiver. After functioning in the low pressure cylinders and providing a cooling medium for the pistons, the whole of the steam is exhausted at about 5 to 6 lb. absolute pressure to the low-pressure turbine driving the scavenging-air blower, and from there to the condenser at about 28-inch vacuum. Such a cycle follows quite naturally on the production of all the steam required from the waste heat of combustion of the main engine itself.

Mr. Rennie

This leads me to ask Messrs. Vickers what quantity of steam they expect to generate from the waste heat in the exhaust gases, and what are the results of the trials on which their estimate is based. Might I put the same question to Messrs. Swan Hunter, who also rely to some extent on the generation of steam from the waste heat in the exhaust gases for working their auxiliaries, theirs being a two-stroke engine as against Messrs. Vickers' four-stroke engine. I am led to ask these questions because I believe that the quantity of steam that can be generated from the waste heat in the exhaust gases alone of an ordinary heavy-oil engine, is small indeed, and especially so in the case of a port-scavenging two-stroke engine.

A second point, which emphasizes the close alliance between the Scott-Still engine and steam-driven auxiliaries, is the natural provision it makes for driving the scavenging-air blower as an independent unit. In some cases the determination of this question, *i.e.*, the method of driving the blower, fixes the complexion of the whole auxiliary machinery installation, for if an electric drive is decided upon, the power required is usually so high that a little more is found to be sufficient for the needs of the whole ship both at sea and in port, and so, in spite of any desires he may have to the contrary, the designer is led to put forward a scheme for a complete electric outfit, with its attendant high cost and heavy weight. No such compulsion obtains in the Scott-Still-engined ship, for here the natural drive is a steam one, and the electric generator, so far as the blower is concerned, may be eliminated.

Messrs. Palmers and Messrs. Cammell Laird call attention to the minimum speed of revolution at which their engine will function and the certainty with which it will do so; and rightly so, for the question of flexibility which it raises is a very important one if only because of its influence on the manœuvring qualities of the ship. But the point loses all significance when referred to the Scott-Still engine, which in this respect also, is superior to all other known types of heavy-oil engines, for by reason of its steam cycle, its range of flexibility is greatly extended, and the revolutions are carried down practically to the limits obtained by the steam reciprocating engine.

When manœuvring, as mentioned in some of the papers, it is found necessary with most types of heavy-oil engines to start up an extra air compressor. In the Scott-Still-engined ship this situation is met by lighting up a burner in the regenerator which is always carrying a head of steam generated from waste heat—and I would put it to our seafaring friends if that is not a much easier and quicker operation to perform. And when speaking to these friends might I also mention for their special interest that the Scott-Still engine is not burdened with any oil- or water-cooling gears, either to pistons or exhaust valves, which are such a fruitful source of trouble in most heavy-oil engines. This point needs only to be mentioned, I think, to be appreciated.

The regenerator circulating pump on the Scott-Still engine is often enquired about. What is its function, and why is it there? Mr. C. R.

Bruce has dealt with its function in his paper, but opinions may differ as to the necessity for it. It represents, I may say quite frankly, a precautionary measure, and has been provided to deal with any excess of grease which by accident or otherwise might at some time find its way into the regenerator circuit. It has sometimes been urged against the Still system that the fuel oil is liable to find its way from the combustion side of the piston past the piston rings to the steam side, and so to contaminate the feed water. In two years of experimental running, no such contingency has arisen, but it has been thought best to retain the pump in the earlier installations on board ship, notwithstanding that it is a source of considerable heat loss due to radiation from the pipes and connexions. The pump, however, is small and its duty light—there being no more than 20-25 lb. difference in pressure between the suction and discharge sides—while every sea-going engineer understands how to work a hot-water pump. Mr. Rennie

In concluding my remarks I should like to add that it will be evident to anyone making a general survey of the proposals that if some at least of the heat which is sent to waste in the exhaust gases and cooling water of the ordinary heavy-oil engine were recovered and usefully applied, a substantial gain in consumption would result. At present it would appear that the Still system offers the most practicable method of accomplishing this, and I have no doubt that if this system were applied, even in part, to the more highly developed types of heavy-oil engines of the present day, they would at once take a much higher place in the economic scale of the world's prime movers than they at present do.

Mr. W. J. STILL: I feel a bit like a fish out of water because the actual details of ship engineering are things of which I have very little knowledge. I am an engine man and not a ship engineer. However, there are one or two points which I may refer to briefly. Mr. Rennie has spoken a good deal about the efficiency of the Still engine, and I think we have an efficiency which we may be proud of; but we all know in this business that the Still engine is a very juvenile engine compared with a great number of prime movers, and it is bound to take time to find out the best way to get efficiency. Messrs. Scott are doing their utmost to find out the best ways and to apply them, but as we all know experience is, after all, the final teacher. I have no doubt that we have still quite a lot to learn. I have obtained, on some trials of high-speed engines, consumptions down to practically 0·33 of a lb. per brake-horse-power, and have no doubt the bulk of our prime movers will in the future work at that figure; but it will take time. I want you to be patient while we are doing it. The next point is that I would like you to remember, we have not simply allied the steam engine and the combustion engine: we have really married them. The product is neither one nor the other; I hope it has all the good points and none of the bad points of both. That is my hope. You must not take the Still engine as being simply a combustion engine allied to the steam engine. The Mr. Still

Mr. Still

steam cycle has a real effect upon the combustion cycle, and that is where the bulk of our information has still to come from. We have to study the combustion cycle afresh, and find out those points by which we can get the highest combustion efficiency from the combustion side wedded to the highest efficiency on the steam side. That is the problem that I spend most of my time on to get the highest efficiency combined with reliability in all its different aspects, however, at present the main concern is to ensure reliability under all conditions of service.

Written Contribution.—It was suggested during discussion that the piston rings of two-stroke engines have, whilst heavily loaded, to pass over ports. The remark seemed to me to be misleading because the greater part of the loads the rings carry is due to the gas pressure behind them and this pressure is spilt when the ring reaches a port; only the initial tension then remains. This is but a very small part of the load carried by the rings during the remainder of the cycle and one they can easily support if port widths are reasonable.

Mr. Theunissen

Mr. T. THEUNISSEN (Werkspoor, Amsterdam): In view of the revolutions and mean pressures adopted by the Vickers proposal and others, it is necessary to reconsider these factors for the Hawthorn-Werkspoor proposal in order that a fair comparison may be made. The rating of this engine is a very moderate one, and should permit of the full power being developed after long periods of running without overhaul; but as 102 revolutions and 100 lb. per square inch will be accepted as a good value for a four-stroke engine on trial conditions a smaller engine could satisfactorily be substituted, having a cylinder diameter of 730 mm. and a stroke of 1,300 mm., developing 1,860 b.h.p. This engine would permit of a further saving of two frame spaces in engine-room length being equal to the Vickers proposal in this respect and having revolutions which are 13 less per minute. Also smaller auxiliaries could be used, say 65 kw. each, instead of 88 kw., and, as the Werkspoor air storage is very ample, one reservoir could easily be dispensed with, leaving a margin of air still in excess of that of most other makers of four-stroke machinery.

The total weight of machinery would be about 500 tons and if steam auxiliaries were used, as in the Vickers arrangement, a further saving of some 20 tons would be effected giving a machinery weight of about 480 tons.

It is thus seen that the engine of the Werkspoor proposal would not be heavier than the Vickers when the question of rating is given full consideration.

Mr. Souter

Mr. W. A. SOUTER, *Associate*: I am not going to presume to discuss this Paper from the technical point of view; all I would say is that I am one of those shipowners who would very much like to build a steamer with internal-combustion engines if I dared. Evidently a large number of shipowners dare not, judging by the orders that have been placed

recently. From the technical point of view all I would say is that I would much prefer, if I dare, to build an internal-combustion-engined vessel with steam auxiliaries rather than with electrically-driven auxiliaries. I think the use of the latter would introduce a considerable additional element of risk of breakdown. The other point is that it seems to me the use of the heat from the waste gases, either in the form of the Scott-Still engine or in the form of one of the other engines, where it is used to drive the auxiliaries from the donkey boiler, seems to be a very good idea from the economical point of view, and I think it is capable of development. Mr. Souter

From the commercial point of view I would like to say this: In various comparisons between the internal-combustion-engined vessel and that driven by reciprocating engines, I have always noticed in taking into account the economies obtainable from the internal-combustion engine that both sides of the question are not put. I suppose to-day that a ten-knot 8,000-ton steamer with steam engines would cost about £72,000. A vessel of the same deadweight, with the same speed, with internal-combustion engines would cost about £100,000—I am only giving the rough figures—that is roughly about 40 per cent. more. Since the shipowner has to consider this matter from the point of view of his capital, he must allow in the case of the steam engine 5 per cent. depreciation—that is the only thing these comparisons I have mentioned do allow. Five per cent. depreciation in the case of a steam engine which will last twenty-five years is all right, but is it all right in the case of the oil engine? That seems to me to be the unknown factor, and I do not think a shipowner is safe in reckoning on less than 7½ per cent. depreciation on an oil engine. Then you must allow 5 per cent. interest on your money—and I must say I have not found many shareholders content with that—but it is the usual amount laid down in these calculations. Then another point which most people seem to overlook is that you must allow 5 per cent. for insurance, and I think if a shipowner wants to be absolutely on the safe side he ought to allow 2½ per cent. for upkeep on an oil engine, so that you have 15 per cent. per annum in the case of steam engines on £72,000 and 20 per cent. in the case of an oil engine on £100,000, a very wide difference, which must be made good by increased profits from voyages. I have worked out for my firm a good many voyages on this basis, and although the results seem to be slightly in favour of the oil engine I must confess that the figures are not very convincing. If some makers of oil engines can convince me that my suppositions are incorrect I shall value the information.

I should like to congratulate very much the Secretary and the executive of the Institution on having succeeded in getting these world-wide-known and representative firms to give papers on their respective engines. To me it is by far the most interesting item on the agenda of this session, and I think the information given is of the utmost value to the shipowner who is keeping his eyes open. I know when I asked our consulting engineers a little while ago about many of these details they

Mr. Souter

sent out enquiries for information, and when I read the Papers which were given a fortnight ago I found that much of the information I was asking for was contained in these Papers. It is quite evident that this Institution is providing material for which there is a great demand, and on my own behalf and on behalf of shipowners generally I should like to convey to the Institution and the Authors of the Papers our most hearty thanks.

Mr. Ainslie

MR. A. F. AINSLIE, *Member*: One hesitates to call in question any part of this admirable symposium, but I should like to ask whether it is certain that Fig. 1 is based on a correct interpretation of Froude's work. While it is true that increased power is usually required with faster-running engines the reverse is true when the propeller diameter is much restricted. Increased revolutions do not necessarily lead to lower efficiency, and indeed for the specified conditions a rising efficiency is obtained over a long range.

Recently I had occasion to consider a very similar case and to find out how the propeller efficiency was influenced by varying the revolutions. Four diameters were investigated, each over the pitch ratios from 1·5 to 0·8. It was found quite definitely that only with exceptionally large and therefore slow-running propellers does the efficiency fall with increase of revolutions over the entire range of pitch ratios stated. For the present case it is expected that the various builders would prefer to keep within these limits for pitches.

For the proposed ship the slip-ratio is high and the propeller efficiency is rising with increase of the revolutions. It is when the slip is low that, for increasing revolutions, the efficiency falls; but when the slip is high the efficiency improves with the revolutions over a considerable extent. It would appear that the idea of necessary loss of efficiency with increase in revolutions is a tradition from the very early days of screw propulsion, and it is surprising that it should still linger.

The propeller question is not one that can be ignored, for unless it is given due attention the results may turn out very unsatisfactory, and it is known that some of the troubles of internal-combustion engines in marine work have been traced to the propeller end. In one case I know of, when the engine was "all out" the revolutions were 10 per cent. less than desired which is much too great a discrepancy.

One firm mentions a cast-iron propeller and another recommends bronze blades; why should both use the horsepower of Fig. 1 corresponding to the revolutions chosen? It would add greatly to the precision of all the Papers to learn what particulars of diameter and pitch are considered suitable for each proposal. Each firm would use a Michell thrust block so that with the low surface speed at the bearings the shaft friction is only of small consequence.

Those designers who have chosen a high speed of rotation are unduly handicapped, as R. E. Froude's published work, in this case, is in their favour and not to their disadvantage, for it is unlikely that any usual

values of the co-efficients—wake, thrust deduction and relative rotative efficiency—can have been used with his propeller results to give the almost linear increase of brake-horsepower over the range of revolutions shown in Fig. 1. The matter is of so much importance that I trust I shall be forgiven for challenging the very basis of this comparative Review, as the figures quoted for the running cost per ton-mile, at least, require to be revised.* Mr. Ainslie

Additional note communicated 9th March, 1923.—Just to keep the matter in order I should like to say that my remarks at the discussion were made on the original specification giving 14 feet 9 inches for the maximum diameter allowable in the aperture, and on which dimension the proposed designs were submitted and read. Now that 15 feet 9 inches is given I need only add that anyone interested should look into the matter independently, for even at this diameter the point of maximum efficiency still lies above 85 revolutions per minute.

The PRESIDENT, Sir Archibald C. Ross, K.B.E.: Mr. Ainslie has given us subject matter for another paper. The Reading Committee will certainly hope that some day he may favour us with a paper on propellers. The President

Mr. R. J. EYRES, *Member of Council*: We have heard the remarks of a shipowner. It has been my good fortune to be consulting engineer to shipowners whose first question always is: Will it pay? Now, I must say perfectly candidly if a tramp-ship owner came and said: Shall I fit an internal-combustion engine? I should say No, emphatically, and it is only reasonable that I should say why I take up this attitude. First cost plays a very important part—perhaps more so to the shipowner than to the consulting engineer. What we are faced with as consulting engineers is the choice of engine. Now I am not going to be tempted to get on to thin ice, but I will say that personally I should be compelled to give the choice to the firm that had made the greatest number of engines and had had the greater experience. Mr Eyres.

* The diagram (Fig. 1) connecting b.h.p. and propeller revolutions is the envelope of a series of curves of b.h.p. on revolutions for diameters ranging from 12 feet 9 inches to 15 feet 9 inches (not 14 feet 9 inches as inadvertently printed in the first proofs of the Review) with a few spots for larger diameters as a check on the ending of the curve at the 85 r.p.m. end.

Mr. Ainslie in his interesting remarks does not suggest any definite curve of b.h.p. on revolutions which he would consider more suitable than that shown on the diagram, and it does not appear necessary to enter into a detailed discussion of the data upon which the construction of the curve is based.

It is, however, believed that the interpretation of Mr. R. E. Froude's model propeller results used in the derivation of the curve is quite correct. A great many estimates of quantities other than the open water efficiency and power characteristics of model screws are of course involved in the construction of any such diagram, and considerable difference of opinion may legitimately exist as to the values to be assigned to these quantities. It was to eliminate such comparatively irrelevant differences of opinion from the discussion of the Papers contributed to the Review that Fig. 1 was incorporated in the specification, and it is believed that Fig. 1 is a perfectly fair basis for the design of engines running at from 85 to 130 r.p.m. for the Review ship.—*Editor.*

Mr. Eyres

The life of efficiency is unknown and the human element plays a very important part. The shipowner looks to his consulting engineer to find reliable, suitable men to take charge of his property. I ask anybody here: Could I to-day or to-morrow, immediately get hold of five suitable men to go away and take care of the engines of a big ship? Not only that; Do I know with certainty in case of illness or accident whether I could, at a moment's notice get a reliable man sent out to join that ship? I do not think I could. That is one of my reasons—and a strong one—for taking up this attitude, and I do not think the time has arrived for putting internal-combustion engines in an ordinary tramp steamer.

I have a note of breakdowns at sea, but I was told only quite recently, "Yes, but you know internal-combustion engines do not break down at sea." I was delighted to hear that, but it is not correct. The consulting engineer has to be a very optimistic man, and it is no use going to the shipowner unless you are optimistic, but you must back a sure starter and a dead-certain winner. In other words the shipowner always comes back to this point: Is this engine going to pay?

I know I am voicing the thought of every consulting engineer in the North of England, numbers of our Society (The Society of Consulting Marine Engineers) in saying that we are very much indebted to the Institution for these Papers that have been read here. Personally I have followed them very closely, as I have followed this question for the last two years, although I have not been able to give as much time to it as I should have liked. I am convinced that the internal-combustion engine has come to stay, and it will come in time to our tramp steamers; but I still think I am justified in saying that that time has not yet arrived.

Mr. Tweedy

Mr. G. F. TWEEDY, *Vice-President*: The few remarks that I wish to make are not made as a Diesel-engine expert. I have been interested in the question of marine Diesel engines for a considerable number of years but I do not pretend to be an expert. It is only because I think this Paper was produced with a view to creating an interest in marine oil engines amongst shipowners and from that point of view that I would like to make one or two statements, although I am afraid they will be entirely in opposition to what my friend Mr. Eyres has just said.

As far back as 1909 or 1910 my firm built a small vessel to be fitted with Diesel engines, and since then we have built a number of vessels fitted with oil engines and we have had a certain amount of experience. I am also associated with another firm which, prior to the war, constructed entirely new works for the sole purpose of manufacturing marine oil engines. Having had opportunities to study the manufacture and the actual running of marine oil engines on service it is my opinion that shipowners can adopt oil engines for propelling purposes to-day without undue risk. It seems to me they are practically as reliable as the average steam propelling machinery. In saying this I would like to add that it is necessary to have engineers who have some

training and some experience of internal-combustion engines; but at the same time I do not think it is difficult to obtain engineers who can run marine oil engines. Any good engineer with the experience he would gain in watching the erection and testing of a Diesel engine in the shops would be quite competent to run them. What is necessary is to have—I was going to say, young men—at any rate men with a certain amount of keenness and intelligence. I consider the British marine engineer will very soon pick up all there is to learn and can soon run the engines entirely satisfactorily. As regards repairs and upkeep, my own view is —I may be wrong—that present-day shipowners would find that the repair bill for oil engines will not exceed the repair bill for ordinary steam engines and boilers. Mr. Tweedy

There is another point I would like to refer to and that is the question of consumption. The figures given for the consumption of marine oil engines by the various firms vary to the extent of about 10 per cent. It seems to me that it does not matter much from the shipowners' point of view whether one vessel fitted with any one type of oil engine is 10 per cent. more or less in consumption than the other. What is important to the shipowner is the established fact that the oil engine uses very much less fuel than the steam engine, its consumption is only 30 to 35 per cent. or in other words 65 to 70 per cent. less than what is used in steam-propelled ships, with ordinary reciprocating engines, taking oil fuel as the basis in each case. As regards cost, Mr. Souter has referred to this question and perhaps my experience has not been in connexion with purely tramp steamers: but as regards the estimates of costs of running in the various types of ships that I have had to do with, after allowing for, say, 15 per cent. interest, depreciation, etc., I think there was only one type of vessel where a saving was not shown and that type was the Canadian lake and canal vessels; otherwise the advantage has always been on the side of the motor vessel.

The President

The PRESIDENT, Sir Archibald C. Ross, K.B.E.: May I say what a pleasure it is to me to have Mr. Tweedy speaking here to-night? A very old friend of mine, he is the son of a former President of the Institution, who was one of the big men in the engineering world. I believe this is Mr. Tweedy's maiden speech and I am sure we all hope that it will not be his last.

Mr. Fleming

Mr. J. FLEMING, *Member*: I have listened with great interest to the remarks made by the different speakers on the various topics introduced, and feel somewhat bewildered at the subjects which have been mentioned, some of which do not seem to have much bearing on the main theme which we have been asked by the Institution to discuss.

Professor Hawkes has made reference to the separate pump for lubrication of cross-head bearings for the Armstrong-Sulzer engine, and it may be of interest to state that such a provision is not necessary in

Mr. Fleming

the Camellaird-Fullagar, two-stroke engine, as the direction of load changes at every stroke.

With reference to Messrs. Doxford's contribution it is noted that two fuel-injection valves are in action for ahead running. Has this provision been found necessary to obtain the high rating at which this engine works? An auxiliary H.P. fuel pump is shown driven by motor. It is observed that the other solid-injection proposals have not included such a provision. Is this pump brought into use when starting the main engines? What is the lowest working pressure at which the fuel pumps have been proved to give efficient results?

As other two-stroke builders are now using salt circulating water throughout, has experience in Doxford's case proved it necessary to retain fresh-water cooling for both pistons and jackets; or is sea water too low in temperature for circulation in this engine?

We now come to the proposition placed before us, and here I might almost complain that nearly all the firms have stepped over the traces and have departed from the conditions laid down by the Institution. To do so has hampered a fair comparison being made.

It is noted that for the Scott-Still proposal a smaller machinery space than the basic length given by the Institution is shown and a claim made that on account of the small space into which such machinery can be accommodated a revision of the tonnage laws is desirable. I have tabulated figures for two lengths of engineroom for the Camellaird-Fullagar proposal both being about 13 per cent. of gross tonnage, 39 feet 1 inch not including light and air casings and 33 feet 4½ inches including light and air casings; net tonnage 2,430 and 2,490 with carrying capacity of 350,000 and 358,000 cubic feet respectively.

Considering the Scott-Still engineroom of 31 feet 10½ inches without and with light and air casings, there are obtained net tonnages of 3,110 and 3,016 with carrying capacity 363,000 cubic feet; engineroom percentage of gross tonnage, 8·48 and 11·3 respectively. Having regard to the existing tonnage laws, 2,430 tons is the smallest net tonnage obtainable combined with the largest possible cargo space, and with the smaller engineroom a larger cargo space can be obtained at the expense of an increase of 60 tons net tonnage.

When these net tonnages, 2,430 and 2,490, are compared with those for the Scott-Still proposal, namely 3,110 and 3,016, it is doubtful whether the increase of cargo space obtained in this proposal will justify the reduction of engineroom with such an increase of net tonnage, or even if any revision of the tonnage laws took place the amount of net tonnage might still be too great to give the shipowners the benefit.

I should like to ask our Scott-Still friends if one boiler has to do all the work? Do they consider one boiler quite sufficient since this must work both at sea and in port? I would not say it is sufficient, and if it is not sufficient then you would require another one, and you would have to add that on to the total weight of the engineroom machinery.

Regarding the utilization of the waste gases from a Fullagar engine, as the engine is low rated and slow running the temperature of exhaust is low, from 400° to not more than 500°. Consequently only a small amount of steam can be raised compared with that which could be obtained in the Vickers engine, in which 700° would probably be available and a high pressure of steam could be maintained. From the Fullagar engine exhaust possibly 8 to 10 lb. steam pressure could be obtained which could be used for heating or other ship's purposes, or the exhaust be used for heating feed water; there are, therefore, possibilities in the way of economy in these directions. Mr. Fleming

Mr. JOHN TINDALE (of the North Eastern Marine Engineering Co., Ltd.): In this highly interesting Paper many kinds of proposals have been put forward, and it would be perfectly bewildering to a shipowner without previous experience of motor-ships to make a suitable choice of propelling machinery. The most attractive from his point of view so far as the various proposals appear on paper would naturally be those of light weight, and requiring the shorter machinery spaces so as to increase the cargo-carrying capacity of the vessel. There are, however, more important considerations, namely, that of reliability and cost. Mr. Tindale

In the early stages of the development in the marine internal-combustion engine, owners were reluctant to adopt it owing to its somewhat doubtful reliability, but at the present time the element of novelty has passed, and shipowners who have not hitherto adopted the Diesel-driven ship are looking round for the most suitable type. It is therefore well to look back and see which types of motor have inspired this confidence, and established a reputation for reliability, rather than to adopt the most attractive proposition as it appears on paper.

Upon looking up the registers of the various classification societies we find that for vessels fitted with motors of 1,000 h.p. and upwards there is a great preponderence of four-stroke over the two-stroke type. In a search of this nature which I had occasion to make within the last few months, the proportion was about 7 or 8 four-stroke engines to 1 two-stroke, which is fairly conclusive as to the type of engine which has so far given most satisfaction.

The two-stroke engine is, of course, the natural line of development, but there is one difficulty which appears almost insurmountable, and which hampers the designer in evolving a mechanically perfect engine, *i.e.*, the piston rings have to work past the exhaust ports. These rings are, of course, designed to exert sufficient outward pressure to ensure gas-tightness during the high pressure of combustion, and when passing over the ports the outward pressure on the bars is increased to about three times the pressure on the higher parts of the cylinder. This is bound to cause excessive wear in way of the bars, and ultimate barrelling of the liner with consequent risk of breaking the rings as they pass over the

Mr. Tindale

ports. This was mentioned and confirmed by Mr. A. E. L. Chorlton in his recent paper.

Referring to the proposition of Messrs. Sir W. G. Armstrong Whitworth & Co., one point is brought forcibly to notice in Proposal (A): I speak of the separately driven scavenge air pump. The classification societies in almost every case require alternative means for carrying out the various functions; *e.g.*, a second pump is required for supplying cooling water, also an auxiliary compressor for supplying injection air, which can be used in case of a breakdown of the main pumps, and now that the scavenge pump is a separate unit, need of a stand-by pump becomes more apparent than when it formed part of the main engine.

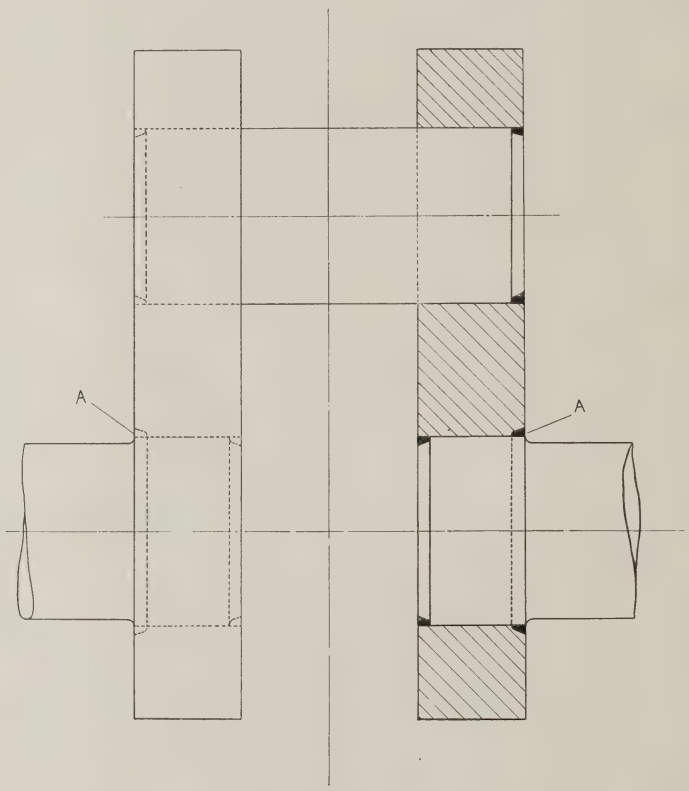

FIG. 5.—SHRUNK-WELDED CRANKSHAFT.

With reference to Mr. Keller's suggestion that contributors should state the method of crankshaft construction used, the firm with which I am associated (The North-Eastern Marine Engineering Co., Ltd.), has recently been carrying out extensive experiments in connexion with combined shrunk and welded crankshafts, the welding being so arranged that it is in sheer. Tests were carried out on this construction in comparison with the ordinary method of shrinking the parts together, by applying a turning moment to the web in an effort to move it round the shaft. The material used was ordinary ingot steel, the journal being

$6\frac{1}{4}$ inches diameter which was swelled to 7 inches in the web and the latter was $4\frac{1}{8}$ inches thick, the shrinkage being $\frac{1}{500}$. In the case of the ordinary shaft a load of $33\frac{1}{2}$ tons applied at a radius of 17 inches, *i.e.*, a moment of 570 ton-inches, commenced to move the web round the shaft. In the case of the combined shrunk-welded construction the general proportions were the same with the exception that a groove was turned at each end of the web bore, that next the shaft journal being in the web and the other being on the shaft as shown in Fig. 5. The grooves were approximately $\frac{1}{2}$ inch wide by $\frac{1}{2}$ inch deep and were filled in by the Quasi-Arc process of electric welding. The severest tests were applied, and when the full load, 58 tons, was applied at radius of 17 inches giving a moment of 986 ton-inches, the web was struck numerous blows with a 32 lb. hammer, the points of application being immediately over the shaft, and at the outer end of the web. However, none of these blows appeared to have any effect, no movement being observed between the web and pin, but the length of shaft between web and coupling, which was 10 inches, was twisted, taking a permanent set. Mr. Tindale

This proved that the joint was stronger than the shaft, and further tests are about to be made to ascertain whether the outer weld will be sufficient, as it is expected it will be, in which case weld (A) will be dispensed with. Microscopic inspection shows that the effects of the welding do not penetrate more than about $\frac{1}{10}$ inch, and as the shaft is swelled in way of web, there can be no objection raised as the strength of the various members is in no way affected by the welding process. This patented construction has the official approval of Lloyd's Register. Micro photographs of the welding and surrounding parts would take too long to describe, but can be shown to any interested parties after this meeting if required.

In conclusion it may be mentioned that our most recent motor-ship, M.S. "Segovia," has for a period of 6 months, between May and November of last year, averaged 10·3 knots at a speed of 107 r.p.m. the displacement being 2,976 tons and the fuel per day for all purposes 4·23 tons. The oil coefficient $\frac{V^3 \times D.2/3}{\text{oil per day for all purposes}}$ works out at the high figure for a ship of this size of 53,000. Our first motor-ship, the "Sevilla," has completed her first year, and only yesterday a letter was received from the chief engineer stating that the engines had passed their first year's survey, and the engine repairs were nil. The cylinder bores were measured in Christiania and are reported as being the same as when previously measured after six months' running.

The President

The PRESIDENT, Sir Archibald C. Ross, K.B.E.: My concluding remarks are the very warmest thanks of our Council and of all members of the Institution who have not taken part in the authorship or the discussion, to those who have.

CORRESPONDENCE.

Mr. Arthur

MR. J. C. ARTHUR, *Chairman of the Graduate Section*: In making a general survey of the proposals put forward in the Review it would appear that, in comparing the lengths of engineroom required for the various engines, some account should be taken of whether or not a thrust recess is claimed. If this recess be eliminated and the thrust blocks are placed in the engineroom proper, a considerable difference in the length of the latter is apparent; that required by Messrs. Armstrong Whitworth then becomes 42 feet 6 inches instead of 36 feet 1½ inches, by Messrs. Doxford 46 feet 9 inches instead of 40 feet 4½ inches, by Messrs. Scotts' 40 feet 4½ inches instead of 31 feet 10½ inches, by Messrs Vickers 46 feet 9 inches instead of 42 feet 6 inches, by Messrs. Hawthorn Leslie 57 feet 4½ inches instead of 46 feet 9 inches, while those required by Messrs. Palmers and Swan Hunter remain unaltered. Alternatively, if thrust recesses be allowed in the case of the last two enginerooms, Messrs. Palmers' length becomes 31 feet 10½ inches and Messrs. Swan Hunter's 36 feet 1½ inches.

The mechanical efficiency of any engine is an undoubtedly unreliable figure, and a value obtained with slightly imperfect indicating may well be up to 4 per cent. more than the true one. Since the fuel consumptions are given in terms of the b.h.p. the point is not one of the first importance, but it would be of interest to hear if the various engine builders concerned are prepared to state that the values they give will not be materially reduced by the adoption of special precautions in indicating.

The question of starting-air storage capacity is a very vexed one, and figures as to this, as to the pressure of storage, and as to the number of starts that can be made by the various engines on the capacity provided without recharging, would be of undoubted utility.

Messrs. Armstrong Whitworth state that the high pressures resulting from an 18 per cent. overload are not to be recommended. Does this mean that under this condition the *maximum* pressure shown on the card would be increased? It seems possible that some advantage might possibly accrue from a reduction of compression and maximum pressures in marine oil engines, and increased mechanical reliability and possibly some reduction in engine weight being so obtained at the expense of some increase in fuel consumption. The opinions of the participants in the Review upon this point would be of great interest.

The advantages of a supercharging engine over one with correctly designed opposed-port scavenging, appear to be negligible at the comparatively low revolution obtained in ordinary marine practice, and if, as is usually the case if the design be otherwise good, the "short duration" overload obtainable is limited by the perfection of the scavenging, *i.e.*, upon the amount of air available, the very moderate figure of about 118 lb./sq. in. i.m.p. on overload given by Messrs. Armstrong Whitworth cannot be taken as adequate compensation for the

added complication of supercharging. Is any trouble experienced with sticking rotary valves in service? Mr. Arthur

Messrs. Palmers' figure of 16½ r.p.m. for steady slow-running is an extremely good one, but further information as to the time over which the revolutions were counted and as to the weight of fly-wheel employed would be of interest. Slow running is, of course, to be expected of an opposed-piston engine and the actual relative piston speed upon which the compression temperature depends is, in the case of the engine concerned, 137·4 feet per minute and not 68·7. Have the b.m.p's. of 80-90 lb./□″ been maintained over long periods by the Camellaird-Fullagar engine or by other types of opposed-piston engine having a higher mechanical efficiency?

A weakness of the opposed-piston-engine cylinder liner would appear to be the presence of holes, for the injection and starting valves, in its most highly stressed portion. Have Messrs. Palmers or Doxford experienced any trouble because of this?

Messrs. Scotts' proposal is, if somewhat complicated, of extraordinary interest. Is the 35 per cent. overload obtained in this plant by increasing the load on the internal-combustion cylinders or by means of the oil-fired regenerator? The figure of 11 lb./□″ for friction losses is presumably arrived at by consideration of the ratio of the b.h.p. to the i.h.p. of the internal-combustion plus that of the steam cylinders.

Messrs. Vickers state that sufficient steam can be generated for their sea auxiliaries by means of a waste-heat boiler; what is the exhaust gas temperature? Sea-water cooling is used for the pistons in their proposed engine: it would be interesting to hear why such a system is being employed in preference to air cooling with alloy pistons used in some of their earlier engines.

Do the figures given in the Review for lubricating-oil consumptions cover in all cases all engineroom requirements or merely those of the main engines?

Mr. P. BELYAVIN, *Member*: Mr. Keller suggests, that the firms who have contributed papers to the Review should further supply some information *re* bearing pressures, etc. I wish to point out to Mr. Keller, that the bearing pressures do not mean anything at all. To calculate the bearings on the basis of maximum pressure per unit surface is an obsolete method inherited by some Diesel-engine designers from old steam-engine practice. A certain maximum pressure per unit surface of the bearing may make the bearing too small for a short-stroke engine running at a high piston speed or too big for a long-stroke engine running with a moderate piston speed. Mr. Belyavin

Steam-engine practice for bearing surfaces in this country is, of course, very sound in many cases: however, if we wish to adopt this practice for Diesel engines, the maximum pressure should not be used as a basis of comparison.

Mr. A. D. Bruce

MR. A. D. BRUCE, *Member*: In a perusal of the various proposals, perhaps the most interesting features are the weights, engineroom lengths, the cylinder diameters, and cost per ton-mile.

Weights.—The Werkspoor proposal is the heaviest one, but when compared with the Vickers proposal, the only other four-stroke representative, the adverse weight of 90 tons is accounted for by considerations of rating. The Werkspoor proposal embodies the latest developments for single-screw motor-ships, viz:—high propeller efficiency, long stroke, and moderate mean pressures.

When comparing four and two-stroke engines, Prof. Watkinson in his recent paper before the Liverpool Engineering Society, referred to the motorship "Peru," engined by Burmeister and Wain, and compared the Camellaird and Sulzer engines with the four-stroke engine actually fitted. He said that the respective net weights were 288 tons, 323 tons, and 456 tons, showing a saving for the two-stroke proposals of 37 per cent. and 29 per cent. respectively. In the Review the lightest proposals are the Still, showing a saving of 32 per cent., the Sulzer 25 per cent., and the Doxford only 18 per cent. In view of the fact that the two-stroke engine has two impulses for every one of the four-stroke engine, the difference in weight is not so great as one might have expected.

Engineroom Lengths.—In comparing engineroom lengths, it is surprising that a two-stroke contributor arranged for a length greater than the Werkspoor proposal. On referring to the two-stroke diagrams concerned it is apparent that such length is not required. Excepting this instance, the Werkspoor proposal has the longest engineroom, even including the other four-stroke proposal. The majority of the contributors have just qualified for the 32 per cent. deduction from gross tonnage, but in the case of the Still proposal, the figure is astonishing at 31 feet 10½ inches. On reference to their plan it is seen that the reasons for the saving in length over the two-stroke proposals are: No air compressor on forward end of engine, no scavenge pump, no middle space, while also, for approximately the same mean pressure on the combustion side of the piston, a smaller diameter of cylinder will suffice, on account of the power produced on the under sides of the pistons derived from the waste heat.

So far as the two-stroke proposals are concerned, a considerable saving in length was expected, but in the case of the Vickers proposal, at first sight it would seem that it was a much more compact scheme than the Werkspoor one could be. Here again it is principally a question of rating, which for a fair comparison should be as near equal as is practicable. The saving in engineroom length of two frame spaces is entirely due to the higher revolutions and mean pressure of the former, which enable shorter engine centres to be adopted, as a consequence of the main bearings, bottom ends and cylinder diameters having smaller dimensions. It should be noted that the fact of Vickers proposal having solid injection makes no difference in length, as the Werkspoor air compressor is driven by levers from the cross-head.

Cylinder Diameters.—The four-stroke cylinders are the largest, which is quite natural. The Werkspoor cylinder is 1·9 inches greater than the Vickers engine, on account of lower mean pressures and lower revolutions. The Werkspoor engine was selected from a standard range of engines, although a smaller engine could have been run out, having a mean pressure of 100 lb. per square inch on trial conditions; but the information regarding the standard engine is more accurate than an intermediate engine run out for the occasion could possibly be. The single-piston two-stroke engines are smaller again, any difference in diameter between the individual cases of this class being a question of rating, stroke-bore ratio, etc. Mr. A. D. Bruce

It is to be noted that the opposed-piston engines have a much smaller cylinder diameter than the single-piston two-stroke engines, this being due to the much greater effective strokes of the opposed-pistons for fairly constant revolutions. It is apparent that the Doxford design is influenced by the fact that it is being compared with other types, for it is their practice to recommend very low revolutions (for example their 3,000 i.h.p. engine running at 77 revolutions). Their high revolutions give Doxford the smallest cylinder diameter in the Review.

Cost per Ton-mile.—The basis is now a common one, and a comparison can be made for Diesel oil. The lowest values are the Still and Doxford figures. They attain such economical proportions on account of their small fuel consumption per day for all purposes. Their deadweights are also large. The cost per ton-mile for the Still engine is given at 0·0037 pence, but is only at a speed of 10·45 knots; thus it is about 5 per cent. lower than it would be if worked out at 10·75 knots.

The Werkspoor proposal, on the other hand, has a sea speed of 10·75 knots, as the engine can maintain its trial power continuously, but if 10·45 knots had been taken as the sea speed, which was not given as one of the conditions of the Review, the cost per ton-mile would have been reduced about 5 per cent. and would work out at 0·0045.

Mr. CHARLES R. BRUCE: *Armstrong-Sulzer Engine.*—In the Armstrong-Sulzer proposal the scavenging blower is driven by a steam turbine and one is led to ask if the winch condenser in the installation has an air pump to produce a vacuum. Is it intended that the steam produced from oil burned under the Cochran boiler is first used for driving the steam auxiliaries and then passed on to a turbo-scavenging blower exhausting to a vacuum condenser? This would certainly appear to be the most economical method of utilizing the steam in this particular case. Mr. Chas. Bruce

The cross-head lubrication in the Armstrong-Sulzer engine is ensured by using high-pressure oil. It is understood that the consumption of power is proportional to the pressure and the oil clearance. Does the cross-head in this engine have a much finer clearance than usual, and what method is adopted to prevent leakage at the ends? Would not better results be obtained by adopting a low bearing pressure associated with a low oil pressure?

Mr. Chas. Bruce

Camellaird-Fullagar and Doxford Engines.—These engines have no cylinder covers, but each cylinder has, of course, two pistons, and it is suggested that trouble may be experienced with the complication of water-cooling devices. The necessity of providing apertures in the cylinder liner in way of the combustion spaces for fuel-injection and air-starting valves appear to have disadvantages. Possibly Mr. Shannon and Mr. Keller are in a position to reassure us on this point.

In the Camellaird-Fullagar engine the disposition of scavenging pumps which appear to be in tandem with the main line results in a shorter engine, but does the arrangement not prejudice the accessibility of the top pistons, and are the scavenging piston rings not liable to over-lubrication?

The mechanical efficiencies of 85-86 per cent. stated by Mr. Keller as being obtained on the test bed strike one as being exceedingly good for an engine driving its own scavenging unit. This means that if this unit were independently driven the efficiency of the main engine would be at least 90 per cent. Does this result agree with Mr. Keller's views?

Neptune Engine.—In two-stroke-cycle engines the temperature of the exhaust gas depends upon the combustion efficiency, and upon the excess scavenging air supplied to the cylinder. Due to the complications of wave action in the pipes and the release pressure the proportion of excess air passing through each cylinder may not be the same, causing a variation in exhaust temperature between the respective cylinders. In the Neptune engine pyrometers are provided on each cylinder exhaust. Is it considered that reliance can be placed upon these readings to the extent of justifying an alteration in the powers of any of the cylinders, or would the indicators be utilized to ensure that fairly even mean pressures were carried?

Mr. Chaloner

Mr. J. L. CHALONER, *Member*: In view of the present unfortunate controversy between two-stroke and four-stroke advocates, which comes to the surface at every possible occasion, the choice of vessel made by the committee responsible for the drafting of the Specification is a particularly happy one. It is clear that for a unit such as is under consideration there is little to choose between the two principal types.

The practical usefulness of any questionnaire depends on the extent to which the original instructions are carried out; a disregard of the original suggestions introduces difficulties in compiling comparative results and thus detracts from the value which the figures are to prove to interested and to-be-interested parties. This deficiency has been lessened by subsequent modifications in the specification, but there are still a number of important items which cannot be included in the adjoining table thus defeating the object of intelligent comparisons of the limiting values which each manufacturer introduces with his own particular design. The figures are self-explanatory and furnish very instructive comparisons.

The question arises in connexion with the calorific value of fuel oil

TABLE I.

PARTICULARS:—	Hawthorn 4-stroke.	Vickers 4-stroke.	Armstrong 2-stroke.	Neptune 2-stroke.	Doxford 2-stroke	Camellaird 2-stroke.	Scott-Still 2-stroke.
Propeller speed, unit revolutions per minute	95	112	92	90	96	85	106
Continuous engine power (nos. of cylinder), total S.H.P.	1,848 (6)	1,760 (6)	1,840 (4)	1,840 (4)	1,850 (3)	1,828 (4)	1,870 (4)
,, ,, ,, ,, per cylinder S.H.P.	308	293	460	460	617	457	467
Cylinder dimensions, bore × stroke, m.m.	760 × 1,400	710 × 1,140	680 × 1,200	660 × 1,320	540 × 1,080	635 × 915	635 × 1,140
,, ,, (ratio)	1·8	1·6	1·8	2·0	2·0	1·5	1·9
Piston speed, feet per minute	870	840	725	780	680	510	795
Engine rating — Continuous — M.E.P. (indicated), lb. per sq. in.	91	96	101	100	103	91	30 (oil and steam)
Engine rating — Continuous — M.E.P. (brake), lb. per sq. in.	66	75	75	73	84	65	79
Engine rating — Continuous — Mechanical efficiency, per cent.....	73	78	74	73	82	72	88
Engine rating — Overload — Increased horsepower, per cent.....	8	?	19·5	25	23	25	35
Engine rating — Overload — M.E.P. (indicated), lb. per sq. in.	99	?	101	113	118	106	112
Engine rating — Overload — Piston speed, feet per minute	870	?	870	856	730	550	860
Oil consumption — Fuel oil — per B.H.P. hour (main engine), lb.	0·41	0·42	0·39	0·44	0·40	0·39	?
Oil consumption — Fuel oil — per B.H.P. hour (all in), lb.	0·45	0·43	0·45	0·46	0·41	0·40	0·37
Oil consumption — Fuel oil — per sq. in. of piston area, lb. per hour	0·18	0·20	0·32	0·38	0·35	0·18	?
Oil consumption — Fuel oil — per cub. ft. of cylinder volume, lb.	0·0020	0·0020	0·0020	0·0024	0·0024	0·0017	?
Oil consumption — Lubricating oil — per day, gallons	12	?	?	?	11	?	?
Cost per 1,000 ton-miles, pence — Light fuel oil	4·76	4·37	4·7	4·88	4·1	4·32	3·7
Cost per 1,000 ton-miles, pence — Heavy fuel oil	4·28	?	?	4·41	3·7	3·92	?
"Power-speed" co-efficient, continuous load....	1·46	1·34	1·54	1·48	1·47	2·5	?
,, ,, overload	1·34	?	1·30	1·18	1·20	2·0	?
General — Weight of engine, ton	550	460	414	430	450	430	375
General — Auxiliaries (electric), Nos. of	3-88 kw.	3- ?	2- ?	3-65 kw.	3-50 kw.	2-35 kw.	Steam.
General — Engineroom crew	11	9	?	?	6	?	12
General — Heavy fuel oil recommended	No.	No.	Yes, but qualified.	Yes, but qualified.	Yes, but qualified.	Yes.	Yes.
General — Exhaust gases utilized	No.	Yes.	No.	Yes.	No.	No.	Yes.

Mr. Chaloner

in estimating cost per ton-mile, whether it would not be better to take the gross value as the basis; this value is determined, whereas the net value is calculated, which more often than not means estimated.

The "power-speed" coefficient, or "maintenance" factor as a function of the former, is based on the assumption that for continuous service the product of piston velocity (feet per second) and mean effective pressure (brake) should not exceed 1,400.

The size of auxiliaries appears to introduce considerable divergency of opinion, thus reflecting very accurately the general position with regard to this point. The tendency for a type of vessel as referred to in the Specification is undoubtedly towards electric auxiliaries; the recommendations with regard to the utilization of the exhaust gases have not added much to change that opinion.

It is rather difficult to find the reason for the difference in the number of the engineroom personnel, because it would appear that 10 is probably the actual number that would sail with a vessel of this size.

The information with regard to the use of heavy fuels is instructive and indicates that there is no immediate necessity for recommending such fuels for the purpose of bringing running charges within competitive limits. The comments made by various manufacturers point to the necessity of straightening out the confusion which exists with regard to the constituents of a commercial specification and the method of analysis.

It is very gratifying to note the support that the scheme has received from the industry, and it is hoped that at a later date another specification may be issued to review the different types of engines in the light of, perhaps, a greater tonnage and a higher speed.

Mr. Keller

Mr. K. O. KELLER: *Armstrong-Sulzer Engine.*—Nearly all contributors propose to drive the bilge pumps off the engine which, owing to recent Board of Trade regulations forbidding the discharging of oily matter within three miles radius of the coast, will have to be abandoned.

Regarding removal of the crankshaft, I should like to ask Messrs. Armstrong Whitworth whether the thrust shaft and fly-wheel shaft are in one piece with the after part of the crank-shaft and whether structural parts of the engine will have to be removed in order to replace one of these shafts.

With steam-turbine driven blowers it will be necessary for reasonable economy to have condenser under vacuum. Perhaps this information will be given to us. There is a good deal to be said in favour of a reciprocating scavenging pump against high-speed blowers, steam or electrically driven, at any rate for cargo ships.

It will be noticed from the ship arrangement that the donkey boilers are screened in, which is a very desirable feature. With electric auxiliaries the donkey boiler has been dispensed with. How is it proposed to deal with viscous oil in the double bottom? The same remark applies to the proposal submitted by Messrs. Palmers.

Palmers-Camellaird-Fullagar Engine.—It must be very comforting to our friends in stating that "opposed-piston engines having brake mean pressure of 80 to 90 lb. have been satisfactorily at work for long periods." I assume that this has reference to the Doxford engine. Please confirm. Mr. Keller

It would be interesting to know the reason why Messrs. Cammell Laird base their engines at only 65 lb. mean brake pressure, which is the lowest of any two-stroke engine in the Review. On the other hand it must be borne in mind that in the Doxford engine 90 lb. mean brake pressure is obtained with the same mean indicated pressure as proposed in the case of Sulzer and Neptune engine.

The mechanical efficiency of the Camellaird engine is given as 73 per cent.: would Mr. Shannon state how this has been arrived at? The mechanical efficiency of any engine depends very much upon the reliance that can be placed upon the indicator cards. To obtain reliable information of what is going on inside the cylinder a special indicator drive is necessary in the opposed-piston type of engines. I notice in a recent publication on trials carried out on the Camellaird engine by Professor Watkinson, that diagrams were taken to correspond with the movement of the lower piston, and separately to correspond with the movement of the upper piston, *i.e.*, the mean pressure was obtained from two diagrams taken in one and the same cylinder at different times. This would not appear to give reliable information.

I thought it curious that reference should be made to the difficulties in starting a Diesel engine from cold. Why not adopt the obvious remedy? I do think that engineers on the whole are a stubborn class—I am not thinking of shipowners' superintendents. We get accustomed to do one thing and it appears that nothing will induce us to try something else. We have here a case in point, but I understand that Burmeister & Wain are now warming up the cylinder covers with steam—which should appeal to the shipowners' superintendents—while others prefer to adhere to the bogey that "so-and-so's engine cannot start from cold." We all know that the heat carried away by the jacket water is lost, yet after thirty years of experience of Diesel engines we still cool the cylinders, covers and pistons as rigorously as we can devise, except in the case of the Still and Doxford engines. Overcooling of these parts is as wrong in principle as it would be wrong to brick line a furnace to save it from collapse.

Neptune Engine.—In this engine the pistons are cooled by sea water which is conducted through telescopic tubes and a specially designed stuffing-box. I would like to ask Mr. Rowntree whether, in case the glands do become leaky (an occurrence always to be reckoned with in the best design) the sea water could drip into the crank-pit and mix with the lubricating oil. The telescopic tubes have been tried with much perseverance in the Doxford experimental engine where fresh water was used but have had to be abandoned in preference to a more costly device.

Mr. Keller

It would appear impossible to keep a sliding stuffing-box tight for, say, thirty days on end, and more so when navigating in sandy waters.

In this engine it is also proposed to pass the exhaust through a boiler. Since the engine is of the two-stroke type it would be interesting to know the exhaust temperature at a given mean indicated pressure, and the weight of steam that could be generated at that load and 100 lb. boiler pressure. It would also be interesting to know the material with which the exhaust gases come in contact in the boiler, as oil containing sulphur may have to be used.

General.—I can support Commdr. Hawkes's view with regard to the Scott-Still engine on consumption per i.h.p. In a two-stroke engine the cooling effect by the excess scavenging air is very considerable and it would appear false economy to cut down excess air to the benefit of the boiler. In the Doxford engine the exhaust temperatures are down to 400 to 500° Fahr. measured 12 inches from the exhaust ports with 100 lb. mean indicated pressure and a consumption of 0·33 lb. per i.h.p. With these temperatures I would not expect to maintain 100 lb. boiler pressure, not to speak of generating steam at any appreciable amount.

The auxiliary power proposed for the Werkspoor installation of 274 kw. would appear to be excessive for the requirements of deck machinery and the size of vessel. An explanation will be appreciated.

Mr. Myles

Mr. DAVID MYLES, *Fellow*: If seven steam-engine builders were asked to quote for steam engines for a cargo vessel, there would be very little difference between their proposals; but a comparison of the sizes of engines put forward by the Authors of the papers contributed to the Review shows there is still a great difference of opinion regarding the best dimensions and the relation of diameter to stroke of the internal-combustion engine. This, together with the number of designs included in this Review, indicates that this type of engine is still in process of development—that the designs built by all makers will eventually become a little more nearly a similar, appears to me very probable. Even now and certainly when that stage is reached—there can be no doubt there is a field for the internal-combustion engine. But I do not agree with some enthusiasts who say steam engines will soon be found only in museums. The steam reciprocating and turbine engines will still be best in their proper places, and I do not think there will be the difficulty of getting sea-going engineers fully qualified to take charge of marine internal-combustion engines that Mr. Eyres and others fear there will be.

The question "Will it pay?" is certainly the most important one of all: the engine builder can quote figures and show tempting results, but this question can be definitely answered by the shipowner only and only by him after extended experience. There are so many allowances to be made on both sides of the question, so many small things that are of so great importance, that there must always be an element of risk until experience and results accumulate. This end can never be reached unless some owners face the risk after careful enquiry into the matter from

all known points of view. This is being done in an increasing number of cases and the fact that some owners, after considerable experience, have continued to order new vessels with internal-combustion engines, at least indicates that so far they are satisfied. Further developments in the design of internal-combustion engines are sure, and reduced first cost and increased economy will assist in bringing them into more general use. Mr. Myles

Mr. Ainslie, speaking about revolutions and the propeller question, referred to the efficiency and I understood him to say there is little difference between a large and a small propeller in that respect. This is quite correct in ordinary weather but with a usual-type cargo vessel in bad weather the efficiency of the small diameter falls very quickly and seriously.

The designed pressure on the principal bearings has been referred to but without a clear statement how that figure is arrived at, the information may be very misleading. The difference between the maximum and minimum pressures in an internal-combustion cylinder is much greater than in a steam cylinder.

I congratulate the Institution on this most interesting series of papers and consider we are greatly indebted to whoever originated the idea of such a Review, as well as to the Authors who have taken part in it.

Mr. A. I. NICHOLSON (Messrs. Scotts' Shipbuilding and Engineering Co., Ltd.): The results put forward by the different contributors form a valuable and most interesting comparison, and, apart from a few differences of interpretation, are all on a comparable basis. The indicated mean pressures proposed for the engines are interesting. Four of them are over 95 and three are under 95 lb. per square inch. Another point of interest is the sea power that the contributors suggest. In two cases only (Messrs. Vickers and Messrs. Scotts') a power less than the trial power is proposed, so that indicated pressures required will be, except in those two cases, a little higher at sea than on trial, due to the reduced speed of the ship. It would be interesting to have confirmation that this is what the contributors contemplate. Mr. Nicholson

The mechanical efficiencies stated for the several types of engine are not so different as they appear at first sight. The engines in which power is allowed for blowers and air compressors have an efficiency of about 73 to 75 per cent. The Doxford engine, which has no air compressor, has 82 per cent. efficiency. Of the four-stroke engines the Werkspoor has a compressor to drive, and its efficiency is 73 per cent. while the Vickers' engine with no compressor rises to 78 per cent. Comparing the Vickers and the Scott-Still engines which drive neither blowers nor compressors directly, the mean frictional pressure per stroke amounts to 11 lb. per square inch in the Scott-Still and 10·5 lb. in the Vickers. As the latter is a four-stroke cycle engine ($10{\cdot}5 \times 2$) lb./square inch have to be added to the brake mean pressure to get the necessary indicated mean pressure, and consequently the engine efficiency is 78 per cent. as

Mr. Nicholson

against 88 per cent. in the Scott-Still engine. There is therefore reasonable agreement between these results.

Mr. Shannon has referred to the purity of the air samples taken from the Camellaird-Fullagar engine, the inference being that these are indicative of good scavenging. It may be pointed out, however, that many compression samples were taken on the Scott-Still experimental engine, which, when analysed, proved to represent practically pure air. A typical analysis was:—$CO_2 = 0{\cdot}25$ per cent.; $O_2 = 19{\cdot}3$ per cent.; $CO = \text{Nil}$; $N_2 = 80{\cdot}45$ per cent. Yet it is admitted that the scavenging in that engine leaves room for improvement, and the reliability of the samples obtained is felt to be open to question. The methods adopted by Mr. Shannon may, however, have ensured his getting reliable and representative samples, but the above experience is mentioned to show that things are not always what they purport to be, nor what they might reasonably be expected to be.

In the proposal put forward by Messrs. Swan, Hunter & Wigham Richardson, exhaust gases are used to produce the steam required for normal requirements at sea. Why, it may be asked, is the consumption for all purposes, viz., 0·48, so much above that for the main engines alone, viz., 0·43 lb./b.h.p. per hour? Messrs. Vickers raise steam in a similar fashion, yet they change their consumption for all purposes from 0·42 to 0·43 lb./b.h.p. per hour.

AUTHORS' REPLIES.

Messrs. Sir W. G. Armstrong, Whitworth & Co.'s Reply (By Mr. Harold Thomson).

Mr. Thomson

Reply to Engr.-Commdr. Hawkes.—In reply to Engr.-Commdr. Hawkes's question, separate pumps are fitted for the lubrication of the cross-head bearings as he states. It is considered that this method of supplying oil to the cross-heads is simpler and more reliable than that in which the oil is supplied to the bearings through holes in the connecting rod, as is done in some other types of engine. The pumps are designed to work at an oil pressure, if required, of 300 lb. per square inch, but it is not essential to work at this pressure and in the case of the "Conde de Churruca" the pressure varied from 75 to 250 lb. depending upon the conditions under which the engine was running. It is, of course, not essential to run at the full pressure of 300 lb. although it is just as easy for a pump to provide this pressure as, say, 30 lb. Any leakage that may take place will return to the crank case. It is preferred to have an excess of pressure to avoid any anxiety as to the running of the cross-head bearings should they have been badly fitted, or have uneven surfaces. In the event of a break-down of these pumps a supply can be obtained direct from the forced lubrication system.

As regards sulphur, I understand that among the oils at present known, only Mexican and Californian have a high sulphur content,

which may range from 2 to 5 per cent. For example, in a test of straight Mexican crude oil 0·929 specific gravity at 15° C., carried out at the Swiss Federal Fuel Testing Laboratory, Zurich, the sulphur content was found to be 3·6 per cent. with an asphalt content of 15·83. I understand that the assumption which formerly prevailed that sulphur content was detrimental has found but slight confirmation. Messrs. Sulzer advise, however, that exhaust gases containing sulphur should be kept away from air compressor if the suphur content of the fuel exceeds 1 per cent. as the condensation of acid vapours may cause corrosion inside the compressor. This danger may arise if the working pistons, or exhaust piping are not tight. Combustion gases can then find their way to the engine-room and the air inlet of the compressors. Mr. Thomson

The 15 per cent. of asphalt mentioned is intended to represent the total quantity. The term asphalt seems to be rather an elastic one which is sometimes used to cover various residues which frequently consist of substances that are of quite a different character from natural asphalt and the term used in our Paper is intended to cover residues.

Reply to Mr. Arthur.—Mr. Arthur apparently considers that the comparison of the lengths of the enginerooms should be made by eliminating the thrust recess. It is difficult to see for what reason this should be done as there does not seem to be any valid objection to having a thrust recess if this enables valuable cargo space to be saved. It might equally well be pointed out that considerable saving in space would be effected if the engines were placed aft and the tunnel dispensed with. It will be noted that in the Armstrong-Sulzer proposal the thrust recess is in no way detrimental to the arrangement of the machinery and the thrust shaft can be removed when required.

I am not quite clear as to the meaning of Mr. Arthur's remarks regarding the mechanical efficiency of the engines and the modifications in the mechanical-efficiency figures which would be brought about by special precautions in indicating. The figures given for the Armstrong-Sulzer proposal are those which can be obtained regularly on test-bed trials carried out with the most approved type of instruments and the greatest scientific care and are in no way exaggerated, so far as I am aware.

With regard to air starting, the storage capacity provided with the engines proposed is quite ample for all ordinary purposes. In the arrangement put forward the pressure of storage is 1,000 lb. per square inch. If desired by owners low-pressure starting reservoirs can be provided for manœuvring when the engine is warm. The air-storage capacity and the relative advantages of high- and low-pressure starting are largely matters of opinion.

With regard to the statement that the high pressures resulting from 18 per cent. overload are not recommended, this does not mean that the maximum pressure would be increased under overload conditions, but rather that the resulting mean pressure would be increased with the

Mr. Thomson

development of additional power in the cylinder. Mr. Arthur does not appear to have understood that the advantages of super-charging go far beyond the mere question of overload as it is found that this system much improves combustion even when working under ordinary normal conditions. It may be mentioned that Messrs. Sulzer have built many engines with head valve scavenge and ordinary port scavenge with a single port and they only adopted the supercharge port when it was definitely proved by numerous experiments that considerable advantages were gained thereby.

In reply to his enquiry regarding sticking rotary valves, so far as we know, no trouble in service has been experienced through rotary valves sticking. When the "Conde de Churruca" returned to this country after 9 months service, the rotary valves for the whole of the four cylinders could be turned from the end of the rotary shaft by hand quite easily and no trouble whatever was experienced with sticking, nor do we think that this is likely to occur.

Reply to Mr. Keller.—The thrust and fly-wheel shaft in our proposal is independent of the after part of the crank-shaft and this shaft can be removed without disturbing the entablature of the main engine.

I agree with Mr. Keller that in order to obtain the best economy with turbo-blowers it will be necessary to have the condenser under vacuum and this is arranged for in our designs where the power to be developed makes it worth while.

In reply to his enquiry regarding viscous oil in the double bottom, as this vessel was not designed to carry oil fuel and presumably would use Diesel oil, it was not considered necessary to heat the fuel oil; but should it be proposed to use oil of such a viscous character that it could not be pumped without being heated, then it would be necessary in an all-electric proposal to instal a small boiler for this purpose. It may be mentioned here that with electric auxiliaries, the estimated fuel consumption would be about 0·43 to 0·44 lb. per b.h.p., depending upon the arrangement and type of auxiliary machinery in the vessel.

Reply to Mr. Tindale.—Mr. Tindale calls attention to the preponderance of four-stroke engines over two-stroke engines. The explanation lies in his own statement that "the two-stroke engine is, of course, the natural line of development" and seeing that it is a later design, there are naturally fewer of this type of engine at sea than of the four-stroke type which has been longer on the market. Statistics of this kind prove nothing as to the relative advantages of the two-stroke types of engines. There is no doubt that, had the port-scavenge type been developed earlier there would have been many more ships afloat with two-stroke engines than at present. The two-stroke engine received a serious set back due to the complicated system of cylinder-head valve scavenge adopted in the earlier designs and the consequent troubles with cracked covers.

I note Mr. Tindale's objection to the piston rings working over the exhaust ports. The answer is that there are many hundreds of engines

working to-day under these conditions and giving satisfactory results. Mr. Thomson
This matter is also dealt with by Mr. Still and I agree with his remarks.

With regard to Mr. Tindale's remarks concerning the separate scavenge blower, it will be readily admitted that there is very little possibility of anything going wrong with the turbine blower itself, which is a very simple piece of mechanism working at $1\frac{1}{2}$ lb. pressure; and spare parts for the turbine or the electric motor (if it is driven electrically) can be supplied if thought necessary.

If the scavenge pump in the Armstrong-Sulzer steam proposal with steam auxiliaries and all the other auxiliaries necessary at sea, were worked by the main engine, the consumption would be reduced beyond that given in the Paper; but the relative advantages of this proposal and that put forward would require to be studied so that the most advantageous proposal for the particular type of ship could be decided upon. Matters of this kind depend upon many circumstances which it is quite impossible to cover in a short review of this kind.

Reply to Mr. Meikle.—Mr. Meikle asks for an expression of opinion regarding the relative advantages of bedplates being fitted on tank top and on seats. Here again it is impossible to lay down a hard and fast rule; but personally I am at present in favour of building strong fore and aft girder seats where this can be conveniently arranged in the ship. Cases, however, arise, especially when the machinery is placed aft, where this arrangement does not work in well with the design of the ship and in some cases there is very little alternative choice in the matter. I consider it very essential with an engine seat of the built-up type that the vertical plates should be trimmed flush with the angles supporting the top plate so as to take as much as possible of the shear off the rivets.

Reply to Mr. Eyres.—While I cannot agree with all of the remarks made by Mr. Eyres I am glad to note that he thinks the internal-combustion engine has come to stay and that it will be used, at no distant date, in tramp steamers. Personally I have no doubt that the marine engineer of to-day is quite capable of running Diesel machinery with very little additional training. Although, no doubt, it is very good to be safe and conservative, if all shipowners had adopted Mr. Eyres's attitude we should never have had the "Mauretania" or "Aquitania," nor should we have had the steam turbine; and I think it is certainly a good thing for the shipowning industry that there have been shipowners with progressive and far-seeing minds who have been willing to take risks for the sake of progress. These risks are now being reduced to a minimum and I see no reason why a shipowner at the present time, should not order tramp steamers with Diesel engines with confidence, especially when one considers the enormous saving in the cost of fuel to be obtained by their use.

Reply to Mr. C. R. Bruce.—The cross-head of the Armstrong-Sulzer engine does not have a finer clearance than usual and no special means are adopted for preventing leakage at the ends of the bearings. I do not think that anything is to be gained by increasing the cross-head surfaces beyond that now adopted in the engines proposed by us.

Messrs. Cammell Laird and Palmers' Reply (By Mr. James Fleming).

Mr. Fleming

The Doxford engine is not the only opposed-piston engine that has worked at brake mean pressures of over 80 lb. On the first voyage of the "Malia," the first Camellaird-Fullagar-engined vessel, the engines developed over 80 lb. brake mean pressure for many days continuously. The low rating of 65 lb. has been chosen as one that can be maintained for any length of time under all circumstances giving reliability, which coupled with the low rate of revolutions, renders this an attractive proposal for a cargo vessel—that being the proposition before the contributors to the Review. By taking advantage of the oblique rods no special indicator drive is required in the Camellaird-Fullagar engine and the diagrams obtained are as reliable as any taken from single-acting engines.

Does Mr. Keller take all his diagrams from four cylinders on the same revolution, so that his total power is an exact record of the power developed during that revolution?

The warming up of cylinders is certainly worthy of consideration for, if the necessity does not call for the provision of a boiler in a ship, there will be a considerable saving by omitting it.

From observations of published drawings of the Doxford engine, the connecting rods would appear to be two strokes long, a comparison in height with the Camellaird-Fullagar engine, whose practice is $2\frac{1}{4}$ strokes will be obviously to the disadvantage of the latter. This will no doubt account for the similarity in height of the Camellaird-Fullagar and Doxford proposals (though of different stroke) in the Review.

In determining the length of engineroom, we adhered strictly to the minimum length given by the Institution, which was based on the 13 per cent. gross tonnage. This length of course could have been reduced as has been done in the Scott-Still engine proposal. We do not propose to dispense with the donkey boiler, and the remarks of Mr. Keller regarding the viscuous oil in double bottom will not apply in our proposal.

Messrs. Doxfords' Reply (By Mr. K. O. Keller).

Mr. Keller

Replying to Commdr. Hawkes's question on our experience on spiral-gear drive on the Doxford engine, in our first ships the camshaft drive is taken from the forward end of the crankshaft. This is wrong because all cyclic variations, which do occur in all crank-shafts, are imparted to the gear; in later engines the drive is taken from the after end. Difficulties experienced in the first two vessels have been due to excessive wear which has now turned out to be the result of defective material.

Our reason for adopting spur wheels and bevel drive is on account of constructional circumstances and partly in order to avoid spiral gearing, although this should not be taken to imply that the latter cannot be made to work satisfactorily.

Date.	Total Cargo Tons.	Speed Knots.	I.H.P.	R.P.M.	Fuel Consumption. Main Engine, Donkey Boiler, and Electric Auxiliaries.	Lubricating Oil Consumption per 24 hours.
Oct. 1921—Brixham to Batavia	7,200	10·71	2,650	70	11 tons at 24 hours.	13·8 galls.
Nov. 1921—Surbaya to Brisbane	5,300	11·29	2,500	69·2	10 ,, ,, ,,	27·4 ,,
Feb. 1922—Melbourne to Dunkirk	7,900	11·1	3,000	73·6	12·75 ,, ,, ,,	18·4 ,,
June 1922—Tyne to San Francisco	9,300	10·57	2,585	69·6	9·6 ,, ,, ,,	16·5 ,,
					Boiler oil at 30s. per ton.	
Aug. 1922—San Francisco to Belfast	9,600	10·5	2,570	69	9·85 tons for 24 hours.	18·5 ,,
Oct. 1922—Leith to Gothenburg	—	—	—	—	— —	—
Dec. 1922—Gothenburg to Table Bay	7,000	10·07	2,000	65	8·25 ,, ,, ,,	16·75 ,,
Jan. 1923—Table Bay to Melbourne	—	—	—	—	—	—
Average	7,717	10·7	2,551	69·5	10·24 ,, ,, ,,	18·56 ,,
M.S. "Eknaren."						
Nov. 1922—Hamburg to Batavia	8,200	10·8	2,200 approx.	69 approx.	8·15 tons for 24 hours.	—
Dec. 1922—Batavia to Brisbane	—	11·2	—	—	7·5 ,, ,,	—
M.S. "Dominion Miller."						
Engine Room Crew=4 Engineers, 3 Greasers.					Tons per 24 hours.	
Mar. 1922—Las Plams to Jacksonville*	850	11	2,430	72·5	9·25 2·25	28 gals.
Apl. 1922—Savannah to Bremerhaven	8,840	11·21	2,760	73	10 2·6	15 ,,
					Persian Boiler Oil.	
May 1922—Southampton to Tampa	2,980	11·47	2,850	74·6	10·4 3·8	18·5 ,,
					Texas Boiler Oil.	
July 1922—Savannah to Bremerhaven	6,230	12·15	3,000	76	10·8 3	20·9 ,,
					Texas Boiler Oil.	
Aug. 1922—Immingham to Philadelphia	8,950	10·6	3,100	77	11·4 3·9	22·5 ,,
					Texas Boiler Oil.	
Sep. 1922—Halifax to Liverpool	6,520	9·35	3,100	77	11·2 3·9	20 ,,
					Texas Boiler Oil.	
Nov. 1922—Liverpool to Boston	8,700	8·7	2,550	69	9·8 3·4	14 ,,
					Texas Boiler Oil.	
Dec. 1922—Halifax to London	7,557	10·78	2,600	73	10·25 4·4	19 ,,
					Texas & Persian Boiler Oil.	
Jan. 1923—London to Philadelphia	3,723	9·3	1,975	67·7	8·0 3·5	21 ,,
					Texas Boiler Oil.	
Feb. 1923—New York to London	7,128	10·4	2,400	70·7	8·8 3·7	17·4 ,,
Average	6,736	10·44	2,704	73·1	10·07 3·6	18·7 ,,

* Not included.

Mr. Keller

The figures of cost per ton-mile given in the seven proposals may serve as a basis of comparison, but they may prove quite misleading to the shipowner because the most important factor is left out of consideration, namely, the economical speed of the vessel. A typical illustration of what this means is given in the accompanying table of Trading Records (Table II.).

It will be observed from the trading records of Doxford single-screw motorships that in the case of the "Yngaren" it has been endeavoured to run the engine at a power beyond the economical speed of the ship and therefore the consumption of the first three journeys averaged about 11 tons per day. In later voyages this consumption has been reduced to about 9 tons. In the last ship, m/s "Eknaren," for the same owner, the consumption is actually still better, averaging about 8 tons per day. The remarkable feature about these ships is that the engines are all operated on boiler-grade fuel oil.

Mr. Meikle, on behalf of Messrs. Vickers, raised the question as to whether it is better to bolt the engine direct on to the tank top or whether it would be preferable to have built-up seating. The shipbuilder knows which of the two he prefers and as far as experience has shown no trouble whatever has been experienced in carrying either oil or water under the engine when bolted direct to the tank top.

Diesel-engine builders have departed in one important detail from the usual marine practice. With steam machinery it has always been customary to fit holding-down bolts on the inside and outside of the bedplate girders. The inner bolts have been abandoned in most Diesel engines due to the difficulties introduced by an oil-sealed bedplate. Considering the flexible structure of a ship this seems to be a precarious omission. An expression of experience on this point would be welcome.

The table hereunder, giving bearing pressures on Doxford oil and steam engines, does not take inertia forces into account.

Bearing Pressures on Doxford Opposed Piston Marine Oil Engine and Doxford Triple Expansion Steam Engine.

	Oil Engine.	Steam Engine.
Power, i.h.p.	3,000	3,000
Revolutions per minute	77	70
Max. pressure on pistons, lb./sq. in. ...	570	180
Max. load on pistons, tons	106	46
Bearing pressures—		
Main bearings, lb./sq. in.	Nil	245
Crank Pins, lb./sq. in.	650	453
Top end bearings, lb./sq. in.	1,200	970
Crosshead shoes, lb./sq. in.	35	54

Reply to Mr. Fleming.—Two fuel-injection valves are provided in all Doxford opposed-piston marine oil engines in order to obtain a high mean indicated pressure at a lower localized temperature in the combustion chamber and lower mean working temperature. For the normal sea conditions the mean indicated pressure is not higher than engines

of the proposed Sulzer and Neptune type, but with the Doxford engine there is a net gain of 9 per cent. in power obtainable on the propeller shaft for the same work done inside the cylinder of the Camellaird-Fullagar engine. Mr. Keller

The auxiliary H.P. fuel pump is a stand-by to the main fuel pump and is used for priming and testing the system prior to starting up. The fuel-oil pressures now in use vary from 3,000 to 4,000 lb./square inch giving fuel consumption of 7 to 8 tons per day in the three motorships now in service.

Whether salt water or fresh water should be used for cooling cylinders and pistons is a question of thermal efficiency and of design. The temperature of sea water should not exceed 100° Fahr. to avoid deposit of salt and at this temperature unnecessary heat is carried away from the combustion of fuel which could do useful work. Most designs of present-day marine oil engines do not give the margin to work with a higher cooling water temperature; hence the use of fresh water does not offer advantage.

Reply to Mr. C. R. Bruce.—No trouble has been experienced in the Doxford engine with the piston-water-cooling device referred to, neither has there been any disadvantage observed from the apertures in the cylinder liners for fuel-injection and air-starting valves.

Mechanical efficiencies of 85 to 86 per cent. were repeatedly obtained in our 4-cylinder 3,000 i.h.p. engine driving its own scavenging pump, allowing 6½ per cent representing i.h.p. absorbed by this pump the net mechanical efficiency is 91 to 92 per cent. including frictional losses in fly-wheel bearings and thrust block.

Messrs. Swan, Hunter & Wigham Richardsons' Reply.

Commdr. Hawkes comments that the consensus of opinion appears to be against the general use of heavy boiler fuel oil, especially that containing a high percentage of hard asphaltum. A large quantity of oil sold as boiler fuel oil is entirely suitable for use in a Diesel engine but unfortunately the term "boiler oil" at the present time covers a variety of oils and there is undoubtedly among these a small percentage the use of which in Diesel engines may produce undesirable results. Messrs. Swan, Hunter

Mr. Archibald Rennie and Mr. Meikle raise the question of engine seating. The out-of-balance couples and forces will in most cases be smaller than in the case of the ordinary reciprocating steam engine and on the other hand the Diesel-engine bedplate is, in the majority of cases, stiffer and stronger than a corresponding steam-engine bedplate, and there does not appear to us to be any reason calling for stronger seating than under normal conditions.

Mr. Rennie refers to water cooling systems as being a fruitful source of trouble in most heavy-oil engines. While this criticism might legitimately have been made some years ago, we think it is not justified to-day.

Messrs Swan, Hunter

There have been few features in connexion with the Diesel engine in which more marked progress has been made than in the development of efficient and reliable water cooling systems.

Mr. Rennie also raises the question of steam to be raised from exhaust gases, and he, as have other contributors to the discussion, expresses some doubt as to the amount of steam so obtainable. Our proposals in connexion with the utilization of waste heat are largely based on our experience at sea with two Diesel-engined vessels which we completed prior to the war in which two auxiliary boilers of the Cochran type were provided. A diverting valve was fitted in the engine exhaust pipe, enabling the exhaust gases to be passed through one or both of these boilers if desired. The engines in question were of small power, being only run at about 1,000 combined b.h.p. on service. The auxiliaries working at sea were:—Boiler feed pump; dynamo; steering gear; and service donkey occasionally.

Under these conditions sufficient steam was not obtained to provide for the whole of the requirements, but the quantity of oil burned in the boiler to supplement the exhaust gases was only from 0·8 to 1·0 ton per day. The steam requirements of Diesel-engine cargo vessels will not increase in anything like the same proportion as the b.h.p. of main engines and there appears to us little doubt that with engines of about 2,000 b.h.p. sufficient steam should be obtainable to meet almost all the normal requirements at sea.

Mr. Nicholson refers to this same question and asks why, if exhaust gases are to produce the steam required for normal requirements at sea, is the steam for all purposes raised from 0·43 for main engines to 0·48 for all purposes. 0·05 lb./b.h.p./hour represents rather less than one ton per day, and as it appears to us very desirable at the present stage of Diesel-engine development to be somewhat conservative in publishing statements of anticipated consumptions, such an amount is added as a reasonable margin to cover all possible contingencies.

Mr. Nicholson also raises the question of scavenging efficiency, and this is a question to which we have devoted much attention. Exhaustive experiments have been made, the scavenging process having been reproduced in a glass cylinder, which provides a very satisfactory way of observing the scavenging process. The amount of scavenging air for a clear scavenge was reduced to a remarkable extent as a result of these experiments.

Mr. Tindale remarks on the wearing of cylinder liners in way of the ports in two-stroke engines. This is a criticism which one does not hear made by anyone who has had actual running experience of these engines, and the fears that Mr. Tindale expresses have been proved to be groundless by experience over long periods of service. We would remind Mr. Tindale that the actual pressure of the rings is very small. Records taken over a number of years show the greatest wear at the upper portion of the cylinder for obvious reasons, which apply equally to both four- and

two-stroke engines. We cannot imagine that Mr. Tindale puts forward his figures as to the proportion of four- and two-stroke engines in the classifications societies' registers as a serious argument when considering relative efficiencies of machinery installations. Messrs. Swan, Hunter

Mr. Souter expresses some mistrust of electrically driven auxiliaries and a preference for the development of the utilization of the exhaust gases. It appears to us that the adoption of steam auxiliaries is the obvious course when sufficient or nearly sufficient steam can be obtained from the exhaust gases for service requirements; but in cases where the supply of steam from this source has to be considerably supplemented there is no doubt that the economy of the installation is seriously affected, and under these conditions the adoption of electrically driven auxiliaries results in a large saving in fuel consumption. We think the doubts as to the reliability of electrical machinery on shipboard have been largely produced by the adoption in some cases of unsuitable electrical appliances. There are probably few types of machinery in which one finds such wide differences of rating and of quality, and in cases where proper precautions have been taken in connexion with electrical plant installed on board ship, that plant has given reliable and efficient service over long periods.

Mr. Souter refers to the commercial point of view. When dealing with this the marine engineer is always somewhat diffident as it is difficult to put forward cost comparisons which are applicable in a general sense. Variation in price of bunker coal and fuel oil in different ports is so great that each particular service must be considered on its merits. We think, however, that a somewhat better case can be made out for the oil engine than is shown by Mr. Souter's figures. For an 8/9,000-ton steamer with engines developing, say, 2,000 equivalent steam i.h.p. on service, we think the additional cost of Diesel engines should not exceed £21/22,000, and on this basis we have usually found that, after allowing for 5 per cent. interest, 5 per cent. insurance and a higher rate of depreciation, etc., the Diesel proposal will give a further return of at least 10 per cent. on the additional capital expended.

Mr. Arthur refers to the length of engineroom and this has been very fully dealt with by other speakers. It is obvious that in the case of the Neptune engine a considerable increase in cargo capacity can be obtained by the adoption of a thrust recess and still further increase by the adoption of pocket bunkers in the engineroom; but it is questionable to what extent this latter would be considered worth while by the shipowner.

We think Mr. Arthur raises a point of considerable importance when he refers to the question of mechanical reliability and its relation to pressure. Fuel consumption can undoubtedly be decreased by the adoption of higher pressures and to some extent by higher rating of an oil engine, and both these are bound to re-act on the mechanical reliability. With the general low consumption of the oil engine it seems to us that

Messrs. Swan, Hunter

there is a danger of too much importance being attached to the actual figure of consumption per b.h.p.

Reply to Mr. Eyres.—We cannot agree with Mr. Eyres's lack of confidence in the abilities of the engineers of the British mercantile service, and we have no hesitation in saying that there is already an ample supply to whom it is safe to entrust the care of marine oil engines.

Mr. C. R. Bruce raises the question as to the amount of reliability that can be placed upon the engine pyrometers used with the Neptune engine and hints that there may be considerable variation in the distribution of scavenge air to the cylinders. The arrangement of scavenge pipes is in all cases entirely symmetrical with regard to the scavenge-air receivers and we think the danger of variation is not great and during fairly prolonged trials on a six-cylinder engine the whole of the exhaust temperatures lay within a range of about 25° C. We should certainly consider it justifiable to reduce the mean pressure in any cylinder which was showing persistently a high exhaust temperature.

Mr. Chaloner's table of comparisons is interesting and it is particularly noticeable that the fuel burned per cubic foot of cylinder volume shows a very marked agreement, and in spite of the apparent wide variation of cylinder dimensions, revolutions, etc., there does not appear to be any really great divergence in the ratings proposed by the various builders.

Mr Chaloner comments on the variation in the proposed number of the engineroom personnel and we are quite in agreement with him that it is difficult to see how this vessel can sail with less than 10 or the necessity for any greater number.

We strongly endorse Mr. Chaloner's remarks as to the necessity for a commercial specification for fuel oils.

Reply to Mr. Keller.—We do not agree with Mr. Keller that the recent Board of Trade regulation with regard to the discharge of oil within a three-mile limit renders it impossible to retain the bilge pumps to the main engines.

Mr. Keller asks whether it is possible, in the Neptune design of the telescope rods for the piston-cooling-water service, for sea water to drip into the crank-pit. We fully appreciate Mr. Keller's remark that in the best design the possibility of leaking glands must be allowed for. It is a special feature of the Neptune design, the result of exhaustive experiments, that should the glands leak it is impossible for water to find its way to the crank-pit. It is also important to note that no special type of packing is required and that gland friction is a minimum. An experimental piston-water-service gear has been running every day for more than a year.

With regard to exhaust temperatures and steam raised this has been referred to in our reply to Mr. Rennie.

Mr. Keller raises the point as to the deterioration of the boiler plates

due to sulphur in the exhaust gases. The result of experience with exhaust-gas boilers appears to be that at the temperatures that are anticipated in service, trouble from this cause is unlikely. Messrs. Swan, Hunter

Mr. Keller also refers to the cooling effect on the excess scavenge air, but, as explained in our reply to Mr. Nicholson, we have, as a result of exhaustive experiments, very considerably reduced the excess of scavenge air required.

Reply of Messrs. Scotts' Shipbuilding & Engineering Co., Ltd. (By Mr. Charles R. Bruce).

Reply to Engr.-Commdr. Hawkes.—The first point raised by Commdr. Hawkes has reference to the mean pressure on the steam side. In the Scott-Still proposal, the total mean pressure for the combustion and steam sides of the pistons is 90 lb. per square inch, and the mean pressure of the steam side is 6 lb. per square inch. In this connexion it has to be remembered that steam produced entirely by the engine, is also serving to drive the auxiliaries and the scavenge turbo-blower. Mr. Chas. Bruce

Commdr. Hawkes next refers to the fuel consumption per combustion i.h.p. of the Scott-Still engine, and makes some comparisons. The consumption of oil per combustion i.h.p. is 0·33 lb. for the Doxford and Vickers proposals, while for the Scott-Still engine it is, as he says, appreciably more, being 0·342 lb. In the Doxford opposed-piston engine there is a straight blow through for scavenging, and in the Vickers four-stroke cycle engine (which also uses airless injection) the cylinder is cleared by the passage of the piston. In the Scott-Still engine, on the other hand, simple port scavenging is adopted and the scavenging inlet ports are opposite the exhaust ports; also, the scavenging is not controlled except by the piston. As a result, the scavenging is not so good as in the other engines referred to, and the consumption rate per combustion indicated horsepower is somewhat higher. It is believed, however, that further investigations will have the effect of reducing this rate.

The next question concerns the combustion mean pressure of the engine. The combustion mean pressure in the Scott-Still engine is lower than in the other engines for the following reasons: Firstly, to reduce the consumption of fuel, which as is well known, increases as the mean pressure increases. It may be remarked, however, that the mean pressure has not been reduced, as suggested, to the point that gives the minimum consumption of fuel. A compromise has been adopted. Secondly, considerable experience with Diesel engines and a study of the conditions under which such engines operate at sea (*i.e.*, for long and continuous periods), both point to the desirability of adopting low indicated pressures in the combustion cylinders. In other words, it is considered better to aim at reliability by proposing a somewhat larger engine than might otherwise have been put forward.

Mr. Chas. Bruce

Notwithstanding the preceding remarks about the Scott-Still engine (as represented in this Paper), it seems only reasonable to observe that in this engine the oil-fuel consumption for "all purposes" works out at a low figure in comparison with other oil engines, and that the weight of the installation and the space occupied are both less than in any of the other types of oil engines under review.

With regard to the question of engineroom complement, one cannot lay down fixed rules, but it is recommended that the total number should not be less than 8 in any case, *i.e.*, one chief, 3 watch engineers, and 4 greasers, one of the latter to act as storeman when required.

With reference to the fuel consumption quoted, namely, 0·37 lb. per b.h.p. for all purposes, it is to be noted that this consumption applies to the continuous sea power. The fuel consumption figure does not include the fuel equivalent of the make-up feed. As stated in the Paper, the intention is to carry 40 tons of fresh water in the double bottom for this purpose. The evaporator is merely installed as a stand-by. The 40 tons referred to has been deducted from the cargo deadweight for the purpose of the calculation of cost per ton-mile. It may also be stated at this place that the weight of 375 tons for the Scott-Still engine includes everything from the propeller to the top of the funnel.

Reply to Mr. Keller.—In reply to the suggestion by Mr. Keller regarding bearing pressures per square inch, we have pleasure in giving the information desired for the Scott-Still proposal as follows:—Main bearings, 425 lb.; crank pins, 960 lb., and top ends, 660 lb. for combustion load and 475 lb. for steam load. These figures are based upon pressures per square inch of 500 lb. in the combustion cylinder, and 150 lb. in the steam cylinder. With regard to the material of the crank-shaft, this is of ordinary mild steel in the Scott-Still proposal, and the shaft is of built type with crank-pins and shaft body pieces shrunk into the crank webs and pinned in the usual manner.

Mr. Keller associates himself with Commdr. Hawkes in his remarks upon the oil-consumption rate per combustion indicated mean pressure of the Scott-Still engine. This matter has been dealt with in our reply to Commdr. Hawkes. We agree with Mr. Keller upon the cooling effect of excess scavenging air, but the quantity of air provided is in accordance with the best overall result, and has not been fixed to benefit the boiler.

We note that Mr. Keller condemns the over-cooling of cylinders, covers and pistons, which is the general practice of oil-engine builders, and that he excepts the Doxford engine and the Scott-Still engine. The design of both these engines permits of temperatures in these parts which result in low compression pressures with the accruing advantages. Between the two engines there is, however, an important difference; in the Scott-Still engine the heat from the jacket water is usefully employed in the production of power, whereas in the Doxford engine this heat is lost overboard to the cooler circulating water.

Reply to Mr. Meikle.—Mr. Meikle has raised a debatable point in connexion with the best form of engine seating. The Scott-Still engine is shown with a flat bedplate resting directly upon the tank top. This is considered to represent the best practice for various reasons. One must be assured, however, that the ship structure in way of the bedplate is rigid enough to meet all requirements, and, to ensure this, it may be advisable in some cases to increase the depth of the floors. No objection is seen to using the double-bottom tanks underneath the engine for oil fuel or water. Mr. Chas Bruce

Reply to Mr. Arthur.—It is not usual to include the thrust recess in the length of engineroom, because it is of no great width, and the cargo spaces at the sides are very valuable. It does not appear that the length of Messrs. Palmers' proposal could be reduced as suggested by Mr. Arthur, as a reference to the plan shows that the spaces at the sides of the thrust block are required for auxiliary engines. Messrs. Swan Hunter's proposal might be re-arranged with advantage, but it must be assumed that the firms mentioned consider the proposals they have submitted the best for their particular types of engines.

Referring to the questions arising out of the mechanical efficiency stated for the Scott-Still engine, it is agreed that inaccuracies occur in indicating the work done in combustion-engine cylinders. It is, in fact very difficult to get indicator readings that can be accepted as accurate. The results given for the Scott-Still engine have been based upon the brake power, and no changes are necessary in the consumption figures given. Special attention has been given to eliminate errors in indicating, as far as possible, in connexion with the shop tests of the engine upon which the figures given are based. The friction losses stated for the Scott-Still engine are the losses in mechanical friction of the whole engine, and represent the difference between the total indicated work developed in the combustion and steam cylinders, and the work measured at the brake. When comparing the mechanical efficiency of this engine with that of the others appearing in the Review, it is essential to bear in mind that in the Scott-Still engine no air compressor and no scavenge blower are driven by the engine itself, or allowed for in the figure given for the mechanical efficiency.

With regard to the benefits of reducing the compression and maximum pressure in oil engines, it is of interest to note that the Scott-Still engine has a compression pressure of 300 lb. per square inch. This is well under the pressure usual in other oil engines. The normal maximum pressure in the cylinder is, however, about the same as obtains in other oil engines, but the low compression pressure limits the maximum pressure that may be obtained accidentally, and gives an improved factor of safety by comparison with high-compression engines. We are of opinion that lower working pressures, as Mr. Arthur suggests, will become more or less general practice in the future even at the cost of a slightly increased fuel consumption.

Mr. Chas Bruce

We should have been pleased if Mr. Arthur had indicated in some way wherein he thinks the Scott-Still proposal is complicated. In this engine there are no high-pressure air compressors; no high-pressure-air starting gear; no camshafts and levers for fuel and exhaust valves; no piston-cooling-water fittings and connexions, while the manœuvring and reversing arrangements and operations are very simple. In view of these facts, which are significant and important, and notwithstanding that the Still system calls for a feed heater, regenerator and certain pumps which do not appear in ordinary oil-engine practice, it is confidently submitted that anyone who studies the question will appreciate that the installation with Scott-Still engines is certainly not more complicated than that with other oil engines.

Concerning the overload of 35 per cent. stated in the Paper for the Scott-Still engine, this overload is obtained from steam produced by the burning of fuel oil in the regenerator. The combustion side of the engine could be utilized to give a further overload as required.

Mr. Arthur's last point refers to lubricating oil. In the case of the Scott-Still engine, the lubricating oil consumption taken in the calculation for cost per ton-mile includes all the oil used in the engine-room when the ship is under way.

Reply to Mr. Fleming.—Mr. Fleming has no grounds for his complaint regarding the length of machinery space so far as the Scott-Still proposal is concerned, considering that the technical qualities of the vessel have been given for both the minimum length of machinery space required and also for the minimum length prescribed by the Institution as being necessary for a 32 per cent. deduction.

With regard to the tabulated figures for two lengths of engineroom which Mr. Fleming has taken the trouble to give, it is pointed out that light and air spaces are always associated with the engineroom volume when their inclusion is necessary, as in this case, so that the question of alternative lengths (with and without air casings) does not arise. Independent calculations show that under the most favourable circumstances the minimum length obtainable, with the light and air casings shown on the Camellaird-Fullagar plan, would not be less than 38 feet 3 inches, which is substantially in accordance with the length given by the Institution. It might be added that Mr. Fleming is not in a position to criticize the length prescribed since it is dependent upon the arrangements for closing in the cargo spaces in the bridge and foc'sle, the details of which have not been indicated by the Institution; but assuming the parts named were included it would result in an increase in length of engineroom of about 4 feet 8 inches (total length 42 feet 11 inches).

It will be seen, further, that Mr. Fleming's figures are in error regarding the relative capacities of the Scott-Still and Camellaird-Fullagar proposals. He states that the Scott-Still capacity for a length of 31 feet 10½ inches is 11·3 per cent., and that 13 per cent. can be obtained by the Camellaird-Fullagar proposal in a length of 33 feet 4½ inches (1 foot 6 inches additional). Considering that the gross tonnage

will not be less than 4,300 tons, the necessary increased length for 13 per cent. would require to be $\frac{(13\%-11{\cdot}3\%)\times 4{,}300}{49\times 25{\cdot}5} = 5$ feet 10 inches, giving a length of 37 feet 8½ inches; but the nearest frame space is 38 feet 3 inches, and these figures agree with the statement, previously made, that the Institution was correct in the basis length it stipulated. It is obviously unnecessary to discuss the other deductions made by Mr. Fleming except to state our agreement to his proposition that it would be unprofitable to reduce the length of machinery space by 1 foot 6 inches at the expense of increasing the register tonnage by 526 tons.

Mr. Chas Bruce

In view of these facts, which must be looked upon merely as additional matter to evidence which has accumulated from other quarters in the same direction, it is probable that Mr. Fleming will agree to our suggestion "that the present agitation" (of which we presume he is aware) "for revision of the tonnage laws is not without reason."

Mr. Fleming raises a question also with regard to the boiler included in the Scott-Still proposal. This boiler does not work, in the ordinary sense of the term, at sea. It is certainly in operation for starting and manœuvring when the vessel is entering or leaving port, under which circumstances oil is burned in the furnace. At all other times it serves the purpose mainly of a steam-and-water reservoir in circuit with the main engine. Its duty under these conditions is very light, for the maximum gas temperature to which it is then subjected is very low relatively. A stand-by boiler has been omitted in the interests of simplicity. If, however, any doubt upon the subject exists in the owner's mind, it is a very simple matter to fit an ordinary donkey boiler for use in port, and the total weight of machinery would be increased to a certain extent. On the other hand, it has to be noted that the main regenerator, when relieved of winch duty, would be considerably reduced in size and weight.

Reply to Mr. Chaloner.—The only comment to be made in reply to Mr. J. L. Chaloner has reference to the power-speed co-efficient or maintenance factor which occurs in the table he has supplied. He works out this factor upon the basis of piston velocity and mean effective pressure. We think it questionable whether such a factor really represents anything that has a relation to the actual maintenance for the following reasons. It deals with one cylinder only, and therefore is not representative of the machinery as a whole. Again, four-stroke and two-stroke engines cannot be placed reasonably on the same footing. It is to be noted further that opposed-piston engines are directly compared with single-piston engines, which is obviously misleading. At the utmost, this factor is comparable only for engines of the same type, and we do not feel satisfied that it is a truly representative maintenance factor even under these circumstances.

Reply to Mr. A. D. Bruce.—Mr. A. D. Bruce makes reference to the

Mr. Chas. Bruce figure given for cost per ton-mile in the Scott-Still proposal, and points out that a sea speed of 10·45 knots has been taken. This is quite correct, the sea speed given being that estimated as corresponding to 1,725 b.h.p., which is to be maintained continuously on service. At sea the speed and revolutions of a ship are less than the trial speed and revolutions owing to weather conditions. To develop the power corresponding to the speed decided upon, the mean pressure must be raised above the pressure necessary to obtain the same power on trial. In addition, the fact must be considered that the individual cylinders will not always take an equal share of the load, and it may be noted also that the propeller revolutions may not come out exactly according to intention. Messrs. Scotts' have had these points before them when choosing a reduced sea power for this ship, and it is to be presumed that Messrs. Vickers took the same view. Mr. A. D. Bruce is not quite accurate when he says that a lower speed than $10\frac{3}{4}$ knots was not given as one of the conditions. On this point, he is referred to the supplementary question No. 1, and the answer given to that question by the Council of the Institution.

Messrs. Vickers' Reply (By Mr. W. F. Rabbidge).

Mr. Rabbidge The specification issued by the Institution is much more definite than many received with ordinary trade enquiries, and was designed to bring the proposals to a fairly common basis. Nevertheless, the variations in the proposals, apart from those in the main engines, undoubtedly render comparison difficult. In actual tenders first price is a further most important consideration confusing the issue, so it is to be hoped that these papers and the discussion will show the desirability of owners giving particulars as fully as possible when forwarding enquiries. Nothing is more disheartening to an oil-engine manufacturer than to be expected to present an attractive proposition when the only information given is the speed of ship and the power of suitable steam machinery. The main question is in practice one of financial economics, so the service of the ship and the manner in which she will be run, the type of deck auxiliaries and similar information will assist the engine builder in selecting the best arrangement for submission to the owner.

The Papers have thrown a light upon the question of using boiler fuel, which is known to have interested owners, mainly due to claims lately made by various firms. It will be noted that in this particular case there is a general disposition to make reservations regarding the use of the more difficult fuels. It may be permissible to refer to the opinion on this subject expressed by Sir James McKechnie in December, 1921, and fair to deduce that marine experience is now bringing some of the new comers into line with that opinion (*vide* "The Motor Ship," December, 1921).

Proposals have been received for four-stroke engines in two cases only. The Vickers engine speed is 115 revolutions per minute on trial.

By reducing the speed from 115 to 90 revolutions per minute there would be a saving of 52 brake-horsepower, accepting the curve issued with the specification. This would entail about 80 tons on the machinery weight, the cost being of course increased, while the saving in fuel would be about 1 ton in 4½ days. Weighing up the matter, it was considered that the higher speed proposition would probably appeal to the majority of owners. As explained by Mr. Theunissen during the discussion, the Werkspoor proposal embodied a slower speed, and they were equally able to put forward a lighter-weight proposition; but even as the four-stroke proposals stand, it is plain that four-stroke engines running at reasonable ratings are able in this particular case to compete in point of weight and size with two-stroke designs, although some of the latter are using indicated pressures of a possibly high order for continuous sea work. Mr. Rabbidge

The fuel consumptions quoted may be expected to be subject to tolerances and, in some cases, to embody a measure of optimism. Putting on one side for a moment the Scott-Still proposition, it is doubtful whether the differences in fuel consumption would incline the owner to one engine or the other, there being more important points to consider first.

On the whole, it may be taken that the Papers show that there is available for the vessel in question a variety of oil engines, each with its particular claims for attention, and it will be for the owner to decide, after closely considering the compromise which each must represent, which engine most closely meets his ideal.

The Vickers design has been based upon reliability as its main feature. In mercantile conditions simplicity is regarded as necessary for easy comprehension, handling and overhaul. The underlying idea is that if complication exists there is a natural tendency towards neglect, so that sooner or later trouble will arise. For this reason the control is made very elementary, even at a slight sacrifice of fuel economy. Similarly, the mechanical-injection system is applied on account of its extreme simplicity, but this is at least equal in economy to the air-injection system. It is interesting to note as proof of this the zeal with which so many makers are now following Vickers' lead, and are evolving similar or alternative systems in which no air injection is used. The exhaust heating of the boiler is in this case proposed as being easily applied and leading to no particular complication. Its efficiency is warranted by sea running, the "Scottish Musician" having made long runs without oil fuel being used in her boilers, though the auxiliaries are independent. The engines in this case are twin screw, 2,500 total brake-horsepower, and the exhaust gas on entrance to the combustion chamber is at about 700° Fahr.

The cylinder pressure, which influences the fuel economy, is reduced well below that permissible, to give an easy-running engine and to allow for faulty adjustment. The consumption of 0·42 lb. per brake-horsepower per hour is conservative, but a deliberate sacrifice of economy

Mr. Rabbidge has again been made to meet what are believed to be mercantile requirements. Vickers Admiralty engines have given full-power consumptions as low as 0·378 lb. per brake-horsepower, which incidentally agrees closely with the Scott-Still anticipation, but the conditions in which these engines work are very different from those in the merchant service.

During the discussion a number of records of running at sea were shown. It may be of interest to see similar records from Vickers-built ships. It will be understood that in some of the ships the owners have not always desired full-speed passages, but the average speeds cover all runs, including a number of Atlantic crossings in tempestuous weather in which the ship speed was very low.

RUNNING REPORTS FROM SOME VICKERS MOTORSHIPS.

Name.	Miles.	Average Speed Knots.	Engine Fuel Tons.	Record up to
"Narragansett"	127,600	10·06	9·68	4/2/23
"Seminole"	112,500	10·22	9·81	1/3/23
"Scottish Standard" ...	72,200	9·77	9·2	28/1/23
"Scottish Maiden" ...	43,900	10·14	8·78	17/11/22
"Scottish Minstrel" ...	39,550	10·29	9·45	9/1/23
"Scottish Musician" ...	21,000	10·14	9·38	10/2/23

These figures are taken from the abstracts of the vessels, the speeds and fuel being taken from pilot to pilot.

Each vessel is 10,050 tons deadweight, designed for a trial trip speed of 10·5 knots, with twin-screw engines of 2,500 b.h.p. total.

In the "Scottish" class, the auxiliary consumption, unless exhaust gas is used, is somewhat higher than in the "Narragansett" and "Seminole," which average about $1\frac{1}{2}$ to 2 tons per day, as the "Scottish" auxiliaries are independently driven. With exhaust gas in use, long runs have been made without fuel being used under the boilers, all steam required for sea-going purposes being obtained from the heat of the exhaust gases.

Exact quantitative tests have not so far been carried out, so that it is preferred not to quote any definite figure in reply to Mr. Rennie's query; but the chief engineer's reports show that sufficient steam is generated to drive the 50 kw. generator which supplies current for lighting and for driving certain engineroom auxiliaries as well as the steering gear and refrigerator, to heat cabins, etc., to operate the fire and bilge pump as required periodically, and to operate the boiler feed pump and the circulation pump for the winch condenser—these covering all the services required at sea.

A number of specific points raised during the discussion, and not already dealt with, remain.

In reply to Mr. Arthur, I consider the indicator diagram more a basis of comparison than a very accurate measurement of power. All I can say is that the mechanical efficiency quoted for the Vickers engine is that taken from the result of a number of shop tests, in which diagrams were taken with ordinary care and with ordinary apparatus, cards

extreme in any particular being discarded as unreliable. The air-starting capacity is 300 cubic feet at 600 lb. per square inch pressure, which, with reasonable skill, would suffice for over 15 starts. Mr. Rabbidge

Sea water is used for the pistons because it entails less space, weight and cost than does fresh water. No trouble from deposit in pistons has been experienced even in the Mississippi and Persian Gulf. The

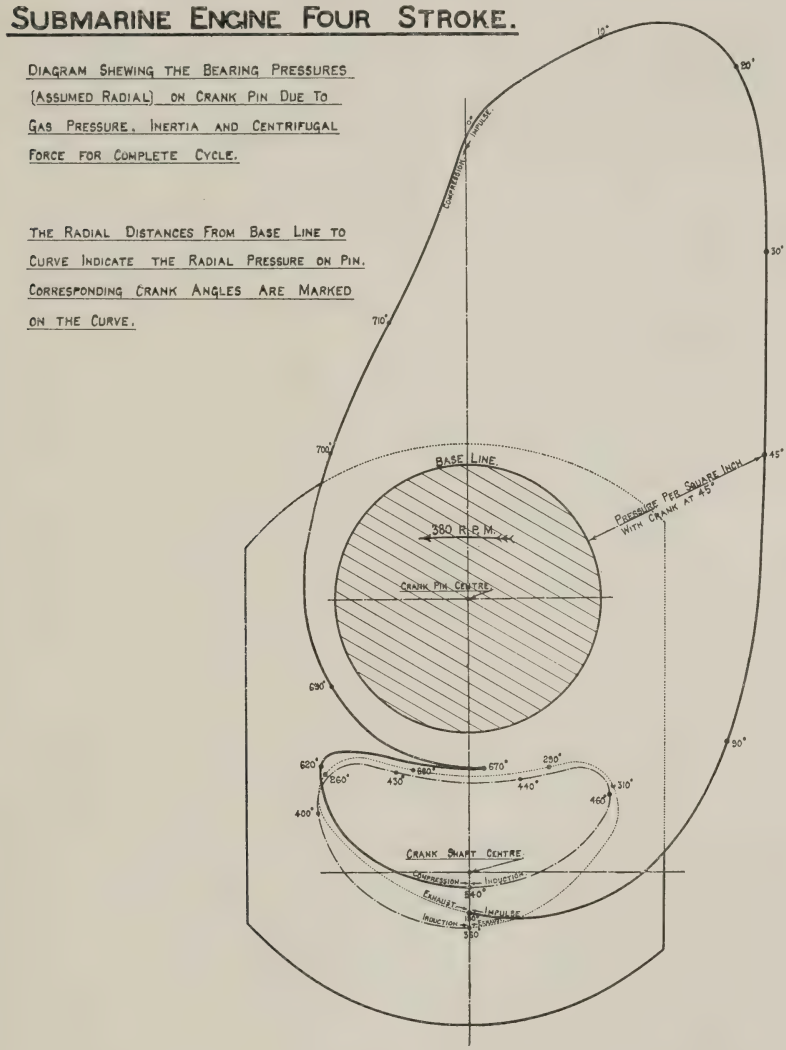

FIG. 6.

reference to air cooling with alloy pistons is not understood. The size of the pistons in the case under consideration would render desirable cooling otherwise than by air.

The lubricating-oil consumption covers all purposes in Vickers' proposal. The actual loss in a forced-lubrication engine depends largely upon the care with which doors and joints are kept tight. Twin-screw

Mr. Rabbidge

ships of 2,500 brake-horsepower have run long voyages on a daily consumption of 11 gallons, about 35 per cent. of which is fed to the pistons or otherwise lost from the main-engine system. On this basis the 11 gallons quoted for the single-screw ship is considered conservative. The diaphragm between the crank-case and the cylinder is regarded as an important factor in reducing the periodical loss through changing the oil due to contamination which will be necessary with engines without this feature.

The question of bearing pressure was one raised. It is a very debatable point whether a comparison of maximum loads at one point is a safe guide unless all the conditions are known and appreciated. The sketch, Fig. 6, showing the variation of load on a trunk type four-stroke engine, may possibly interest many. In the Vickers engine the loads at maximum pressure, corrected for inertia at full speed, and divided by the projected area of the bearing, less oil ways and radii, are as follows:—

Main bearings, 440 lb. per square inch.
Crankpin, 910 lb. per square inch.
Cross-head pin, 1,125 lb. per square inch.

These pressures are much below those in satisfactory use in other oil engines. Comparison with the pressures in ordinary drip-lubrication steam engines is not considered of any particular value.

The Vickers oil tankers have done a good deal of running in canals and confined waters, and from the results it is suggested that slow-speed running received undue attention during the discussion. Experience seems to show that the minimum speeds obtainable from the ordinary four-stroke engine easily meet all practical requirements.

Messrs. Hawthorn Leslie's Reply (By Mr. A. D. Bruce).

Mr. A. D. Bruce

Main Bearing Pressures, Etc.—The main-bearing pressure, taking into account a gas pressure of 500 lb. per square inch alone, equals 460 lb. per square inch, but when taking the inertia force of the reciprocating parts, the centrifugal force of unbalanced revolving masses, and the deadweight into account, it is 315 lb. per square inch. The material is the best Siemens-Martin ingot steel, having a tensile strength of 28 to 32 tons per square inch, and an elongation of 29 to 25 per cent., with bending test of 180 degrees. The shaft is in two portions, each being built-up.

Flat-bottomed Bedplates.—This means that no engine seating has to be supplied by the shipbuilders, but to compensate for the lack of a built-up structure, they have to deepen the double bottoms in way of the machinery space. In the four-stroke six-cylinder engine the balance is particularly good, much better than that of the two-stroke four-cylinder engine, so that in the case of the former no anxiety need be entertained with regard to tank leakage, or as to whether fresh

water would be contaminated if kept immediately under the engine-room tank top. But a flat-bottomed bedplate means an excessively heavy one, because the cast-iron girders under the main bearings have to be sufficiently deep to allow of the proper clearance between the bottom end and the tank top when in the bottom dead centre. The bedplate under these circumstances is far in excess of the dimensions required for strength and rigidity, and has the disadvantage that when quoting engine weights a heavy figure for the engine must be given. The extra material means increased cost, although when considering the ship as a whole the deadweight may not be altered, as what is put into the bedplate is to some extent saved by what is not required for the engine seating. Nevertheless the deepened tank may outweigh any saving expected by the elimination of the engine seating. Mr. A. D. Bruce

Reply to Commdr. Hawkes.—The percentage of sulphur content was given in the case of Diesel oil, where it was stated that it should be 1 per cent. as a maximum. In Werkspoor engines it has been found advisable to use fuels as free from sulphur as possible, and therefore a fuel containing 1 per cent. would be ideal in this respect. It is recognized, however, that fuels generally contain more than this amount, but in any case the Werkspoor engine can use them satisfactorily. With boiler oils more frequent attention must be given to the exhaust valves, which means that boiler oils do affect the valves more than Diesel oils. It may be that the sulphur dioxide, as a result of combustion, forms sulphuric acid when in contact with the condensed steam on the cylinder walls, having a detrimental effect on the liners; but pitting of the exhaust valves could hardly be accounted for in this manner, as the temperature of the exhaust gases passing the valves is too great for sulphuric acid to be formed. It is possible that, when using oils having a high sulphur content, the ill effect on the valves is not due to the sulphur, but due to the free carbon passing the valve, which, when striking the very hot valve head attacks the low carbon steel when carbonization takes place, causing pitting. More free carbon will be present in the exhaust gases when using heavy oils, because of the greater difficulty of obtaining complete combustion.

In view of the non-controversial nature of the discussion, together with what has been said on behalf of the Hawthorn-Werkspoor proposal already, there is little occasion for further remarks in the form of a reply. Since most of the points raised by other speakers relative to this proposal have been previously answered, any further remarks would be additional information about the installation, which is not essential for the purposes of the comparison. It is thought, however, that the Werkspoor engine makes a favourable comparison with the other four-stroke engines contained in the Review, when the question of rating is given due consideration, and that as regards its position relative to the two-stroke proposals, the comparison from a weight-and-space point of view is not so unfavourable as might have been expected by some,

Mr. A. D. Bruce having in mind the rather optimistic claims made at times for the two-stroke type.

Probably the most pertinent question that the Review has yielded, so far as those installations are concerned which have auxiliaries entirely driven by electricity, is: How should the waste heat of the exhaust gases be utilized? Considerable economy can be effected by taking advantage of this heat, especially in the case of the four-stroke proposals, and as the energy from this source is always going to waste, there seems little reason why, in the interests of economy, provision is not made thereby for driving the steering gear and dynamo, and heating the accommodation, etc.

MARINE OIL-ENGINE INSTALLATION AND AUXILIARIES.

By P. BELYAVIN, *Member.*

[Read in Newcastle-upon-Tyne, on the 2nd February, 1923.]

The suggestions I am putting forward in this Paper are purely my own ideas, based on my personal experience with marine Diesel engine installations since 1908. I do not claim that they are ideal from the point of view of convenience; my intention is to suggest the cheapest and most economical installation without making any sacrifices in reliability. I shall welcome any criticism of the suggestions made in this Paper, and if any of them or the resulting discussion brings us an inch forward on the way to industrial recovery, I shall feel gratified.

General considerations.—The total cost and economy of a marine oil-engine installation on board ship depends not only on the cost of the main engine but also on the total cost, arrangement, and number of the auxiliaries. In the first days of the marine oil engine, its reliability was always suspected and for this reason most of the first motorships were twin-screw. A twin-screw installation is, of course, more expensive for the same power, and as the reliability of oil engines now is considered to be unquestionable, a single-screw installation appears to be the first step to be desired on the way to economy. The most experienced oil-engine makers, such, for example, as Burmeister & Wain, have recently produced special long-stroke slow-running engines to suit single-screw motorships. Messrs. Swan, Hunter and Wigham Richardson, who are the builders of Neptune two-stroke oil engines, are, however, the pioneers of long-stroke engines.

Type of Engine.—I shall consider only two, most popular, types of engines, namely, the four-stroke single-acting and the two-stroke single-acting. Both of the above types can be built now with the same margin of reliability.

As the two-stroke engine for the same power can be built at present at about half the weight and half the size of the four-stroke one, with about the same economy in fuel consumption, there appears to be no sound reason why we should adopt the much heavier, larger, and more expensive four-stroke engine in preference to a much lighter, smaller and cheaper two-stroke one.

The number of cylinders.—Six or four is the usual number of cylinders in a two-stroke engine; a four-cylinder engine for moderate

power will be shorter and cheaper because of the smaller number of parts; for the same piston speed its number of revolutions per minute will be less, which will suit a single-screw ship better. Therefore, up to a certain limit of power, a four-cylinder engine should be recommended.

Thus, with regard to main-engine arrangement, we arrive at the following conclusion:—The economical motorship should have a single-screw, and a two-stroke four-cylinder oil engine.

At present, of many existing types of oil engines some are very complicated, the others heavy and bulky. We can reasonably expect that in the near future some of the existing types will come to the shape suggested in Fig. 1.

D = 30 inches. S = 60 inches. R.P.M. = 80. P_m = 100 lb. per square inch (maximum). I.H.P. = 3,500.

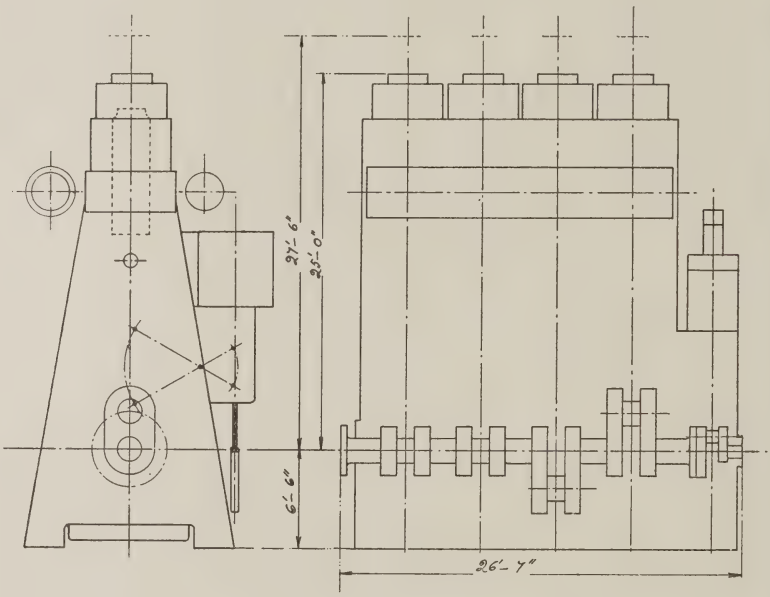

FIG. 1.

Weight, about 320 tons, with all pumps, compressor, and fly-wheel on, but without the thrust block.

In Fig. 1 the outlines of the crank-shaft and levers are shown to avoid a possible accusation of undue optimism.

The crank-shaft works out to be of the size similar to half of the crank-shaft with four cranks of the successful four-stroke eight-cylinder engines of the design of Burmeister and Wain. The shaft horse-power of the above engine works out at about b.h.p. = 2,500, or about 3,000 i.h.p. steam indicated. This engine will suit the motorship of about the following proportions:—Length, 445 feet, breadth, 55 feet, d.w. capacity, 9,400 tons. The engine suggested is probably of the largest practicable size for a four-cylinder engine with the crank-shaft made in one piece.

Let us now consider the question of one or other way of driving all the auxiliaries required by the engine, or for general use. The most important engine auxiliaries are the following :—

Scavenging pumps, compressor, auxiliary compressor, cooling-water pumps for the cylinders, pistons, etc., auxiliary water pumps for the cylinders and pistons, lubricating oil pumps, auxiliary lubricating oil pump, fuel-oil pump, waste-heat boiler.

The most important ship auxiliaries are :—

Electric generating sets, steering gear, winches, windlass, general-service pumps, etc.

The most frequent practice at present is to drive all the auxiliaries, including the engine-room auxiliaries, electrically. This is, of course, a very sound proposition; nothing is more comfortable than electric machinery in working. However, from the point of view of economy, this is in some cases one of the most extravagant means of driving such auxiliaries, which should usually be driven off the main engine. Where all the ship auxiliaries, such as steering gear, winches, windlass, etc., are driven electrically, it is often suggested that the engine-room auxiliaries should be driven in that way also, the required current being available on the voyage as the winches are not working. A closer consideration of this question will show that it is not such a simple proposition as it appears, and will in some cases involve considerable additional expenses. The reason for this is that every engine, although safe and reliable, from time to time will require some attention and overhaul. If the electric generating sets have to run continuously at sea as well as in port, a stand-by generating set will most certainly be required to run while the other one is being overhauled. The main engine is from time to time requiring some attention in port when not running, and a quick-running engine, such as those used to drive the dynamos, will require more attention and overhaul than the slow-running main engine, in proportion to the increased speed of running.

There were sometimes too many stand-bys for the auxiliaries; they could very often be dispensed with. It has to be remembered that the main engine has no stand-by in single-screw ships; however, nobody has any fear that it will fail in case of necessity.

From the above considerations it is apparent that for the sake of economy and simplicity we should not drive the auxiliaries required on voyage by the same means as the ship auxiliaries required in port. This means that the engine-room auxiliaries and the auxiliaries required at sea should be driven off the main engine to avoid the necessity of fitting a special stand-by generating set. Of course, this is practicable only to a certain limit of power; should a very large power be required, it might be advisable to drive all the auxiliaries from a separate engine, or electrically, but this, of course, is an extreme case. On the above lines we can now consider the whole installation for the above-mentioned engine and ship in detail.

(1) *Scavenging pumps.*—In most cases the scavenging pumps are driven by the main engine; sometimes off the crank-shaft and sometimes by levers working off the cross-head. In the first case, the engine becomes longer; in the second, slightly wider. The length of the engine should be considered of more importance than the width because of its direct influence on the length of the engine-room and thus on the cargo capacity. I should certainly recommend that the scavenging pumps be driven by levers.

There are two ways of arranging the scavenging pumps in the latter case:—(1) above the levers, (2) below the levers. The first method evidently has an advantage as the width of the engine is being increased less and the water pumps can be driven off the same levers; with the pumps arranged below the levers they take a considerable part of the floor space, and substantially increase the width of the engine.

Let us consider now the case of separately-driven scavenging pumps. As a slow-running marine oil engine requires only a very small pressure of scavenging air, the reciprocating scavenging pumps could easily be replaced by turbo-blowers. However, as the scavenging pump often represents about 6 or 7 per cent. of the total power of the engine, it will always mean a substantial auxiliary Diesel engine and a corresponding increase in cost of the installation; in addition, it will mean a certain increase in repair bills. I do not altogether reject this idea; above certain powers we shall most decidedly have to drive all the engine auxiliaries separately in order to relieve the main engine of its burden, and to enable us to get more power on the propeller shafts; but it appears to me to be very extravagant to do so with engines of moderate size and power. It always means an extra generating set of considerable power, more overhaul, more space in the engineroom taken by the auxiliaries, more complication, more trouble and more repairs. As stated above, the generating set required to drive the auxiliaries at sea cannot be the same as that which drives the ship auxiliaries while in port, and a stand-by will be required.

For marine oil engines of very high power an electrically driven turbo-blower is, of course, a very attractive proposition. As a very good example of existing installations of turbo-blowers for two-stroke engines, I would mention Messrs. Sulzer's latest installations in the motorships "Handicap" and "Camranh," particulars of which were published some time ago, although the power of their main engines is perhaps not big enough to make the turbo-blowers a necessity.

Returning to the scavenging pumps driven off the main engine, I wish to draw your attention to the design of levers and guides for the pumps. I have heard of several cases in which the levers driving scavenging pumps or compressors have not worked satisfactorily. I investigated some of these cases and a wrong design of levers appeared to be responsible for the trouble. In the usual arrangement on steam engines, the plungers of several pumps driven by the levers appear to be

a sufficiently strong guide to take all the side loads which may occur, and a single extra guide on top of the crosshead is perhaps all that is required, considering the moderate load.

If levers are used to drive a compressor or a scavenging pump with a single guide in the centre as in the previous case, the load may be very much greater, whereas the water pump plungers which were helping to take the side load may either be insufficiently rigid, or be driven by the other pair of levers. In such a case any occasional difference in length of links connecting the levers to the crosshead (Fig. 2), a greater wear on one of the links, etc., will produce a horizontal

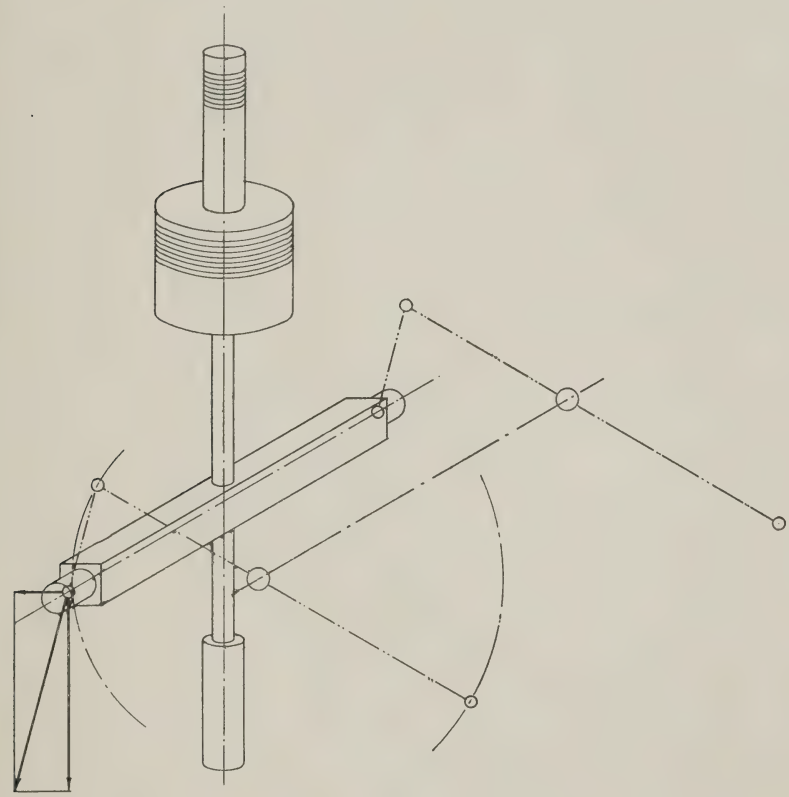

Fig. 2.

twisting moment, which will twist the guide and crosshead and cause an excessive wear on all the bearings. The correct design would be to have a reliable guide at each end of the crosshead to eliminate the possibility of free horizontal forces and twisting arising.

Another point of importance is the type of valves that should be recommended for the scavenging pumps. As the pressure used for scavenging is very small—usually not more than 1·5 lb. per sq. inch—every increase in suction or delivery pressure in the pump affects the mechanical efficiency of the engine. Therefore, it is advisable to use the valves which can give the suction and delivery process in the pump as

close as possible to the theoretical; it can be achieved only by very light automatic valves with fairly large area. Besides, if mechanically operated valves were used, a special reversing gear would have to be provided, which is very often a serious disadvantage. Thus, for the most economical installation of moderate power, the scavenging pumps should be driven off the main engine, preferably by levers.

(2) *Compressors.*—I suppose the most important problems of Diesel engine compressors are at present fairly well studied. I shall, therefore, mention only a few points, referring more to the problem of economy. The question on which opinion appears to be divided is the capacity of the auxiliary compressor. Considering that a compressor requires fairly great power to drive, from the point of view of economy the capacity of the auxiliary compressor is a very important factor. Roughly, the compressor on the engine absorbs about 6 or 7 per cent. of the engine horse-power. For 3,500 i.h.p. as suggested in our example, the horse-power of the compressor would be about 210. Thus, if an auxiliary compressor were made the same size as the main compressor on the engine, it would require a special engine of substantial horse-power to drive it.

Lloyd's Rules require an auxiliary compressor to be fitted, although it is not stated what its capacity should be. This appears to be a very wise rule, as it leaves the shipowner with considerable scope for choice. From my personal experience with marine Diesel engine installations, the large auxiliary compressor does not appear to be an unquestionable necessity, except for charging the bottles for the first start. Of the existing installations, there are some with large auxiliary compressors, some with small, and some with none at all, except a small emergency compressor. I think an emergency compressor is desirable.

The capacity of starting and injection air bottles should be absolutely sufficient to start the engine as many times as the possible maximum number of manœuvres in the worst possible conditions might be. Although it is somewhat difficult to estimate the possible number of manœuvres required, usually it is supposed to be sufficient to have enough air for twenty starts for each engine. I should think it must be more than that for a single-screw ship.

The most important point referring to the consumption of air and replenishing of air supply is the adjustable lift of the fuel valves; I think this is more important than a large auxiliary compressor. The compressor on the main engine is usually made big enough to supply some extra air required for filling the bottles, and in most cases this is sufficient if the adjustment of the fuel-valve lift is provided for. Thus a little improvement in the valve gear will save a big auxiliary compressor, and is certainly very important from the point of view of economy. The adjustment of the fuel-valve lift should be so arranged that it would always correspond to the amount of fuel injected in the cylinder, *i.e.*, the fuel-valve lift and the fuel pump must be regulated by the same gear simultaneously and automatically. I am afraid that

with solid injection a more substantial auxiliary compressor will be required.

The main compressor can be driven either from the crankshaft and arranged on the forward end of the engine, or can be driven by levers; in the latter case, of course, two guides should be provided for as already mentioned. The compressors for moderate-power engines can be of three stages, but for larger powers the compressor, if convenient, should have four stages of compression. The reason for this is that in the high-pressure stage the oxidizing effect of the highly compressed air on the discharge valves is so serious with higher temperatures that in large compressors it will pay to have an extra stage of compression to reduce the temperature in the high-pressure stage to the minimum. Of course, at present, the design of compressor should be considered sufficiently studied to make it as safe and reliable as the main engine.

Oil and Water Pumps.—The main pumps should be driven by the levers off the crosshead, but small independent pumps should be provided to supply the lubricating oil to the bearings, or water to the jackets while the main engine is not running. No other stand-by pumps should be required, but the pumps on the main engine should be divided into several units so as to be, if possible, all of the same dimensions and interchangeable. All the valves should be standardized, and if possible made interchangeable.

Waste-heat boiler.—There is no other part in a marine oil engine installation that has been so miscalculated and misjudged as the waste-heat boiler. Several of the early installations were supplied with waste-heat boilers, but they were never fitted in later ones. The impression was given that waste-heat boilers were a failure. Information on this subject available is rather scarce. Judging from such little data as I have been able to obtain, it appears to me that rather more was expected from these boilers than they could give. Although the exhaust gases carry away about 30 per cent. of the total heat of combustion, the amount of heat that can be extracted and used for steam production is very much less, especially in two-stroke engines, where the exhaust gas is diluted with scavenging air and has a much lower temperature. This is apparent from the following rough estimate:—

If the exhaust gas of a two-stroke engine is being utilized in a waste-heat boiler for producing steam so that the temperature of the gas leaving the boiler would be $t°$ higher than the temperature of hot water in the boiler, the amount of heat as a percentage of the total carried by the exhaust gas will be roughly:—

If T_1 is the temperature of exhaust gas
T_2 the temperature of water or steam
H_1 the percentage of the heat utilized in the boiler of the total carried by exhaust gas
η the efficiency of the boiler

$$H_1 = \frac{T_1 - (T_2 + t)}{T_1}\eta \qquad . \quad . \quad . \quad . \quad . \qquad (1)$$

If the steam pressure raised in the boiler was about 60 lb. per sq. inch

T_2 would be about 151° C. and assuming the temperature of exhaust gas for a two-stroke engine $T_1 = 290°$ C.

η about 90-95 per cent., and

$$H_1 = \frac{290 - (151 + 50)}{290}\eta = 0{\cdot}273, \text{ or}$$

about 27·3 per cent.

If the total amount of heat carried by the exhaust gas is $h = 0{\cdot}30$ of the total heat of burnt fuel, the total amount of heat available for raising steam will be $H = 27{\cdot}3 \times 0{\cdot}30 = 8{\cdot}2$ per cent. The quantity of steam that could be raised in a waste-heat boiler can be roughly calculated in the following manner :—

If we call the total amount of heat required to evaporate one kilogram of water of the initial temperature $T°_3$ C. at the pressure of p kilogram per sq. cent. h_1 Cal. per 1 kilogram, which roughly equals

$$h_1 = \lambda - g_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where λ Cal. the total amount of heat required to evaporate 1 kilogram of water of the initial $t° = 0°$ C., and g_0 Cal. is the heat contained in the water of the temperature T_3. If the fuel consumption of the engine per b.h.p. hour is, say, $g = 0{\cdot}195$ kilograms, and the calorific value of the fuel, say, $Q = 10{,}000$ Cal. per kilogram, the total amount of heat developed per b.h.p. hour will be

$$H_2 = g \,.\, Q \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The amount of heat which can be used for raising steam is $H_3 = H_2 \times h \times H_1$ or, substituting the value of H_1 and H_2 from the equations 1 and 3.

$$H_3 = \frac{g \times Q \times h \times [T_1 - (T_2 + t)]}{T_1}\eta \text{ Cal.} \quad . \quad . \quad (4)$$

Thus, the amount of steam per b.h.p. per hour equals—

$$W = \frac{g \times Q \times h \times [T_1 - (T_2 + t)]}{h_1 \times T_1}\eta$$

$$= \frac{g \times Q \times h \times [T_1 - (T_2 + t)]}{(\lambda - g_0)T_1}\eta \text{ kilograms} \quad . \quad (5)$$

Fig. 3 gives the curves for W for various T_1, T_2 and p, assuming $h = 0{\cdot}30$, $g = 0{\cdot}195$ kilograms per hour, $g_0 = 15$ Cal., $t = 50°$ C. and $\eta = 0{\cdot}90$, which are, of course, very conservative figures. The boiler efficiency here represents really only radiation and some possible small losses, as all the other factors affecting the steam production in the waste-heat boiler are already taken into account. From Fig. 3 it is apparent that a lower pressure in the boiler is advantageous, and the higher temperature of the exhaust gas is essential. Thus the four-stroke engine is more suitable for working with a waste-heat boiler than a two-stroke one, which has lower temperature of the exhaust gases.

In the example of an engine mentioned previously with about 2,500 b.h.p., the total amount of steam at 60 lb. per sq. inch pressure per hour with $g_0 = 15$, $T_1 = 275$, and $t = 40$ would be about $W = 1{,}500$ lb. Of course, this is not sufficient for driving all the auxiliaries or any similar use, but for heating, cooking, some smaller pumps, dynamo, etc., this amount of steam always required on board ship may prove very useful. As a

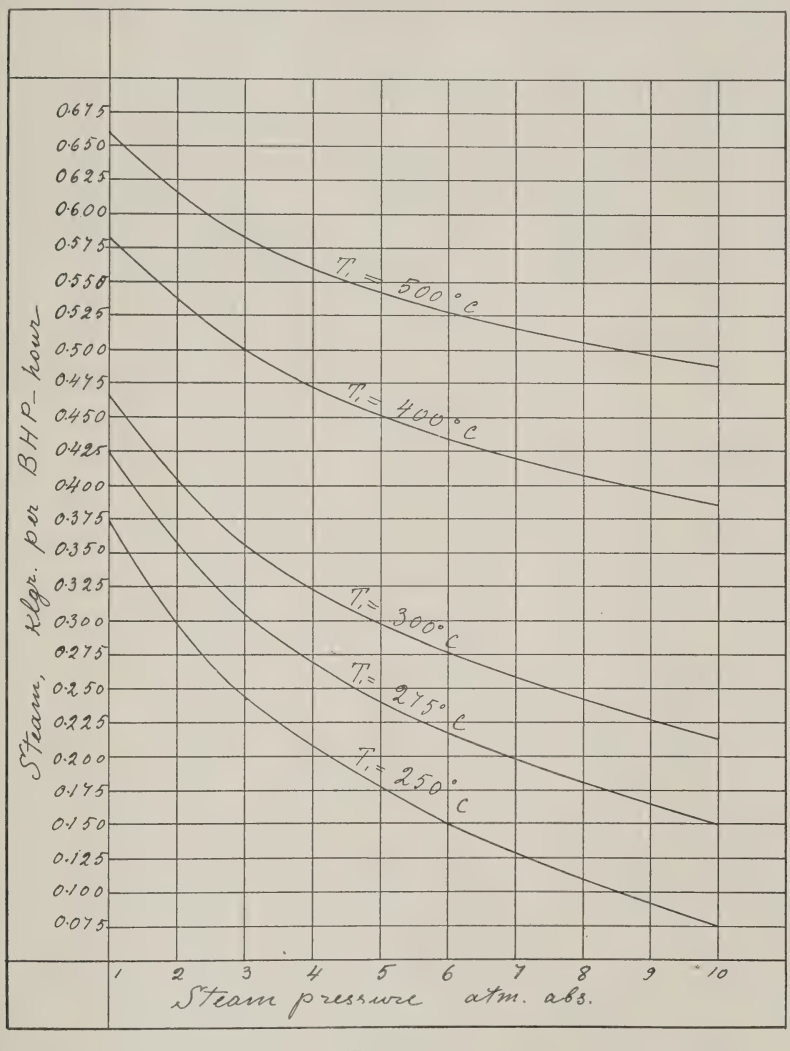

FIG. 3.

waste-heat boiler cannot cost very much and at the same time can always be adapted to act as a silencer, which is indispensable, I see no reason why we should not return to waste-heat boilers in certain cases where steam in quantities as mentioned above is required.

In some cases an additional oil burner in the boiler was recommended in order to assist the steam production in case of necessity. In the waste-heat boiler, as suggested above, the exhaust gas has still a

fairly high temperature when leaving the boiler and could be again utilized in an economizer for heating the feed water. However, I leave the question of whether this additional complication is desirable, open to discussion. As the conditions for the utilization of heat in waste-heat boilers are rather unusual, a special boiler should be designed to suit the purpose. For the sake of economy, the boiler should also act as a silencer. The two conditions which should be fulfilled in the boiler are somewhat contradictory; to get a high engine efficiency the boiler should offer the least possible resistance to the exhaust gases; to get a high boiler efficiency the speed of the gases through the boiler tubes should not be below a certain figure. To make the above conditions agree, I would suggest the following points:—(1) The direction of the gas flow in the boiler should not be changed; (2) the changes in the area of the passages in the boiler should be as little as possible.

Auxiliaries.	At Sea kw.	In Port kw.		Manœuvring kw.
		Day.	Night.	
Windlass	—	—	—	25
Steering gear	15	—	—	15
Lighting	5	1	5	5
Various pumps	—	—	—	—
Sanitary donkey	5	5	5	5
Auxiliary compressor ...	—	—	—	15
Winches	—	65	—	—
Total kw.	25	71	10	65

Fig. 4.

Electric generating sets.—If the oil-engine electric generators are used to work various ship and engineroom auxiliaries, the most important point is to get a very accurate estimate of the loads required under all possible conditions. The power required to drive electric auxiliaries can be estimated very accurately, which, of course, is clear from the way they work. Steam auxiliaries are a much more difficult proposition in that respect and I doubt very much whether any figures available for this calculation can be considered reliable. If even the steam consumption of various steam auxiliaries were known, many losses of steam, such as condensation in the long pipes, leakages, etc., can hardly be estimated with sufficient accuracy. With electric current, of

course, all these losses do not exist if the insulation, etc., are well-made, and for this reason I am sure electric ship auxiliaries will always be more economical in the long run than steam ones, even in steamships. Therefore, I would recommend an oil-driven electric generator as an economical proposition for driving all electric auxiliaries in a steamship. I had an opportunity of experience on seagoing ships with total power of electrical auxiliaries of over 4,000 b.h.p. on each and I can assure you no trouble was ever experienced with the electrical part of the installation. Half the generating sets were driven by Diesel engines and these were the only part of the installation that gave trouble. Of course, since then we have learned much and all the difficulties with quick-running oil engines are overcome. To estimate as accurately as possible the power required for electric auxiliaries under various conditions, I would recommend the preparation of a table such as is shown in Fig. 4. This table is only a specimen and it can be made for any number and kind of auxiliaries, either steam or electric, and for many and various cases. The table must show the total power required on various occasions and with its assistance we can estimate the size of the generators or boilers to suit our requirements best.

The generating sets form a much more intricate and important question than is often thought. The first Diesel-driven generators I had experience with were fitted with four-stroke, four- and six-cylinder, quick-running engines; the dynamo was coupled to the engine by means of a flexible coupling. The flexible coupling was introduced to allow for any deficiency in alignment because of the special conditions on board ship. Our first surprise was that the flexible coupling did not work satisfactorily. Careful investigations showed that the variations of speed between the electric generator, running at constant speed and constant turning moment, and the engine, running with varying acceleration, produced undesirable loads, having a detrimental effect on the flexible coupling. To overcome this difficulty the weight of the flywheel was substantially increased and this considerably increased the life of the flexible coupling. After running for some time the trouble started again, this time accompanied by heating of the dynamo bearing next to the coupling. The problem was once more carefully looked into and it was found that the flexible coupling was really perfectly useless. It might be all right if the two shafts were in line, but as soon as the alignment was, for one or other reason, spoiled, the trouble was exactly the same as if there were no flexible coupling at all. The most satisfactory result with regard to alignment was obtained by coupling the generator direct to the engine, without any intermediate bearings, as shown in Fig. 6. One of the most important points in the above arrangement is that the dynamo and the engine should have a common rigid base.

Another serious difficulty often met in quick-running generating sets is the torsional vibration of the shaft. It happens most frequently in the crank-shaft and its effect is very detrimental and even dangerous for

the engine. It produces a terrific noise and hammering in the whole engine; every part appears to vibrate with tremendous force as soon as the engine is coming into critical speed. This effect immediately dies away when the revolutions are altered. Sometimes the governor ceases to work during the critical period and I remember a case in which an engine could not come off the critical revolution without hand regulation.

The critical period can be altered by adding counterweight to the cranks. Very often the critical period coincides with the designed revolution. This is a very had case and to save the designers this trouble

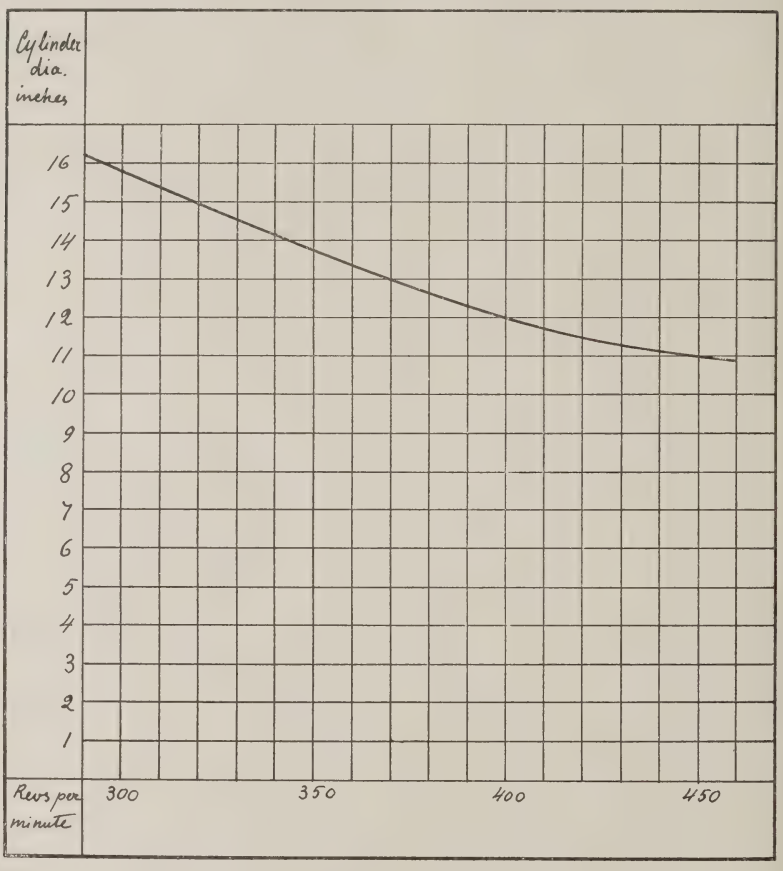

FIG. 5.

I give in Fig. 5 a curve, based on my own experience, showing the dangerous revolutions for various cylinder diameters for four-stroke quick-running six-cylinder engines of a common moderately light design, and with a stroke-to-bore ratio of about 1·1. I think it would greatly benefit the whole oil-engine industry if the various designers with some experience in torsional vibrations would publish similar diagrams for their engines with different numbers of cylinders and revolutions I would recommend that the normal revolution of the engine, when designing, be kept at least 10 per cent. below the probable critical speed.

The question now arises: What would be the ideal oil engine for a generating set? I am afraid there are no oil engines yet on the market that could be called ideal for that purpose, at least as I understand it. I think the auxiliary engine should be as simple as it is possible to make it, and should have the minimum of working parts requiring overhauling. A brief specification of an engine which in my opinion would be most suitable for driving a generator is as follows:—The engine should be of two-stroke port scavenging type, coupled directly to a generator; scavenging air supplied from a separate scavenging pump, driven off a crank on the forward end, with light automatic valves with small lift, forced lubrication for every moving part on the engine, mechanical injection by means of timed fuel pumps and automatic fuel valves in the cylinder; no special hot bulb, but as the pistons are not cooled the temperature in the cylinder should be sufficient for good combustion; compression pressure to be between 400 and 500 lb. per sq. inch, which is sufficient for starting from cold without any special arrangements.

The little complication in having a separate scavenging pump instead of using the crank case as a scavenging pump is, in my opinion, amply justified by the following advantages gained. A separate scavenging pump gives an adequate supply of air to the cylinders and ensures good scavenging and combustion even at higher mean pressures; all the working parts in the crank-case are much more accessible if the crank-chamber is not used as a scavenging pump; the lubricating-oil consumption will be greatly reduced with the usual forced lubrication, which cannot be used if the crank-case is acting as a scavenging pump; in the latter case the oil is supplied to each bearing with a separate adjustable pump and the lubricating-oil consumption would be greater and the engine would require more attention. For the sake of simplicity, the engine should have not more than two cylinders, which with the two-stroke engine will correspond to four cylinders in a four-stroke one with regard to turning moment.

The most important and difficult question in the above engine is the running of pistons without cracking. This problem can only be solved with the experimental data for the pistons and a correct design of the piston top. Non-cooled pistons usually fail not only due to heat stresses because of the temperature difference, but also because of the cast-iron being subjected to considerable alternating stresses when in overheated condition.

The life of the piston in the engine running continuously depends chiefly on the average temperature to which it is subjected. Thus, if in a two-stroke engine we have the same average temperature of the working process as is the case in a four-stroke engine, we shall, in all probability, get exactly the same average temperature of the piston as in a four-stroke engine of approximately the same dimensions.

If the generating set has to supply the electric current for the winches, it must be designed to supply the maximum current required, which

fluctuates very considerably. Therefore, if the generator is driven by a four-stroke engine the latter must be designed with substantial reserve of power, thus being much heavier than it should be for the average load. If we design a two-stroke engine to run at a very low mean pressure, say, 50 lb. per sq. inch, the mean temperature of the working process and thus the average temperature of the piston will be much the same as in a four-stroke one running at about 100 lb. per sq. inch mean pressure, with moderate piston speed.

With a piston speed of about 800-850 feet per minute, a four-stroke engine piston of correct design will be perfectly safe without cooling up to 16 inches cylinder diameter; in a two-stroke engine of the same proportions and piston speed the average mean pressure will have to be about 50 lb. per sq. inch to get similar mean temperature, but the power developed

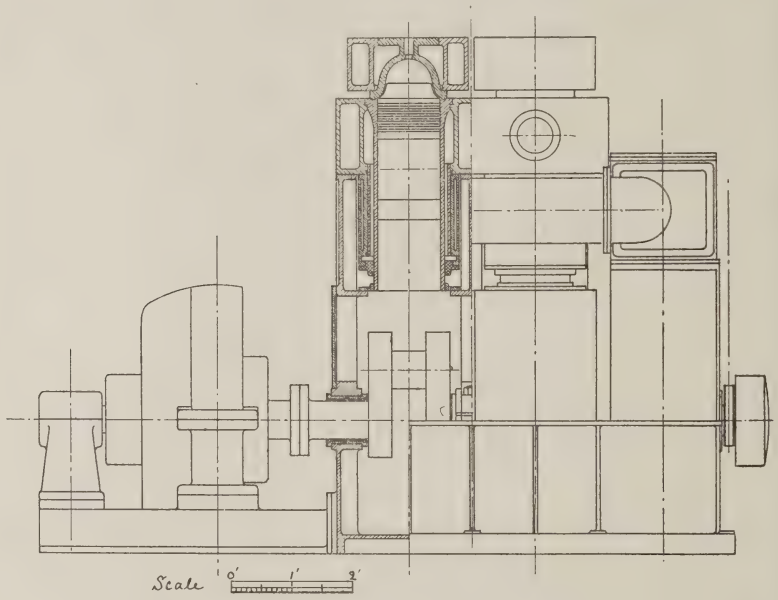

Fig. 6.

will be exactly the same as in the four-stroke engine. It means that a two-stroke engine of the same cylinder dimensions and revolutions as a four-stroke one will be capable of more than 100 per cent. overload. Thus, it need not be designed for maximum load required to work the winches, but only for the average.

Occasional short overloads are by no means dangerous, as the temperature of the piston does not increase instantly but comparatively slowly, especially if immediately after the overload the power drops below the average; and as soon as the average load which is considered to be safe is not exceeded, the engine will be perfectly safe and reliable.

Taking into account the above considerations we will arrive at the following conclusions: A two-stroke oil engine driving an electric generator supplying the current to ship auxiliaries should be designed to

run at about one half of a safe mean pressure of that in a four-stroke engine with the same piston speed and cylinder dimensions. It should be designed for the average load and must stand an overload of more than 100 per cent. The engine in Fig. 6 is designed to the above suggestion and is suitable for installation in a ship mentioned above. It is designed to develop normally about 75 kw. at 250 revolutions per minute and about 50 lb. per sq. inch mean pressure continuously. Thus, a generating set of this description can be used in place of two 75 kw. sets in case of alternating load, which is, of course, an enormous advantage. It will be a substantial saving on the whole installation and a two-stroke engine to the above specification will be much simpler and require less overhauling and less repairs than a four-stroke one of the same proportions.

Although an engine as above suggested appears to be the limit of simplicity, its design is by no means an easy problem and very little is required to make it either a complete success or failure. The idea of an engine of this kind always appeared to me very attractive and, having this in view, I carried out some experimental work which made it possible for me to put forward the above suggestion as thoroughly reliable.

I think mechanical fuel injection with timed fuel pumps and automatic injection valves would be perfectly satisfactory for an engine of this description, as it will not be running continuously, but only when the winches are required, and in the case of imperfect combustion with heavy grades of fuel, the engine can be easily cleaned from time to time. Mechanical injection with automatic fuel valves has a considerable advantage with regard to regulating. With air injection the regulation of the engine is a very difficult problem, as the injection air and the lift of the fuel valve must be adjusted automatically to give satisfactory results.

The current required to run all the auxiliaries on voyage must be supplied by a special small generator. In the installation shown as an example below, the current on voyage is required for the steering gear, some small pumps, and lighting—total, not more than 25 kw. To save all the trouble of running an auxiliary generating set, I would suggest that the generator be driven by a chain from the propeller shaft. I shall not deal with this arrangement in detail, but I am sure that electrical engineers can make it a practical proposition. However, for ship lighting and running the auxiliary pumps when the ship is in port or when it is manœuvring, a small generating set of the same capacity about 25 kw. will be required. I suggest one with a single cylinder the same as in the larger set but with the crank-case used as a scavenging pump.

For the above-mentioned installation the following generating sets are suggested :—One oil-engine set, 75/150 kw.; one oil-engine-driven set, 25 kw.; one driven by chain off the propeller shaft, 25 kw. The 75 kw. set is specially fitted to work the winches when in port. The 25 kw. chain-driven to be used on voyage. The 25 kw. oil-engine-driven, when manœuvring and in port when the winches are not working.

Any of the above sets can be used as a stand-by on voyage.

If we compare the installation as shown in Fig. 7 with most of the already existing installations of the same kind, we can see that it is undoubtedly much simpler and most certainly will be cheaper; whereas nothing is sacrificed with regard to efficiency and reliability. Fig. 7 shows that main engine, 75 kw. generating set, 25 kw. generating set, 25 kw. chain-driven generator, and an emergency compressor. Considerable space is left for various pumps which are, of course, of secondary consideration and can be made to suit the requirements, as they have no material influence on the principal points in the suggested installation. The starting-air tanks shown are of sufficient capacity for about 40 or 50 starts.

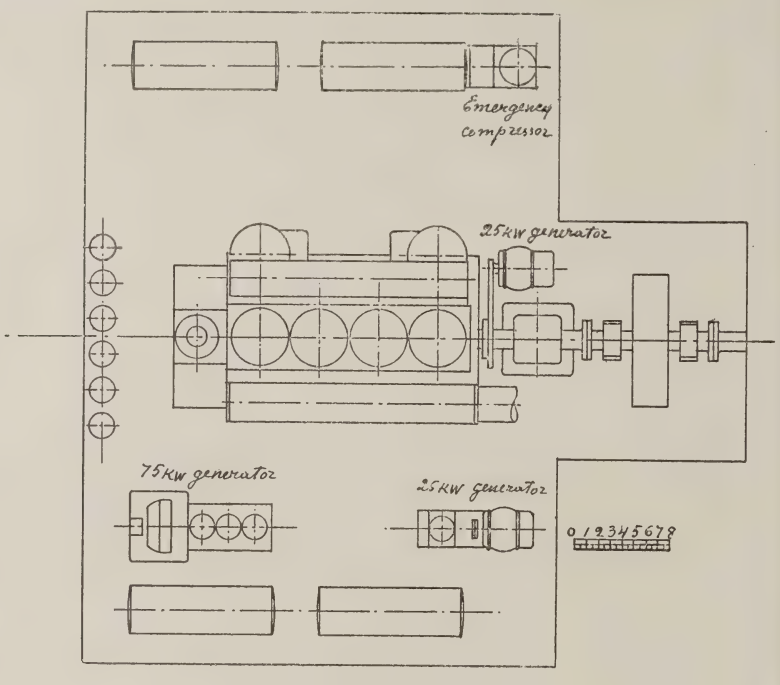

Fig. 7.

Direct or electric drive.—The first Diesel-engine installations of substantial power on board ship were with electric transmission of power, as at that time there was no reliable method of reversing known. Since then I have had experience with several electro-transmission installations and always found them most comfortable and reliable. However, electric transmission has its own weak points. The first of these is its higher cost. Although quick-running engines are lighter and smaller, for larger powers they are bound to have a greater number of working cylinders. The machining must be much more accurate and the materials used of much higher quality. Thus, the cost of the engines should not be any less if built to the same relative quality standard if compared with the economical direct-driven installation.

The electric part will be an additional expense and complication to those of the engine. Besides, quick-running engines require considerably more attention, overhauling, and repairs than slow-running engines which, of course, depend on the condition of working.

In certain cases, for instance, if exceptional manœuvring qualities are required, electric transmission is invaluable. For ordinary cargo ships, however, it is too complicated, too expensive and too fragile. It is also less economical, as a considerable percentage of power is lost in transmission. Direct-drive is certainly a much simpler proposition for a conventional motorship.

Conclusion.—The above outline of a Diesel-engine installation should be regarded not so much as an ideal from the mechanical point of view, as the simplest and cheapest that could be thought of and one which is very economical in running. The single-screw four-cylinder engine installation having the minimum number of working parts should be considerably cheaper than the twin-screw, or six-cylinder set. With regard to facilities of starting and manœuvring, a four-cylinder two-stroke engine should correspond to an eight-cylinder four-stroke engine; as at present there are many single-screw installations with six-cylinder four-stroke engines, there should be no question as to its manœuvring qualities.

The absence of a special stand-by generator is no more risky than a single-screw engine. I am certain there are many ships with only one steam generating set; why should there be two for a reliable oil engine? This is certainly a prejudice left over from the time when an oil engine was regarded more as an experiment. This position created an extravagant habit of having stand-bys for every part of the machinery, no matter how unimportant it was.

I think it would always be more advisable to fit one reliable engine than two unreliable ones. However, I must add again that the installation described is purely my own suggestion and I do not know whether any shipbuilders will be agreeable to undertake the difficult task of building one to the suggested specification.

Further reduction in the cost of oil engines can be achieved only by standardization of engines. No other engine is so suitable for standardization as an oil engine, having as it does numerous identical parts and the additional advantage that it can be built in combinations of various numbers of cylinders for different powers and revolutions.

All the above problems cannot be solved by shipbuilders or shipowners independently. They should be discussed jointly and no doubt the cost of the motorship will be brought within the reach of every shipowner. Doubtless both shipbuilders and shipowners would welcome such a possibility.

In conclusion, I must thank The Metropolitan Vickers Electrical Company, Limited, for the very valuable information they have kindly supplied me with in regard to the electrical part of the installation, and the sketches of dynamos shown coupled with oil engines on Figs. 6 and 7.

DISCUSSION.

Mr. Thomson

THE CHAIRMAN, Mr. Harold Thomson, *Vice-President*: I think you will agree with me that we have had a paper which is full of all kinds of subjects which call for discussion. Mr. Belyavin starts off with the well-known subject of discussion—Which is the better, the two- or the four-stroke cycle engine? Some of you may be getting a little tired of that phase of the Diesel-engine controversy, but no doubt there will be some four-stroke-cycle people present to-night who will desire to show that the four-stroke engine is as good as the two-stroke proposed by Mr. Belyavin.

Then there is the question of the desirability of fitting steam auxiliaries or electric auxiliaries. It is a question which I think it is sometimes rather difficult to decide upon, but in my opinion it is one to be largely determined by the type of vessel and the kind of trade in which it is going to be engaged. For ordinary cargo tramp steamers I am inclined to favour steam-driven auxiliaries. Steam winches and machinery of that kind can be overhauled and repaired in any part of the world and it is not necessary to carry an electrician, and there are many other advantages in steam-driven auxiliaries for that type of vessel. In other types of vessels, electric auxiliaries have the advantage. A great disadvantage of electric auxiliaries is the first cost, for generators of a very considerable size have to be provided to cope with the load required even in a steamer of moderate size.

The subject of scavenging is most important, and I agree in general with what the Author says; but I am not quite sure about driving the pumps by levers. I know one superintendent engineer who will not have levers under any circumstances whatever, and it would be very difficult to persuade him to accept scavenge pumps driven in this way. Beyond a certain size, in the case of the particular engine with which I am most familiar, we adopt turbo-blowers, as suggested by the Author. Possibly the engines of the "Handicap" are rather on the small side for turbo-blowers but for large powers there is a considerable advantage in having turbo-blowers. They occupy less space and in many ways are more convenient than large scavenging pumps. I agree with the Author with regard to the scavenging valves suggested, and valves of this type are in wide use to-day.

The Author's remarks on the subject of Diesel-electric drive are very interesting. It happens that I was recently going into the question of the design of a vessel of very high power, and I came to the conclusion that a Diesel-electric drive was impracticable and that there was no advantage at all in this case to drive the ship by motors rather than drive the propeller direct by Diesel engines.

The steam turbo-electric drive may have many advantages, but the Diesel-electric drive is a different question altogether in my opinion, except for very special purposes. I think there are ample grounds for discussion, and I hope we shall have a very good debate on this excellent Paper.

Mr. A. D. Bruce

Mr. A. D. BRUCE, *Member*: Mr. Belyavin has presented to the Institution a Paper of more than ordinary interest as it deals with practical problems that many firms have to handle frequently. Few firms have collected much data in connexion with the machinery of motorships, but the firms, collectively, have a considerable experience to go on; hence the value of such a Paper at this time.

In selecting a Diesel direct drive instead of a Diesel electric drive, the Author has made a wise choice. The experiences with the electric drive are conflicting and little reliable information is to hand as to its satisfactory working. There are, however, possibilities in the scheme especially for high powers where a greater number of units can be utilized than is possible with the direct drive; therefore, the size of cylinder necessary is smaller. The starting plant would be greatly reduced and simplified, on the lines of the equipment for a land installation, the reversing gear dispensed with, and the propeller designed to give the highest efficiency.

Accepting the direct drive as the more suitable, the question of type of engine presents itself and the Author has chosen a two-stroke engine which certainly makes a simple, compact, and cheap arrangement. Nevertheless, a twin-screw four-stroke plant would make a suitable alternative, having an engine-weight of 420 tons as against 360 tons, giving an extra weight of 12 per cent., but with auxiliaries the extra would only be about 6 per cent.

Referring to the plant set forth by the Author, it is at once seen to be as simple as possible and so long as the working of the machinery is normal, eminently satisfactory; at the same time I think it is desirable that in addition to an emergency compressor, there should be an auxiliary compressor fitted to provide manœuvring and blast air, and—being installed in a single-screw ship—in case the main-engine air compressor broke down at any time, to enable the ship to proceed, by supplying the necessary blast air, etc. Although air compressors are very reliable, yet they do sometimes break down and in this plant the ship could not proceed until the repair had been made good.

It is to be noted that, excluding two small generating sets, there is only one large one fitted. As stated already, this is quite satisfactory so long as conditions are normal, the set being used for loading and unloading the ship, and being overhauled at sea when necessary; but if an auxiliary compressor is fitted a large generating set must be available at sea, which interferes with any overhauling that has to be done from time to time. Similarly, as with the air compressor, large generating sets are known to have failed and it is wise to ensure against misadventure by having at least two large generating sets.

On board a particular vessel the armature of one set "blew out" and in the other the bottom end of the connecting rod of the air compressor "fired up." In this case two generating sets were not too many.

The use of waste heat from exhaust gases and jacket cooling water,

Mr. A. D. Bruce

is very important on account of the large proportion of the heat supplied that is lost through these mediums, and the most perfect example of economy in this respect is the Scott-Still engine. Where electrical auxiliaries are fitted throughout it is not easy to decide what to do with the 70 horsepower which might be available from this source in a vessel of the size given in the Paper. The generating sets which are essential for cargo handling have a very easy duty at sea and to drive some auxiliaries by steam would leave no duty for them at all. One does not care about fitting costly electric generating sets and only using them in port; they are so much useless cargo when at sea.

At any rate the waste heat could be used for heating the accommodation, oil tanks, etc., and save a fraction of a ton of fuel per day that is used to fire the donkey boiler at present in most motor-ships.

Mr. Calderwood

Mr. J. CALDERWOOD, *Graduate*: Mr. Belyavin is to be congratulated on having presented to us this evening a most interesting paper in which he has made many novel and interesting proposals, which are of particular importance at the present time when the development of the oil engine is being hindered by the high first cost of installations.

As Mr. Belyavin states there is little doubt that a two-stroke engine can be built more cheaply than can a four-stroke, and has the further advantage of smaller weight and size with a consequent increase in cargo capacity. The design that he proposes has a single-piece crank-shaft and while this will reduce size and cost it has obviously serious disadvantages. I should be glad if Mr. Belyavin could give any information as to the increase in weight, cost and size involved by the use of a two-piece crank-shaft in his design.

The question of the waste-heat boiler and its use is one of great interest. Working on an exhaust-gas temperature of 280° C. and steam at 80 lb. per square inch pressure, a quantity of about 1,500 lb. per hour could easily be obtained; this would be sufficient for a 30 to 35 kw. compound-engine-driven generator which would supply all the current necessary for the auxiliaries running at sea. For heating and cooking the exhaust steam from the generator engine, could be used at a pressure of from 5 to 10 lb. sq. in. The boiler for an engine of the size proposed in the Paper would be about 8 feet long, 5 feet to 6 feet in diameter and would have about 100 to 120, $2\frac{1}{2}$-inch-diameter tubes. The cost of such a boiler would be very small and would I think fully justify its use. I should suggest a modification of Mr. Belyavin's engineroom arrangement, the 25 kw. generator driven from the main engine being omitted while the 25 kw. generator shown as driven by a small oil engine should be arranged so as to be driven through a clutch either by a compound steam engine, at sea, or by an oil engine, in port.

One of the most interesting features of Mr. Belyavin's Paper is the generating set which he proposes for driving winches, etc. A generator designed for the mean load but capable of 100 per cent. overload is a novel suggestion, but one which should be very useful when put into practice.

Mr. Belyavin mentions the vibration of generators and suggests that makers should carry out experiments to obtain curves similar to that which he gives. Such experiments do not appear to me to be necessary as the natural frequency of vibration can very easily be calculated for any d.c. generating set. In the case of a.c. generators running in parallel, however, it must not be forgotten that we get an elastic force tending to keep the generator in phase. We thus get the system attached to a comparatively infinite mass (that is, the hull of the ship) through an elastic connexion. This involves a somewhat different calculation from that used for a d.c. generator. Mr. Calderwood.

Additional remarks communicated in writing: In making the calculations for the natural torsional frequency of a generating set, the reciprocating masses must be replaced by equivalent rotating masses: a method of making this substitution was given by Dr. Gümbel in a paper read by him before the Institution of Naval Architects in 1902.

Formulæ for the natural frequencies of any generating set can easily be obtained by the method described in the paper recently written by Mr. Thorne and myself for this Institution, or, if a graphical method of solution is preferred, that described by Dr. Gümbel in the Zeitschrift des Vereines Deutscher Ingenieure (No. 11, 1922) may be used.

Mr. HARRY HUNTER, O.B.E., *Associate Member*: Mr. Belyavin proposes a 30-inch bore by 60-inch stroke two-stroke engine, but I do not think there is a marine engine of that size running at the present time. Would he anticipate any scavenging trouble in an engine of these dimensions, particularly in view of what we heard from Mr. Rennie last week in connexion with scavenging difficulties? He also mentions a boiler efficiency of 90 to 95 per cent. for his waste-heat boiler. Is that by calculation or by experiment? What is the radiation loss in that boiler? Mr. Hunter

In connexion with the waste-heat boiler we have 7 per cent. claimed from it, but even the Still engine gets only about 10 per cent. from the steam side, and I think it is asking too much to expect the 5, 6, or 7 per cent. mentioned to-night. Mr. Belyavin mentions that with this arrangement the silencer can be dispensed with. Does he consider it unnecessary to provide a silencer for manœuvring? I know a number of pilots who are very nervous when they hear an engine starting and they want to stop it as soon as it fires.

With regard to the quantity of steam used by auxiliaries I think there is an amount of information available now from turbine-driven ships, where the owners have been endeavouring to cut down consumption, and have fitted water motors in their ships. There is one known case where they used 25 per cent. of steam for auxiliaries, and it is now cut down to 17 per cent. I mention this to show that figures are now becoming available for the consumption of auxiliaries. I take it that by a ship of 4,000 b.h.p. the Author refers to a warship. He mentions that the Diesel engine is the only part that gave any trouble in this case.

Mr. Hunter

Of course, that cuts both ways; I have found Diesel dynamos extremely handy when in trouble with condensers and the boilers priming. If you had a Diesel-driven generator the priming does not stop that, and in case of such a breakdown you have the lights left to deal with the various other consequential troubles.

With regard to overload of this engine—100 per cent.—in the case of certain designs of engines it has been found advisable after a lot of experience to design the fuel pumps so as to limit the quantity of oil you can put into any one cylinder. It very often happens that one cylinder may be developing perhaps 50 per cent. of the whole and the others just 25 per cent., and to get over that the capacity of the fuel pumps is limited so that you cannot push in more oil than is necessary for developing say, 20 per cent. overload.

Mr. Belyavin does not state how the oil fuel, cabins, cylinder jackets, etc., are to be warmed when required.

Mr. Kimber

Mr. H. G. KIMBER, *Associate Member*: I should like to thank Mr. Belyavin for his very interesting paper, which is obviously the result of considerable experience. The curve given (Fig. 5) for the critical speed of six-cylinder engines is particularly interesting, as it not uncommonly happens in six-cylinder engines that the critical speed coincides with the desired running speed, unless special precautions are taken.

Is the curve for engines with crank-shafts in one piece?

Mr. BELYAVIN: One piece.

Mr. KIMBER: I have had some experience with crankshaft vibration on six-cylinder engines with a stroke-bore ratio of about 1·2. With 11½ inches bore the critical speed was 420 r.p.m.; with 12 inches bore, 385 r.p.m., and with 20 inches bore, 208 r.p.m. A previous speaker stated that the calculation of the critical speed of a reciprocating-engine crank-shaft is a very simple process. My recollection is that it is rather complicated and involves some assumptions. If it *is* a very simple process I would suggest that the information would prove a useful addition to the proceedings of the Institution.

With reference to the arrangement shown in Fig 7, I should like to ask the Author his reason for putting the thrust block between the fly-wheel and the engine. No doubt the arrangement has some advantages, but I should think it would render the engine crank-shaft more susceptible to torsional vibration troubles.

With regard to the link-motion method of driving air compressors and scavenge pumps (Fig. 2) a similar arrangement used to be commonly adopted for driving the air compressors of land engines, but the method has been abandoned. There are so many pin joints, each one requiring lubrication and each apt to develop slack. The cumulative effect of slack in the joints gives rise to trouble in the air-compressor cylinders with their fine clearances. With the arrangement shown, motion is

transmitted from the engine crank to the compressor piston through *thirteen* pin bearings. I should prefer to use a crank-and-connecting-rod drive with only *two* pin bearings. Mr Kimber

With regard to the outline diagram shown in Fig. 1, there is a small diameter journal shown at the compressor end of the shaft. I would suggest that it would be safer to omit this journal, leaving the compressor crank overhung. With the arrangement shown, the compressor crank-pin might be subject to severe bending stress at times, due either to whip of the shaft or to unequal wear in the bearings.

VOTE OF THANKS.

THE CHAIRMAN (Mr. Harold Thomson): I propose that we thank Mr. Belyavin very heartily indeed for coming here to-night, and for the second time giving a paper before the Institution. We have all enjoyed the reading of the paper very much, and there is a lot of food for thought in the proposals he has put before us. I am sure he deserves our best thanks. Mr. Thomson

The vote was carried with acclamation.

CORRESPONDENCE.

MR. JOHN S. BROWN, *Associate Member*: Mr. Brown

Exhaust Boilers.—The output obtainable from exhaust boilers as given in Fig. 3 may be in agreement with the performance of some of the earlier installations where normal coal-fired units are in use; but that modern practice can give much better results is brought out by the following examples. Surely some of these must have come under notice and suggested that the basis of the calculation given involved some discrepancy.

(*a*) Any of the firms manufacturing boilers for land engines will undertake to give an evaporation in excess of 1 lb. per b.h.p. hour; and for the steady conditions of marine service it would probably be considerably in excess of this figure.

(*b*) The published figures for the Scott-Still-engine trials show 2·6 lb. evaporation per b.h.p. hour; and as this engine is of the two-stroke type, it is, according to the Author, unfavourably placed. Making every allowance for the special conditions, it appears that, certainly half this evaporation can be definitely credited to the exhaust gases.

(*c*) There are records of a four-stroke Diesel engine giving 4,000 B.T.U. per lb. of oil used from the exhaust gases when operating at 90 lb. sq. in. m.e.p. on 0·31 lb. of fuel per i.h.p., and this without bringing the gas temperature so low that funnel corrosion would be likely.

Mr. Brown

The difference between the figures just given and those in the Paper arises, in the latter instance, from the assumption of a low value for the heat carried away in the exhaust gases and also from the neglect of feed heating. Reference to the Institution of Mechanical Engineers' gas-engine trials will show that only a calorimetric method will give the true exhaust heat, and the figure used, 0·30 of heat in fuel, is certainly low. It has probably been derived from pyrometer observations taken at cylinder flange, and it may therefore be appropriate to illustrate the matter by reference to a case where the exhaust manifold jacket, in way of cylinders only, took up 7 per cent. of heat in fuel without any corresponding reduction of the gas temperature as observed at cylinder flange and manifold outlet. While part of the discrepancy may have been due to after-burning in the manifold, it is probably largely accounted for by the difficulties in obtaining the true mean temperature of a gas subjected to widely varying velocities. The temperature at which the gases reach the boiler is very much less than in the normal donkey boiler, and an efficient feed heater becomes of prominent importance; indeed, the feed can generally be brought up to evaporation temperature without any reduction in the heat available for actual steam production. Further, the heat-flow conditions call for something very different from the standard arrangement of heating surface if a reasonable bulk and cost are to be attained. Here the work of the late Dr. Nicholson on high-speed boilers has a direct application; and by the adoption of a value for the ratio of tube length to tube diameter, which may appear relatively large, it can be shown that an evaporation per square foot corresponding to that in the coal-fired boiler may be attained. Such a tube arrangement might be considered unacceptable due to back-pressure, but there is considerable scope for reducing this factor by making use of the energy represented by the 40 lb. sq. in. pressure in the cylinder at the commencement of exhaust.

Mr. C. R. Bruce

Mr. CHARLES R. BRUCE (Scotts' Shipbuilding and Engineering Co., Ltd.): Mr. Belyavin recommends that four-cylinder engines be adopted up to a certain limit of power, and there are few who will disagree with him; but care must be taken that the limit of power is not exceeded. It is suggested that a two-stroke-cycle engine of 30 inches diameter is bordering very nearly upon the limit for a cargo ship where full power is expected continuously. The main engine, however, plays a very small part in Mr. Belyavin's paper, the main part being given over to a consideration of the best type and arrangement for auxiliaries. The whole paper finally hinges upon the ideal oil engine to be used for a generating set. Is there not a contradiction where Mr. Belyavin says, referring to high-speed engines: "All the difficulties with quick-running oil engines are overcome"? Prior to that he claims that the attention and overhaul required is in proportion to the speed of running.

Among his recommendations for the ideal engine for a generating set are:—

Mr. C. R. Bruce

(1) Two-stroke cycle.
(2) Port scavenging.
(3) Mechanical injection.
(4) Timed fuel pumps.
(5) Automatic fuel valve.

Each of these is a desirable feature to incorporate in such an engine, and the reduction of valves to the minimum assists materially in reducing the attention and overhaul arising from high running speeds. It is agreed that the elements (3), (4) and (5) result in an engine having excellent regulation.

On the question of auxiliary engine rating, however, Mr. Belyavin's views are open to question. There is no doubt that the load from winches in particular fluctuates very much, and the need to cope with the maximum load results in a heavy generating installation; but is he going to overcome this by making an engine of 150 kw. and calling it a 75 kw. machine? Will Mr. Belyavin be able to induce registration societies and others to accept reduced scantlings, and is he not incurring considerable loss in mechanical efficiency by his proposals? Very few makers at present will supply auxiliary engines giving an overload of more than 10 per cent., and the consequence is that engines are so chosen that they are run extensively at low powers. The collection of accurate data of load variation at sea and in port would help designers towards a more economical solution of this problem.

Mr. Belyavin refers to waste-heat boilers. In the Scott-Still engine a special feature of the system is the recovery of waste heat in the form of steam. It is considered that a feed-water heater is a very desirable part of the steam-raising plant, and is preferable to having a low-pressure boiler in addition to a high-pressure boiler. Where a feed heater is fitted the gas temperature may be reduced to 150° or 200° Fahr. if desired. It is agreed that where there is a waste-heat boiler it should be specially designed for the purpose, and it is worth while pointing out that the conditions of such a boiler on service are extremely easy, particularly with two-stroke-cycle engines, and further, such a boiler serves very effectually as a silencer.

The whole question of auxiliary driving on oil-engined ships is a difficult one. There is an inclination towards pumps being driven by the main engines. On the other hand, it is significant that in steam-engine practice there has been, for some time past, a tendency in the opposite direction.

Mr Swinnerton-Dyer

Mr. L. S. SWINNERTON-DYER, *Graduate*: Mr. Belyavin is to be thoroughly congratulated on his solution of the difficult task of effecting a working compromise between the academic ideals of engineering design and the economic necessity of low first cost.

In a paper that deals with big problems it is perhaps unfair to criticize small details, but the overall length of the engine in Fig. 1 would be slightly increased by the necessity of providing some gear to drive the

Mr. Swinnerton-Dyer.

cam-shaft. Further, a stroke-bore ratio as high as 2·0 will introduce—at least at overload—scavenging difficulties not easy of solution.

Driving the scavenge pump and compressor through levers is open to serious objections, as not only does the inevitable play in the many pins, but also the distortion in warming up, cause important differences between the actual and designed cylinder clearances. The rooted objection of many engineers to lever drive is not, therefore, wholly prejudice. It should not be forgotten that the increased complication and cost of turbo-scavenging is to some extent offset by the slightly reduced overall fuel consumption.

The installation of a waste-heat boiler seems doubtful policy with a two-stroke engine, though paying results would probably be obtained with a four-stroke. But even under the most favourable conditions the waste-heat boiler hardly provides a satisfactory solution of the problem of regeneration. It is not impossible that the problem may be solved, while avoiding the considerable complication of the Still engine by a form of exhaust-driven turbo-scavenge. Taking conservative figures for the efficiency of the mechanism, it would appear that there would be sufficient power available for appreciable supercharging—a point of great importance when working at heavy overloads. The two greatest practical drawbacks are (1) the necessity of placing the mechanism very close to the exhaust ports, and (2) the cost due to the necessity of the finest workmanship and the utilization of alloy steels.

The choice of a two-cylinder two-stroke engine for auxiliary work seems very sound, both on the grounds of space and cost. The addition of a separate scavenge pump is undesirable if it could possibly be avoided. Has the Author any data available on the falling off of scavenge efficiency at big overloads with crank-case compression? The consistently successful running of semi-Diesel engines seems to suggest that in practice crank-case compression is not as inefficient as it can be made out to be in discussion The adoption of solid-injection by automatic valves would appear, for small engines at any rate, to be inevitable—though the multitude of different designs in use shows that there is scope for valuable research work on this detail. Does the Author consider that the spring-controlled differential-piston type—used, for example, in the Plenty engines—has any advantage over the simple sprayer with a ball-check valve—such as fitted in the Gardner and Vickers-Petter engines? Is there any data available, in the case of the latter type, on the effect of the strength of the check-valve spring and the space between the valve and the nozzle on the running of the engine? Can "dribbling" be prevented in small engines without reducing the nozzle to microscopic dimensions?

Lastly, while one is bound to admire the ingenuity of the "two-piece" cylinder head in Fig. 6, one is also inclined to wonder whether the increased complication is altogether necessary in a small engine. But even if one disagrees with a few of the details there can be no two opinions that Mr. Belyavin has rendered good service to the cause

of practical engineering by placing his carefully thought-out Paper before the Institution. Mr. Swinnerton-Dyer

Mr. J. FLEMING, *Member* : The Author suggests for the larger powers that the main engine should not drive its own pumps. I am of opinion that if an engine can be made to drive a ship it can be made to drive its own auxiliaries, and that the relative power required to drive auxiliaries will decrease as the size of the engine increases. It is preferable to drive the auxiliaries from the engine for any power to avoid duplication of auxiliaries in the engineroom and the occupation of valuable space, especially in the case of the larger installations. Mr Fleming

It would appear that there is rather undue optimism on the part of the Author on the performance of the auxiliary engine for 75 kw. generating set at a heavy overload. The statement is made that the temperature rise of piston is not instantaneous, but can it be assured that the piston would have time to reach an average safe temperature between the demands for the maximum power on occasions, such as when an easily handled cargo is being rapidly discharged, coupled with a continuous demand for power for pumps in the engineroom? It will be interesting to know whether an engine designed as described by the Author would be sufficiently economical to justify its replacing one designed for total possible load on absolutely safe lines.

Mr. Belyavin recommends the use of waste-heat boilers, mentioning the possibility of the exhaust-gas being first used to give steam at a high pressure, and then, having still a " fairly high temperature " to be used for raising steam in a low-pressure boiler. While advocating the use of the exhaust gas, this proposition would seem to carry matters too far, for most of the two-stroke engines, which are likely to be generally adopted, cannot give a high exhaust temperature; indeed, 500° Fahr. would seem a reasonable figure, and adopting this amount it would mean that only a small pressure of steam, say 8 to 10 lb., could be maintained in the boiler. To obtain a higher constant pressure an additional oil burner is necessary, which again leads to the difficulty of using exhaust gas and oil at the same time under pressure. This difficulty is being overcome, I understand, by the adoption of a balanced damper, which regulates the pressure upon each and gives satisfactory combustion. Arrangements could be made to by-pass the exhaust gas when oil burning, and to use it either in part or wholly for the feed water, which can be raised to 190° and more with this gas alone.

Engr. Lieut.-Commdr. L. J. LE MESURIER, R.N. (ret.), *Member* : In general I am in complete agreement with the conclusions arrived at by the Author. Eng. Lieut.-Com. Le Mesurier

The weight given for the main engine of 2,500 b.h.p. is well within the limits of present designs. A Sulzer engine, for instance, working at the same piston speed and at rather lower mean pressure, would certainly weigh less than 300 tons including thrust block and shaft, air

Eng. Lieut.-Com. Le Mesurier

starting bottles, fly-wheel, silencer, and all pumps required by the main engine. This would be the case whether a four-cylinder or six-cylinder engine were employed.

With regard to auxiliary machinery, it seems to me preferable that the same type of Diesel engine generating sets should be used both in harbour and at sea rather than that a plant designed specially for harbour use should remain idle at sea. A stand-by set would, however, be necessary to enable inspection and overhauling to be carried out. All sets being of the same type supervision and maintenance are easier than where different types are installed and furthermore less spare parts have to be carried.

The load at sea and in harbour should preferably be so arranged that generally it is possible to provide for the generating sets to run at half to full load. Under certain conditions, however, such as during night time in harbour, only a moderate amount of ship lighting may be necessary and for this reason quite a small set should be fitted in addition to the main generating sets.

The Author's specification for an ideal oil engine for generating sets happens to coincide almost exactly with the Sulzer "RV" two-stroke engine with airless injection and water-cooled cylinder head except in one respect, namely, crank-case scavenging; and yet, seeing that simplicity and reduction in the cost of auxiliary plant are two such eminently desirable features, they, in my opinion, more than compensate for the alleged disadvantages mentioned by the Author. Reliability, which in this case is entirely a matter of detail design, has actually been obtained under the most trying conditions, and can certainly be equally well assured with crank-case scavenging as with direct scavenge pumps for all powers up to at least 300 kw. sets and even assuming the lubricating-oil consumption is somewhat higher, it would not appreciably affect the cost of running the complete installation of both main and auxiliary engines.

Mr. Rabbidge

Mr. W. F. RABBIDGE (of Messrs. Vickers, Ltd.): The Author begins his Paper by a curt dismissal of the four-stroke main engine on the allegation that the two-stroke engine is half the weight and size, and is much cheaper. Such a claim, of course, assumes that the mean pressure is equal to that in the four-stroke engine. On the other hand, in submitting a design for a two-stroke auxiliary engine, he deliberately rates it for continuous running at half the pressure he would use in a four-stroke engine of the same size and speed. The reasoning appears to be that the same mean temperature is thereby obtained, and this gives the same durability to the pistons, which otherwise might crack. The main-engine pistons are certainly cooled, but this will not materially affect the argument that what ratio of pressure for continuous running is necessary in the auxiliary engine is also required in the main engine, the larger piston being, if anything, more susceptible to damage by continuous overload. The conclusion would be that the two-stroke main engine,

applying the caution that the Author exercises in his own auxiliary-engine design, should be rated for continuous running at about half the power attributed to it. Mr. Rabbidge

It is assumed that the Author is "trailing his coat" in order to provoke discussion, but it seems hardly necessary to bring forward facts from actual comparisons pointing to the fallacy of his generalization regarding the relative merits of the two types of engine when he has so considerately provided the necessary refutation in the body of his own paper.

I imagine that it is generally accepted that there is a wide field of ordinary application in which both two-stroke and four-stroke Diesels can compete on equal terms, and that sweeping statements as in the Paper are nowadays discounted at sight. The Paper will, however, have served a useful purpose if it attracts attention to the necessity, when comparing marine engines, of ascertaining the mean pressure advocated and of forming a judgment, as far as actual long sea performance permits, as to its justification.

Mr. J. STOTT (of the Fairfield Shipbuilding and Engineering Co., Ltd.): Mr Belyavin has contributed, in my opinion, a most valuable Paper, and one which should be studied particularly by engine builders who have had no experience of marine-motor installations, as it lays before them in a clear and well reasoned manner the engineroom outfit for such a vessel. Mr. Stott

Mr. Belyavin states that the reliability of oil engines now is considered to be unquestionable, but unfortunately, motor-ship logs still record involuntary stoppages at sea, and while the majority of these are of short duration and for trifling causes, still, so long as they appear, the opponents of the oil engine will not fail to draw attention to them.

The Author deals with only two types of engine, viz:—the four-stroke single-acting and the two-stroke single-acting, but there are now three vessels on service, driven by opposed-piston solid-injection engines, whose service records compare favourably with those of the vessels fitted with engines of the type dealt with by Mr. Belyavin. I am of opinion that there is a promising future for these one great advantage they possess being that no injection-air compressors are required. It will be generally admitted that the compressors and their valves are perhaps the most frequent sources of trouble in motor-ships.

Single-screw ships are advocated, which I agree are the most economical for cargo vessels or tankers; but we must now look forward to the building of motor passenger vessels, for which twin screws at least would almost certainly be specified. In such a vessel, I am of opinion that all the main-engine auxiliaries should be independent with the exception of the compressors. Each of these would, of course, require to be in duplicate, or for such purposes as jacket cooling, have an emergency connexion to the ballast or general-service pumps. For the forced-lubrication service, steam-driven pumps (a boiler would certainly be

Mr. Stott

installed in such a vessel) can be procured at such a price that I doubt if it is not a cheaper proposition than fitting these on the main engines. Independent pumps are also capable of regulation of the motive power to suit requirements, which, on engine-driven pumps, can only be accomplished by adjustment of the valves, without reducing the power absorbed. In addition, remembering that we are considering a twin-screw installation, in the event of an engine-driven auxiliary breaking down, although a stand-by may be brought into service or a cross-connexion from the other engine opened, the defective part cannot be repaired without stopping the main engine.

Referring to Fig. 1, I fear very few superintendent engineers would accept the crank-shaft as shown. A coupling in the centre, making the forward and aft lengths interchangeable, would almost certainly be insisted on, which would lengthen the engine by the length of the coupling bolts plus an additional bearing and the necessary clearances. For a single-screw cargo vessel, a spare length of crank-shaft would either be carried or stored on shore, and shipowners would shy at paying for a complete shaft, for the sake of a slight saving in the length of the engine.

Mr. Belyavin's paragraph on the waste-heat boiler is interesting and though his figures and deductions appear conclusive, until results of experimental work in this direction are available, it will, I fear, be difficult to induce shipowners to adopt such an installation.

This is the extent of my remarks on this most interesting Paper, which I hope may incite other engineers to give their institutions similar papers on, say, fast-running and large-passenger-vessel installations.

AUTHOR'S REPLY.

The Author

As many of the contributors to the discussion raised similar points. I shall first give a general reply to these, and then endeavour to answer all the questions raised individually.

Crank-shaft in one piece.—I recognize very well that most superintendent engineers have objections to crank-shafts for four-cylinder engines made in one piece, because a spare crank-shaft has to be made in one piece with four cranks, and costs more; I also appreciate that most engine builders are shy of putting forward this scheme for the same reason; however, I raised these points again to state once more how much this question of the one-piece crank-shaft is misunderstood. Evidently the shaft with four cranks is not objectionable, as there are several motor-ships with eight-cylinder four-stroke engines of very similar dimensions with crank-shafts made in two pieces, *i.e.*, with four cranks in each piece. A spare crank-shaft with four cranks instead of two is certainly more expensive; but the saving on the engine will heavily outbalance this loss, and the additional saving on the length of the engine-room will be a pure gain.

Here is the balance sheet of losses and gains on the above arrangements :— The Author

Crank-shaft in one piece :

Loss :

(1) Two cranks on the spare crank-shaft.

Crank-shaft in two pieces :

Losses :

(1) One coupling on the crank-shaft with all the bolts, nuts, etc., and a length of one bearing.
(2) One transverse section of a bedplate.
(3) One main bearing.
(4) One column.
(5) From two to three feet on the length of the engine and the engine-room.

I reckon that the total saving in weight and cost of the installation with a single crank-shaft will be about 6 per cent. plus an increase in cargo-carrying capacity.

If all superintendent engineers would agree to give up everything that is merely a matter of opinion or their personal fancy and stick to an £.s.d.-and-arithmetic policy, motor-ships would probably become both much simpler and cheaper.

Scavenging pumps and compressor driven by levers.—I know that many engineers object to levers, and I quite sympathize with them, because most of their objections are due to wrong design of levers—the point which I raised in my Paper. If the levers were of correct design and fitted with forced lubrication, the objections would certainly disappear. However, I am myself of opinion that it is better to drive the compressors, which require very fine adjustment, from the crank-shaft directly, because the load on the compressor pistons is rather too great, the levers are too heavy, and the required fineness of adjustment would often be upset by the clearances in the bearings.

For driving scavenging and water pumps levers appear to be quite suitable, as no fine adjustment is required and the load is very small. It must be born in mind that forced lubrication to the levers and the correct arrangement of guides will considerably improve the working of levers.

Waste-heat boilers.—The figure for the amount of steam obtained in a waste-heat boiler depends certainly more on the engine than on the boiler. If the thermal efficiency of the engine is high, the temperature of the exhaust gas should be less and the production of steam in the waste-heat boiler also less.

In the Still engine high steam production must be explained first by the considerable quantity of heat lost to the jackets, and second by moderate thermal efficiency of the internal-combustion cycle with moderate compression ratio, which they use.

Economizers can, of course, considerably improve the steam production in a waste-heat boiler; however, it will always be open to question

The Author

whether the complication of having a low-pressure secondary boiler or an economizer is practicable, and only long experience can tell us whether such a scheme is safe, because of the possibility of corrosion of the economizer tubes and the funnel by sulphuric acid condensing at low temperatures. If we use economizers to heat the feed water to 100° C. (about 212° Fahr.) with a final difference of temperature between the exhaust gas and the steam of 30° C., the temperature of exhaust about 325° C. (about 620° Fahr.) and percentage of heat in exhaust about 0·30, we can considerably increase the quantity of steam produced.

The last figure is rather on the high side, and very often is less—about 0·28, or so. These figures can be calculated most accurately from the heat balance of the actual engine.

The steam production in this case, with the steam pressure about 60 lb. will be (for a two-stroke engine)

$$W = \frac{0{\cdot}195 \times 10{,}000 \times 0{\cdot}30 \times [325 - (151 + 30)] \times 0{\cdot}90}{(652 - 100)\,325}$$

$$= 0{\cdot}47 \times 0{\cdot}90 = 0{\cdot}4125 \text{ kg.} = 0{\cdot}94 \text{ lb. per hour.}$$

However, this figure should be considered an absolute maximum, and is rather doubtful. In a four-stroke engine this figure, of course, can be greater because of the higher temperature of exhaust.

The minimum figure, without economizers, and with the exhaust temperature only 275° C. (530° Fahr.) will be about 0·625 lb. per b.h.p. hour.

The last I consider is a safe and reliable figure for estimating the steam production in a waste-heat boiler. It does not depend so much on the boiler as on the general condition and the engine efficiency.

Proposed 75 kw. generating set.—Experimental data on the fluctuation of current in winches is very scarce; however, judging from some experimental data given by Mr. Reavell in *Engineering*, 21st March, 1913, the average load will probably be only a quarter of the maximum. Thus the working condition for the proposed engine will be very safe, but a maximum output equal to twice the normal will in any case be essential.

Basing my considerations on my experience with semi-Diesel engines, I believe that mechanical efficiency of the above engine will be about 80 per cent. at normal power and rather less at average load if that is about half of the normal. But this will also be the case with any other engine, and the average efficiency at small load will be moderate. The big difference between the average and the maximum load at which the engine will have to run will certainly make the conditions very unfavourable for high mean mechanical efficiency, quite apart from the engine design.

Reply to Mr. Harold Thomson.—I quite agree with Mr. Thomson that in certain cases steam auxiliaries are quite practicable, as they are simpler and cheaper; for example, for motor-ships which require few auxiliaries at sea and trading between distant ports. In many other

cases, however, the saving in the fuel with electrical auxiliaries might be quite worth considering. The Author

Reply to Mr. Harry Hunter.—I am positively certain that no scavenging difficulties can be anticipated in an engine of the size I propose. The scavenging is a very intricate and difficult problem, but, if investigated carefully, no trouble should be expected with any size of cylinder, and with any stroke-to-bore ratio.

In the case of the Still engine, I suppose the design of scavenging arrangements was not correct.

The boiler efficiency about 90 per cent. includes only radiation losses, as all the other losses are to a great extent included in the calculation.

I suppose that, when manœuvring, the exhaust gas can still be passed through the waste-heat boiler, and the required steam can be obtained by burning oil in a separate space of the same boiler. Thus the silencer will be just as efficient as when running normally.

I am very pleased to hear that the steam consumption in auxiliaries is at last being investigated; some startling figures of steam consumption will certainly be disclosed. The overload of the auxiliary generating set I suggest in my paper is really not an overload—it should be considered a normal running power if the pistons were water cooled.

It is quite easy to adjust the pumps to supply equal quantity of fuel in a two-cylinder engine.

With regard to heating cabins, etc., I have not mentioned some of the arrangements of secondary importance, as these are more or less matters of opinion.

Reply to Mr. A. D. Bruce.—Diesel-electric drive in certain cases may have some advantages, but the cost, I should think, would be too high. With regard to stand-by compressor, if we are not fitting any stand-by main engines, why should we fit a stand-by compressor? The same refers to auxiliary generating sets: if they do break down often, it only means that the design is not satisfactory. A break-down should not be considered an every-day occurrence, and the other generating set suggested can be used with care in this case. If the same generating set is used in port and at sea, in order that it may not remain idle, a stand-by will be required, which will remain idle probably both at sea and in port; so what will be the advantage of having it?

Reply to Mr. Kimber.—I am very pleased to note that Mr. Kimber's figures for torsional vibrations agree fairly closely with my curve. I prefer to have the thrust block connected to the engine bedplate. In this case it is difficult to get the necessary space for the fly-wheel. The critical vibrations in a slow-running engine like the one described in the Paper are probably more likely in the tunnel shafting, and can easily be dealt with by adopting a suitable shaft diameter.

Personally, I quite agree with Mr. Kimber's remark on the compressor crank-shaft, and am sure that the crank-pin can be left over-

The Author

hung; however, I never had any difficulties with the design shown in Fig. 1, and think that an overhung crank-pin may lead to some objections.

Reply to Mr. Calderwood.—I quite agree with Mr. Calderwood that if a waste-heat boiler is fitted, the small generating set should be driven by steam on voyage. However, I have no personal experience with waste-heat boilers on two-stroke engines, and did not want to put forward a scheme on which there is not sufficient data available.

I am very pleased to note that Mr. Calderwood has found a simple way to obtain the figures for the critical period, which would be a great advantage to all oil-engine manufacturers.

Reply to Mr. C. R. Bruce.—Under the "difficulties" in quick-running engines, I mean cover and piston cracking, piston cooling difficulties, etc. The general wear in quick-running engines is naturally more than in slow-running ones, and this, of course, will hardly ever be cured.

Referring to the auxiliary engine, the 150 kw. cannot really be called an overload; but the engine is supposed to run normally at half the full load, *i.e.*, 75 kw.

Reply to Mr. Swinnerton-Dyer.—The engine shown in Fig. 1 need not be increased in length for the sake of the driving gear, as it can easily be arranged in several ways; for example, as a train of gear-wheels on the aft column, with the spur wheel mounted on the aft coupling, without any increase in length. However, I had another driving gear in mind.

With regard to scavenging, I can assure Mr. Swinnerton-Dyer that a stroke-to-bore ratio even as much as 3 would not be too bad, and our latest experience confirms that an absolutely perfect scavenging is quite possible with a stroke-to-bore ratio of more than 2.

Mr. Swinnerton-Dyer's remarks on waste-heat utilization are quite interesting.

From what I can recollect now, the volumetric efficiency of the scavenging pumps in the crank-case is somewhere between 60 and 70 per cent. For this reason engines having this method of scavenging cannot run with more than 35-40 lb. mean pressure, although usually bad design of scavenging might be partly responsible for this.

It is very difficult to give general reasons for the adoption of one or other design of automatic injection valves. The ratio of compression, distance of the hot plate from the valve nozzle, etc., have to be considered. From my experience, it is very difficult to prevent "dribbling" in small engines; possibly if the fuel valve is arranged upside down above the hot plate it might help to reduce the bad effect of the same.

A two-piece cylinder cover, as shown in Fig. 6, would not cost any more than one made in a single piece, because in this case the machining of hand holes or core holes will be eliminated. Besides, the casting

having no enclosed water space, will be cheaper and the percentage of bad casting will be reduced. From my experience, it often pays to adopt a two-piece design. The Author

Reply to Engr. Lieut.-Commdr. Le Mesurier.—I am very pleased to note that Commdr. Le Mesurier agrees with the views expressed in my Paper. The weight of the engine I give is certainly a very conservative estimate, and could possibly be reduced.

With reference to Commdr. Le Mesurier's remarks on auxiliary engines, I suggest in my Paper a single-cylinder engine of the same cylinder dimensions and design as a two-cylinder set, so that all the spare parts would be the same. If the big engine will run at sea as well as in port, the stand-by set will remain idle all the time: thus no advantage will be gained, and, besides, two engines will be required instead of one.

The fitting of a separate scavenging pump to the auxiliary engine will permit of one engine doing the work of two at greatly varying loads; thus, one engine only will be required.

The crank-case scavenging certainly does not affect reliability, and I am sure Messrs. Sulzer Brothers' auxiliary engine is a most desirable and important contribution to the at-present small list of reliable auxiliary engines.

Reply to Mr. Rabbidge.—I can assure Mr. Rabbidge that it was not by desire at all to provoke a discussion on four-stroke versus two-stroke engines, as this point appears to me, and as far as I could ascertain when preparing the Paper, to the majority of Diesel-engine designers also, completely settled. I do not know of any field at all where the four-stroke engine could compete with a two-stroke one on equal terms; I cannot imagine an experienced engineer deliberately preferring a much more complicated and costly machine to a much simpler and cheaper one for no reasons whatever. The ever-increasing number of orders for two-stroke engines is an important indication that the advantages of the two-stroke cycle design are now realized.

With reference to the auxiliary generating set, I certainly would not like to run a two-stroke engine with uncooled pistons at the same mean pressure as a four-stroke one; however, if the pistons are water-cooled it becomes only a matter of temperatures, design and experience; and from the data of the actual experience at sea available up to the present time, it appears to be absolutely possible to run a two-stroke engine with the same (or even higher) mean pressure as a four-stroke one.

Reply to Mr. John S. Brown.—As mentioned already in the general reply, the waste-heat-boiler efficiency is of secondary importance; it is the engine efficiency, or rather inefficiency, which must be considered in the first place.

The Author

Reply to Mr. Fleming.—With reference to driving the auxiliaries from the main engine, it should, of course, be done as far as possible; however, if more power will be required than the maximum possible size of engine can develop, the auxiliaries will have to be driven separately, as this will, in some cases, give as much as 15 per cent. more power on the main-engine shafts.

The suggested generating set will probably run continuously on only a quarter of the maximum load, and the maximum will be reached in quite exceptional cases; the efficiency at small loads will, of course, be moderate, but this will be the case with any other engine design.

With regard to waste-heat boiler, I would refer Mr. Fleming to my general reply.

Reply to Mr. Stott.—I suppose mechanical injection is not a special feature of opposed-piston engines as apparently Mr. Stott thinks, but can be applied with advantage to any design of the engine.

Of course in motor liners a single-screw will probably be impossible, but this is rather a different class of ship: the installation suggested in my Paper is for an ordinary cargo steamer.

Conclusion.—In conclusion, I wish to express my hearty thanks to all the gentlemen who have contributed to the discussion on my Paper.

A REVIEW OF TURBINE TROUBLES AND THEIR CAUSES.

By Prof. A. L. MELLANBY, D.Sc., *Associate Member*, and
WILLIAM KERR, A.R.T.C.

[Read in Newcastle-upon-Tyne on the 16th February, 1923.]

Introduction.—When your Council expressed a desire that the subject of turbine failures should be introduced into the syllabus for this session, and it was suggested that the Authors might make this the basis of a Paper, they felt some reluctance in committing themselves to such a task. It is obvious that those not directly engaged in the manufacture and operation of steam turbines can hardly have a comprehensive knowledge of general failures. Only a limited number can, at the best, come within their personal notice, and they are dependent on published records or hearsay for their main information. At the same time, the subject is one that offers prospects of an interesting and important debate, and it is certainly one to which the members of this Institution are uniquely competent to contribute.

We would, therefore, beg to introduce this paper as a straightforward descriptive treatment of the essential features of the subject; presenting, as it were, groups of points and comments that may serve as nuclei on which to centre a discussion that may, it is hoped, help towards the establishment of general conclusions and clear ideas on a very important subject. The paper is simply discursive and cannot be looked upon as wholly authoritative. An endeavour is made to refer to the main causes and aspects of troubles and failures, but it is hoped that inadequacy or imperfection in scope or comments will not be considered a serious fault if the paper is successful in extracting, from those with the special knowledge, the rectifying remarks that must, of necessity, contain the accurate facts.

The recent advances in turbines have been towards the development of enormous powers per unit, under the most extreme conditions of speed, pressure and temperature; so extreme, in fact, that available knowledge ceases to be an adequate guide, and new experience has to be sought. If, in the course of events, unpleasant difficulties are encountered, these can hardly be considered surprising, although the specific kinds of trouble may be rather unexpected. With it all, however, the recent developments constitute a great achievement, well worthy of a line in which the pioneering difficulties were so triumphantly overcome.

The troubles and failures encountered with turbines are traceable to many different causes that find their sources variously in constructional forms and details, conditions and difficulties of operation, weaknesses of materials, and in those general dynamical effects created by the enormous speeds at which the action in a steam turbine takes place. These effects are common to all turbines in a greater or less degree, but the developments towards extreme powers and speeds have intensified them to the limit at which they now frequently show themselves. Thus the large units call for a structural excellence that may be attained generally, but always with a shade of uncertainty; they establish high stress, high temperature conditions that get beyond our exact knowledge of materials, and so create doubt as to choice and results. Above all, they entail high-speed effects that compel a new study of the problems of rigid dynamics and bring some of the finer issues of the theory of vibrations out of the seclusion of formal mathematics into the hard light of practice. In all, there enter factors that when acting alone are not simple, but when grouped together are almost incomprehensible in their effects; and only to be studied through the medium of those difficulties that are certain to be disclosed.

Gearing transmission systems are so closely associated with turbine arrangements that they come, almost automatically, within the scope of any consideration of the latter; and the troubles conjured up nowadays at the very mention of the word "gearing" are so overwhelming, that no excuse need be made for introducing this subject into a general discussion of difficulties. The title of the Paper does not embrace this particular matter explicitly, but transmission faults react on the utility of the main engine system to such a degree that the two subjects may be considered inseparable. In land work the turbine raises the more important questions; in marine work the gearing troubles are most prominent.

In both lines—turbines and gearing—we have somewhat similar features. The thorough reliability and excellence of the earlier forms led to an extraordinary and confident forward movement, which has been brought up rather abruptly by the intrusion of unforeseen difficulties. Usually a progressive step is followed by a period of consolidation, but in the present cases it is no exaggeration to say that the rapid developments of the past few years have overshot the mark, and deceleration is now the more obvious tendency. Turbine designers may not willingly admit this, being perhaps inclined to consider that they have only halted; but gearing builders can hardly take up this attitude as the retrograde movement in their line is rather obvious.

Minor Features.—All faults necessarily arise from errors of prevision or of provision, but these may, furthermore, be within or without the limits of the definitely ascertained facts. Those within such limits belong to the category of blunders; those without constitute the real difficulties. Thus there always exists a certain probability of trouble due entirely to causes that are avoidable or not really

inherent in the system, but engrafted upon it by unnecessary error or established by wholly external agencies.

For instance it would be considered ridiculous to neglect the general principle that, where surfaces of high relative velocity are to run with fine clearance, one should be of a serrated form to provide thin rubbing edges. This principle is adequate to the maintenance of safety in most cases of rubbing, since the serious generation of heat is prevented by the rapidity with which a sufficient clearance is formed. It may not be entirely adequate where the contact is made under heavy distortional forces; but that is no reason why it should ever be ignored.

Again, the oil pump represents a very important external agent in any turbine system, and if this should fail, the results are not happy; but the fault can hardly be held peculiar to turbines. In the remarkable table of turbine troubles compiled by Mr. Baumann,* a case is cited in which severe blade rubbing resulted from a burned out bearing, due to an interrupted oil supply. Many cases of ruined bearings due to oil supply failures could be quoted; but the difficulty is obvious, and leads naturally to the safeguard of a stand-by oil pump which starts automatically if the main pump ceases to function.

Vibration represents a very general manifestation of trouble in running turbines, and while it most frequently arises from dynamical effects or distortion it may also be created by quite minor influences. Thus excessive freedom in the thrust bearing, or extreme journal clearance, may lead to vibration with effects on the main parts out of all proportion to the causes. Mr. Baumann instances a case in which excessive "lost motion" in the thrust bearing was a contributory cause leading to the repeated failure of a high-pressure labyrinth packing.

It is a fairly common belief—principally on the guidance of theory—that there is but little end thrust on impulse turbines. Actually, however, the thrust block is necessary for more than the maintenance of rotor position. The balance holes usually provided in the discs offer indirect evidence of this. If the indications of theory are blindly accepted there may or may not be trouble—it depends on the thrust block. In a series of similar impulse machines no balance holes were provided, and multi-collar thrusts were fitted. There were several cases of thrust-block troubles indicating excessive end forces, but when holes were cut in the discs the trouble ceased. The forces were, apparently, then brought within the capacity of the block. Clearly, if the thrust had been carried on a Michell collar the trouble might not have been shown; but the fault really lay in the neglect of a fairly obvious precaution.

These few remarks may serve to illustrate what might be termed the "avoidable fault." Into this category enters all unnecessary—because understood—errors of construction or erection; all neglect of simple

* "Some Recent Developments in Large Steam Turbine Practice" (Table 23), *Proc., I.E.E.*, April 7th, 1921.

well established principles of design; and all troubles due to auxiliary plant. The instances could easily be multiplied and will be varied according to experience. They may include the emergency trip that fails to work at the critical instant, or the traditional case of the apprentice left inside the cylinder! In fact, in this class of fault we include those errors not inherent in the turbine proper, and all probable causes of trouble that should not really be operative since, in general, they are clearly understood, and it is a primary duty to ensure their absence.

Distortion Effects.—Distortion could be somewhat paradoxically described as an effect that may be anticipated but not foretold; in so far that it is one of the most certain results of operation, but where it will appear, and whether it will appear to a serious degree, cannot be estimated. It arises from irregular heating in starting up; from uneven temperature variations in running; from sudden temperature changes due to alteration of load or condition; or from a restricted freedom for necessary expansion. In all cases it searches out weaknesses in construction or arrangement, and demonstrates its existence by vibration or foul.

At first sight it may be a little difficult to believe in severe distortional effects, or serious heating irregularities, in a machine of symmetry and steady conditions like a steam turbine. The symmetry is, however, more apparent than real and the effects only require to be relatively severe. Consider the casing arrangement. This part is of complex shape and suffers considerable variation of thickness and form throughout the length. It is bound down at the ends, and in certain regions some rigidity is imparted to it by steam-pipe and condenser connexions. It contains an intricate internal arrangement of diaphragms or blade rows which are separate from rotor parts by fine clearances only. The rotor itself is very symmetrical in form, but each part embodies mass variations that are of importance in view of the fact that the main steam flow is localized on the outer boundaries. The diaphragms, again, are bodies of awkward shape crowding on the high-speed rotor elements and requiring some freedom relative to the casing.

It will be clear that in warming up these various parts there must be quite appreciable temperature variations, and these will be in no sense representative of proper running conditions. In the preliminary heating stage the inlet temperatures will be too low and the exhaust temperatures too high; there will be slow heating up from the outside circumferences inwards (and with partial admission stages even the circumferential heating may be inadequate); there may be convection currents of importance; and at all places the temperatures are varying with time and with heat capacities. In changing on to load a new temperature distribution has to be established which, obviously, demands time; while any change of load will require—but cannot enforce—an equally rapid readjustment of the mass temperatures. Can it be held

surprising that it has been found necessary to study the starting operations carefully, and instruct operators fully as to the essential and safe procedure? Or is it to be wondered at that load is imposed on a slow instalment plan; or that sudden changes of load test fits and running allowances—and the heart action of the builders?

In the literature of this subject the seriousness of these heating effects and the necessity for slow and careful starting are rightly emphasized. There appears to be an idea, however, that these difficulties have originated with the development of the large land unit. They may have become serious, but they are not altogether new. They were encountered to a fair degree with the large direct-coupled marine turbines of about a decade ago; and the experiences with these are being renewed—although to a more serious extent. Undoubtedly, many of the complete strips that occurred in large marine turbines arose simply from this fault. The modern land turbine casing is certainly superior in its outline form and supports to the earlier marine type with its complicated—and probably unnecessary—external ribbing; but the more serious temperatures, the higher speeds, and the finer clearances in the former case demand all the advantages it can gain thereby.

Heating of the casing necessitates free normal expansion and distortion should not arise unless this is prevented. Irregularity of heating may be sufficient to induce deformation of shape, but, in general, this should only reach serious dimensions when heavy constraints are present. Thus Mr. Baumann cites two cases in which rigid piping connexions contributed definitely to failure. It is probable, however, that "hogging" of the casing can occur under temperature variations even when there is no serious influence exercised by connexions or supports. In a very large marine drum construction near the centre of the casing length, the blade clearances, as established by the usual cold measurements, were of the order of 150 mils. radially, and quite large axially. Under very light load conditions a strip occurred over several rows in this region. The conditions were such that the tying effects of the external connexions could hardly be severe; and it seems possible that relative "arching" of the casing and rotor took place to the extent necessary to cover the stated clearances in this central region, and is the more likely since the clearances in both directions from this point were greater.

It has been said that with the Parsons turbine the cylinder and blade drum are likely to arch upwards. The fault cannot be altogether characteristic of the type, but it may lead more easily to trouble on account of the smaller radial clearances. It might be expected, however, that reaction types would exhibit the influence of distortion most readily on the drummy rings as, at this point, the clearances are usually very small. Generally the "fine edge" principle should save this part from serious trouble, but there is no doubt that many cases of heavy rubbing have occurred. Thus the two cases already referred

to as illustrating the influence of rigid piping both caused labyrinth, presumably dummy, failure. More remarkable cases have occurred, however. In one instance the effect seemed to be so extreme that the labyrinth rings ultimately vanished. According to a remark in *Engineering** there have been other cases of this amazing occurrence, and while they are rather difficult to account for exactly, they may reasonably be considered as manifestations of casing distortion.

Temperature differences between the various parts of a rotor wheel will introduce stresses adding to those normally established in running. Clearly also temperature variations could search out any inadequacy of the fit between wheel and spindle; and this applies both to the end wheels on drum types and to the ordinary impulse wheel constructions. It would almost be reasonable to classify this trouble under the previous heading of "avoidable faults" as it would seem a simple matter to guard against such a difficulty; but there have undoubtedly been many cases of loosening of such wheels due to this cause, with the consequent setting up of vibration. Indeed, the effect seems common enough to have led to the employment of special arrangements to prevent the shake of slack wheels during starting up.

Actual distortion of the rotor is principally the result of local heating arising out of rubbing at some point. The most usual case is probably due to contact between shaft and diaphragm; intense local heating may then ensue with the natural result of bending, vibration, bearing trouble or actual breakage. One case may be quoted as fairly typical. Serious vibration was experienced in light load running under test, with ultimate gland seizure. On opening up, evidence of serious rubbing at the diaphragms was clear and the shaft was very definitely bent due to heating. A new shaft and increased diaphragm clearance remedied the matter, but whether the original cause was a slightly bent shaft or insufficient clearance is uncertain. In any case the effect is readily understood, and Mr. Baumann's remarks on this subject indicate very clearly that such cases are by no means unusual; and special precautions are necessary to avoid this local heating of the rotor with the resulting distortion. Obviously, from the case quoted, the first sign of vibration that may be due to such a cause calls for shutting down, since any delay will lead to a bent and useless rotor. Starting instructions usually embody a clause to this effect.

The diaphragm with its relatively small side clearance is especially susceptible to distortion effects. It is subject normally to heavy loads and, despite the coned shape usually followed, the deflections due to pressure differences alone are not inappreciable; and any distortion which augments these natural deflections may lead to serious trouble by complete absorption of the clearances. Now the deflection may be increased by pressure rise due to increase of load; by distortion due to temperature changes; by lack of sufficient radial freedom in casing;

* *Engineering*, February 16th, 1917.

or by excessive "give" of the nozzle plates. Baumann's list contains two cases that are illustrative of these influences. In one—a 3,000 kw. turbine at Wallasey—rubbing occurred between the last diaphragm and disc, due to distortion caused by change over from condensing to non-condensing conditions, with the consequent sudden and considerable temperature increase. Again, in a 35,000 kw. turbine at Boston, U.S.A., a very remarkable case occurred in which a low-pressure cast-iron diaphragm—apparently too weak—underwent excessive deflection on sudden overload. The diaphragm rubbed against and welded itself to the wheel; the rotation then threw off the two parts and burst the casing.

There are several interesting cases of diaphragm difficulties in that amazing catalogue of troubles published some years ago* with the quite moderate title "Deterioration of Turbine Blading." In this Mr. Fenwick has described faults and failures encountered with turbines belonging to the power companies on the Rand. Among many other points, he explains how the clearances between diaphragms and wheels diminished with time, how radial clearances of about a millimetre were absorbed within the first year; and how diaphragm loads and distortion led to severe buckling of the nozzle plates. These cases are, however, altogether exceptional as, undoubtedly, they arose entirely from the unsuitability of the materials used, and it is unlikely that such extreme results could be shown on present-day designs. At the same time, diaphragm difficulties and tendencies are very clearly brought out by such examples.

In all, then, we can understand the serious influence of heating and loading irregularities. Imposed on a casing of varying rigidity in different parts, the tendency is to deformation of form with the probability of blade rubbing or stripping. With rotors loose wheels may appear, or local heating may be set up by rubbing, with vibration and the chance of a bent spindle. Cause and effect are fairly well related but, unfortunately, the same can hardly be said of cause and cure.

Materials.—The problems of turbine construction have, at all stages, given a great impetus to the study of materials, more particularly to those non-ferrous metals and alloy steels suitable for blading. This arises from the exceedingly severe conditions of working in the blades that render all but the few best materials useless, and task these few to the extreme limit. Apart from the blading the problem of materials is relatively simple, but it has taken a goodly period of actual turbine experience and laboratory experiment to establish this degree of "simplicity."

Rotor parts as shafts and wheels are readily served by good class steels or, in special cases, by alloy steels. Generally the required conditions can be met with fair certainty. In many cases, however, cast steel has been used for wheels, although this can hardly be said to

* "Deterioration of Turbine Blading," see *Engineering*, May 4th, 1917.

be common in high-power plants. With cast-steel wheels subject to fair running stresses there must always be a slight feeling of unreliability, and founders are not themselves always too certain of the excellence of their productions in this line, as the casting of thin wheels of varying thickness is not too simple. Up to medium disc stresses cast steel may be used with some assurance, but not where there is any chance of vibratory stress in addition. Mr. Baumann tabulates a definite case of wheel failure due to the use of cast steel where vibration occurred; and where the wheels had to be replaced by forged discs.

For casings cast iron is the usual material up to temperatures of, say 450° Fahr., but above this it is often unsatisfactory owing to growth and deterioration. This moderate limit for cast iron was one of the early lessons of turbine experience, and it is now fully appreciated. A great many of the troubles described by Fenwick would never have been shown if this simple fact had been fully realized at the time the turbines he dealt with were constructed. While higher temperature limits than that stated are said to be possible to cast iron if care is taken to keep down the silicon content, cast steel is the material in general use for the high-temperature parts.

Casing difficulties are not in any way due to material, but are generally to be credited to distortion or to the troubles incident in maintaining steam tightness. To counteract distortion tendencies calls for symmetry and freedom; for casting, uniformity of thickness and simplicity of form are demanded; and for tightness the joint arrangements must be simple and adequate. None of these requirements is made easier by the present movement towards high pressures; and in one of the recent plants using a supply pressure of about 400 lb. per square inch the high-pressure cylinder joints were exceedingly difficult to maintain. Cast iron is the most usual material for diaphragms but, again, above the stated temperature limit steel must be used; and generally, high-pressure ends are entirely of steel. Bronze castings have also been used for high-temperature-stage diaphragms and nozzle plates—at least in some marine constructions. With different materials for casing and diaphragm fairly large allowance must be made for relative expansion; thus, with bronze castings, the radial diaphragm clearance has to be large, and special centring may become necessary to avoid upsetting the clearance at the shaft. This adds complication that could conceivably lead to trouble and, on the whole, but little advantage is to be expected from the use of different materials.

It is, however, in connexion with blading that the quality of material is most severely taxed. There have been two tendencies—or rather impulses—noticed in the evolution of blading materials; one arising from the desire to avoid corrosion by the use of non-ferrous blades that, however, weaken decidedly at high temperatures; and the other from a somewhat regretful but compulsory employment of high tensile steels that are actually corrosive.

The blading problem is well exemplified by the bare enumeration of the ideal qualities. A non-corrodible metal is required that can stand the action of high-speed, high-temperature steam jets, the while it is subject to severe steady stress, repeated bending stress and probably vibratory stress. It must possess a good elastic limit at high temperatures, retain a fair degree of hardness, be able to resist erosive influences, and provide a good surface. It has to lend itself to ready methods of manufacture that are certain in their results and ensure freedom from liability to surface cracks or internal defects. It is obviously a specification of no mean order, and the tendency to blame the metallurgist for its non-appearance postulates an inability to recognize that Nature herself has limitations.

It is clear that blading deterioration and failure are always possible from corrosion and erosion effects, since any consumptive or destructive processes of the kind lead ultimately to critical weakness. Even where actual blade failure is not likely, there always exists a chance of the blade form being spoiled, or of the shrouding coming loose, due to corrosion of the thinner parts. The avoidance of corrosive effects requires very special care. Since it may arise in steel blading simply due to the humid internal atmosphere caused by steam condensation in the casing during stand-by periods, it is obviously wise to ensure that the turbine is left in a dry condition; also, to take precautions against leakage of steam into the casing. Again, impurities—such as chlorides—in the feed or the presence of small quantities of carbon dioxide or oxygen in the steam are active causes in all cases and demand that every care should be taken to keep the feed pure and, by the employment of a closed feed system, free from chances of absorbing external gases.

Unlike the working surfaces in steam engines, the internal parts of turbines are not liable to oil coatings. Whether this is an advantage or otherwise it is a little difficult to say; probably it will be found that the oil film is no great drawback, even with boiler effects in view. Thus, Baumann states that it has been found beneficial to inject a little kerosene into the turbine steam for this purpose, while a definite case recorded by Fenwick is of value. In this, some blading that showed a relatively long life was supplied from boilers that also fed reciprocating compressors, and the oil passing over from these seemed to be carried into the turbines and formed a protective coating on the blades.

Erosion is caused by solid particles in superheated steam and by water particles or masses in saturated steam. Hence it may occur in both high- and low-temperature blading, and it would appear necessary to give careful attention to the adequate cleaning of the steam supply and to the drainage of water through the low pressure stages. Neither process is very certain, or necessarily complete, and some erosion will nearly always occur. The edge roughness that represents a moderate degree is very common; but when the effect is severe on a material that is too soft the blades may be cut to half width or so—as if munched out by some order of super-rat.

On the whole it is unlikely that either corrosion or erosion can get too far in these days of greater watchfulness and better materials; but anyone curious to learn how keen such actions may be, under suitable circumstances, is referred to Fenwick's photographs; where these do not appear clear in form the examiner may pass to the title of the paper they accompany for an assurance that they represent some kind of turbine blading.

The history of blading is largely a presentation of materials increasing in quality with the steadily growing severity of conditions. It is perhaps desirable to notice that there is always some difference between reaction and impulse turbines in this matter, with perhaps slight advantage to the former. In both, centrifugal stresses are, of course, high, being due to blade speeds; but the action in the reaction type is less hard on the blade than that of the impulse type where the speed of jet projection is very much higher. This is shown by the way in which the brass blades of earlier days well satisfied the requirements of the reaction machine, while they proved hardly suitable for the impulse type, in many cases cutting up seriously and rapidly. Brass is still useful in conditions of moderate temperatures and stresses, or for stationary blading; but, of course, in such conditions there is no real difficulty. Special forms—such as nickel brass—have been at times adopted on account of higher strengths; but, in general, all non-ferrous metals, with zinc as a main constituent, are too greatly weakened at high temperatures to be of wide applicability.

With the advent of superheating and higher speeds the search for metals relatively free from this high-temperature senility was freely pursued. The direct effect of the experience of copper-zinc alloys was shown at one time by the use of pure copper in reaction turbines. It is rather difficult to understand why this material ever came to be actually fitted, as its growing weakness under the annealing influences of superheated steam is easily demonstrated. In one case, with superheat of only 100° Fahr., "end tightened" copper blading was fitted over the superheat range. After a comparatively short service this range stripped completely. The blading after removal showed dead soft and could practically be worked with the fingers. Copper-manganese has also been used but, while superior to copper under prolonged annealing influences, it is perhaps hardly good enough.

Bronzes such as aluminium-bronze and phosphor-bronze have been extensively used. The former is peculiarly liable to corrosion by the action of impurities in the feed and may flake off quite rapidly. It has, on the whole, proved unreliable for all except the lowest temperatures. Fenwick's recorded experiences with this metal are extremely interesting. Apparently the aluminium content disappears from the surface, leaving a brittle copper layer which allows the thin edge sections to chip off like glass. The shape is gradually lost, although the central core may remain fairly sound. The riveted part holding the shrouding may, however, chip off and leave the latter free with, of

course, immediate possibility of damage. It appears certain that aluminium-bronze was too readily adopted in turbine work, and brass is on the whole as serviceable. High-grade phosphor-bronze has now been fairly thoroughly tested on service and has given quite good results. It must be quite free from impurities such as zinc or aluminium. It has been much used in marine impulse work, but, probably, in such application it does not have a very full opportunity to show the weakening to which it is perhaps liable under high temperature conditions.

Copper-nickel represents, perhaps, the most promising non-ferrous group. The lower percentage nickel type (15 per cent. or so) has apparently been used with considerable success on the Continent; while the well-known Monel Metal has been widely referred to as almost an ideal blading material. Of considerable strength, non-corrosive, retaining good qualities of strength and hardness at high temperatures, it would seem to be quite suitable. It appears, however, to be difficult to deal with in manufacture and is liable to contain impurities that reduce its value—particularly carbon and silicon. In 1920 it was stated that it had been used in U.S.A. destroyers with great success. In 1921 a very complete discussion of blading failures in U.S.A. destroyers was published,* in the course of which it was stated that some monel metal blades which had failed contained an excessive quantity of non-metallic inclusions. The 1921 statement, therefore, modifies somewhat the enthusiastic remark of a year earlier; but at the same time it should be noted that the failures referred to are more a reflection on workmanship than on materials.

On the whole, the general unreliability of non-ferrous metals at the highest temperatures has led to the use of steels for inlet-end blading. Similarly their lower "elastic limits," and the incidence of the hard problem of the "exhaust end" in large turbines, necessitate the use of steels at the outlet end also; so that the highest- and lowest-pressure bladings of high-duty turbines are almost invariably of steel; and with short turbine arrangements this leads to the use of steel throughout. Naturally, to take high stresses, and always under the bugbear of corrosion, use was at first made of high-percentage nickel steel and this was frequently employed at an earlier date. One of the most outstanding lessons learned from the continuous service of turbines has been, however, the total unreliability of this metal. There have been many disclosures of trouble due to this; but Fenwick's paper will probably always remain the classic on the subject. The general experience is that the metal becomes brittle and develops fine cracks that form an intricate network; pieces are then easily broken off with, of course, damage to the machine. There are odd cases in which the material has given good service and it was very reluctantly surrendered. Thus, some manufacturers apparently passed to 30 per cent. nickel steel after finding 25 per cent.

* "Low Pressure Turbine Blading Failures in Destroyers, Ducey, *Proc., Am. Soc. Naval Engineers,* see, *Engineering,* October 28th, 1921.

material bad; and probably the reverse movement was made in other quarters. It seems that the cause of failure is not understood by the metallurgists, although we have seen a casual explanation that it may be due to the unequal expansion of ferrite and sorbite.

The steels now used are principally plain mild steels or those with a low percentage (3 – 5 per cent.) of nickel. Generally, there seems little to choose between these for the purpose of nozzle-partition plates; but the latter is preferred for rotor blading on account of its high elastic limit. In most cases the nickel steel is used for both purposes and, although corrodible, there do not seem to have been many complaints; but the probable life of such blading does not appear to have been very fully defined. Fenwick records the excellence of 5 per cent. nickel steel in some Curtis stages, and states that the corrosion had been negligible and that, within a running life of 40,000 hours, no failure had occurred.

The chromium alloy with steel well known under the name of Stainless Steel seems to offer a probable solution of many blade difficulties. It has not yet, of course, been subject to full service tests as is the case with other materials, but lengthy experimental tests on actual turbines have been made. Mr. Samuelson* in 1916 fitted to one wheel blades of phosphor-bronze, nickel-bronze, brass, mild steel and stainless steel. On opening up in 1918 the stainless-steel blades were the only ones free from erosion and corrosion. The U.S.A. destroyer blading failures previously referred to included several cases of chrome-nickel and chrome-nickel-vanadium steels—although there seemed to be fair variation of structure and constituents.

These last-mentioned failures are of service as pointing to the great care required in blade cutting and fitting, as apart from blade materials. It would appear that there was no very high degree of accuracy shown; bent blades were in cases sprung into position, shroud bands were inaccurately cut, and inspection seemed to be faulty. One of Commander Ducey's conclusions to the effect that "blades of good material, improperly manufactured and mishandled during installation are more likely to fail than blades of an inferior grade of material, properly manufactured and installed," seems an adequate comment on the actual evidence; and the fact itself needs no emphasis.

A treatment of blading difficulties would require to be very lengthy to attain a reasonable completeness, but probably enough has been said in this section to show the many troubles insidiously awaiting faults in material or method. That the necessity for watchfulness is now well appreciated and that the materials in use to-day are of a high order of excellence are, no doubt, points of merit, but somewhat counterbalanced by the very severe testing conditions that obtain in modern plants.

Vibration and Fatigue.—All the previous remarks represent faults and difficulties more or less common to all stages of turbine development;

* See *Engineering*, October 28th, 1921.

but we come now to what might be termed the characteristic defect of the modern high-speed machine, viz.: the liability to individual vibration of the main rotor elements. This particular difficulty is serious because it is the outstanding trouble at the present time, and has most definitely suspended progress. It is, on the other hand, characteristic, because the earlier designs with their lower blade speeds did not exhibit the tendency to any marked degree.

The use of high speeds in machinery has at all times been fruitful of trouble. Each increase in the velocity of operation of massive parts has introduced still finer aspects of the dynamics of rotation, and the present difficulty represents the latest acquaintance formed in this department of practical knowledge. The steam turbine has hitherto been responsible for popularizing the ideas, principles, and importance of such things as static and dynamic balancing and the whirling of shafts; that it now definitely emphasizes the practical significance of the vibration of parts, under quite fine and exceedingly rapid rhythmic forces, shows its power to maintain its high reputation in this direction. That the difficulty was not fully foreseen merely reflects the natural hesitancy of belief in any serious vibration of parts of apparently ample rigidity, and a certain reluctance to carry out complicated estimates that appear unnecessary. One of the best examples of this whole matter, showing at once the possibilities and the difficulty of forecasting them, is afforded by the recent alternator failures at the Dalmarnock Station of the Glasgow Corporation. In this case it is stated that the alternator core bolts, under a slight tension, had a free period in agreement with that of rotation. The resultant natural vibration broke down the bolt insulation with disastrous results. The whole conception is quite simple, but the sure avoidance of the trouble in all cases is by no means so easy.

Consider for an instant the usual action in turbine blading. Instead of the perfectly continuous and regular steam action commonly conceived, we must realize that any particular blade is changing its disposition relative to the operating jets with great rapidity; and the blade must undergo cyclic changes of force in the course of a revolution, if there should be any irregularity of jet distribution in the nozzle circle. There are cases in which irregularity of the kind is deliberately created, as in partial admission—either in single- or multi-arc arrangements—or in nozzle governing. But apart from such definite cases there must occur in any jet arrangement, whether complete or partial, certain minor disturbances of form, supply, or direction sufficient to create rhythmic variations in the action.

With partial admission, or group separation, the force fluctuations are quite evident and the action on the blade varies between a maximum and zero at a high rate. Thus, with a single arc and a speed of 3,000 revolutions per minute, there are 50 repetitions per second, while with 4 groups at the same speed the frequency of operation would reach 200 per second. Variations of such an order and magnitude are

obviously critical under conditions of synchronism; and also quite adequate to produce serious results within that limit.

Apart, however, from any question of the vibration caused by such full force repetitions there arises the problem of the ability of the blade to withstand them under all possible conditions. This is probably an effect that—like blade and disc vibration—is not appreciated beforehand; but clearly with nozzle governing on a first stage only, the steam action on the blading of this stage is being greatly intensified as the load decreases. Now, if this stage should be comprised of a Curtis wheel, the blade loading is being increased in largest proportion on the later rows with, therefore, fairly rapid increase of bending stress therein. Assuming fair stress of the kind at normal power, it is conceivable that the low power value may be excessive. The conditions must be of somewhat rare occurrence, but there is a case in illustration. The second row of a two row velocity first-stage wheel in a naval impulse turbine failed at low power cruising conditions with two separate and short-nozzle groups open. The turbine was of small size and the first-stage blading of fair height to suit the large full powers. The blade section was exceedingly stiff and, of course, the blade speeds were low so that blade vibration hardly enters into the problem. At the *lowest* powers the stresses reached the order of 20,000 lb. per square inch at the roots of the bronze blading used. This, coming on material somewhat weakened by prolonged exposure to steam, possibly produced failure by fatigue. The case illustrates the importance of force variations and fatigue action as apart from the vibration effects.

The whole question of vibration, and failure thereby, introduces naturally the corresponding mode of failure by fatigue. Vibration entails alternating stress and such must be kept within definite limits or, otherwise, it cannot be carried for more than a certain number of alternations. In many cases, this allowable number is very high; but we must realize that the rate of repetition or vibration is extreme in turbine parts, so that even a very large number can fall well within the normal life of the blading. That range of alternating stress which is sufficiently low to be carried indefinitely is known generally as the fatigue limit, so that failure by vibration will really depend on whether the vibrating stress falls within or without the true fatigue limit for the material in its working conditions.

At this point there arises the sense of insecurity and ignorance which persists throughout the whole matter. Previous references to blading material will have indicated that the knowledge of ordinary qualities at high temperatures is not too complete. We now introduce a vague action due to unsteady conditions, calling for a knowledge of resistance to fatigue at these high temperatures, and where such fatigue action is imposed on blading already subject to stress of measurable average amount—but hardly of known distribution. We have, in fact, to realize that the necessary knowledge of blading material entails fair understanding of how it will behave after prolonged subjection to high

temperature steam and while carrying steady, repeated and alternating stress. There is clearly little chance of the metallurgist exhausting his subject in the immediate future!

At normal temperature the fatigue limit for non-ferrous metals appears to be related to the steady mean stress by a simple linear law of the type:—

$$F = F_0 \left\{ 1 - \left(\frac{S}{T}\right) \right\}$$

where F_0 is fatigue limit with zero mean stress; S is the mean stress, and T is the ultimate strength. Whether such a law will represent the facts at high temperatures—where the metal weakens—is not very clear; but even if it does it is obvious that a steady stress value, that gives a low S/T ratio at normal atmospheric conditions, may give too high a ratio for safety with the "aged" material. With steels the same doubts as to the influence of temperature on fatigue ranges may not exist, for not only does the metal maintain its strength more fully, but the corresponding relationship entails a parabolic law, thus:—

$$F = F_0 \left\{ 1 - \left(\frac{S}{T}\right)^2 \right\}$$

so that the fall-away with mean stress is not so rapid. These remarks have been made in order to emphasize that the allowable alternating-stress value must narrow as the mean steady stress increases—and in most turbine parts this figure is very high—and further, increase of temperature may have important modifications on the safe range; so that it is quite probable the permissible vibratory stress in actual working conditions is of moderate value and easily attained.

It is commonly understood that, with an excessive fixed stress thrown on a weak or fine part of a section, the material will give slightly by plastic flow and so relieve the overtaxed part, by transferring load to regions better able to withstand it. But with fatigue action this convenient process fails to assist. The highly concentrated stress regions will be those subject to the severe fatigue action, and a step-by-step failure ensues with ultimate complete rupture. This readily explains the danger of awkward shapes which give rise to non-uniform stress distribution, or sharp corners which create local high-stress concentration.

It is to be understood, then, that the alternating stress ranges do not necessarily require to be extreme and, consequently, the vibration amplitude of the part affected may not really be great. It is the superposition of this stress on the normal steady value, concentrated or augmented by faults of shape or quality that causes the trouble; and such simple ideas fully explain blading failure by vibration and fatigue.

One particular case shows the features of the occurrence quite clearly and might be considered typical. The bronze blading of a single-row wheel broke off at the roots—of the usual dovetail form. The wheel was in the intermediate-pressure section and the stage series involved similar blading and very slow gradation of blade heights, so that the

blading on either side of the failure were not greatly different from it. Still, only this one row gave trouble, and it would seem very possible that nearly exact synchronism was achieved in this particular stage by some periodic force. Vibration of the wheel appears to be the most probable reason.

There have been many other cases all possessing the common feature of blade failure showing typical brittle fatigue fractures. Two cases occurring on large turbines in America have been published, and are tabulated by Mr. Baumann. In these cases long exhaust blades were concerned; but the feature of the example described above was the relative shortness of the blade; thus showing that it is not only the least rigid constructions that suffer in this way. As an instance of the smallness of the variation that can cause the trouble one of the earliest instances of this type of failure might be noted.* Vibrations arising in the exhaust end blading of a large reaction turbine caused failure, and were traced to the irregularity of the circumferential distribution due to the stopper pieces used at the cylinder joints! The cure was to increase the rigidity by means of an additional lacing wire. The cure in any definite case is more or less obvious; the difficulty is to foretell the specific conditions that will need to be cured.

Since blade failure by fatigue is, to a considerable extent, dependent on inherent faults, the excellence of manufacturing methods becomes a question of moment in all cases; all defects have a direct incidence on results. Thus, surface cracks are of critical importance and any method of working—such as the cold drawing of monel metal—likely to produce them must be avoided. In the cases narrated by Commander Ducey there were apparently several blades that showed distinct evidence of cracks in the vicinity of the roots where, of course, failure usually occurred. Inspection of the blade for cracks or other surface faults would appear necessary, whilst the rounding of all corners and edges in the region of the root seems desirable.

Although the wheel structure would appear radically different from that of the blades, the same general points of view are applicable, and very similar results have ensued. The most serious turbine wrecks of recent years are directly traceable to the weakness of disc wheels under vibratory stress arising out of synchronism between some natural mode of vibration and an impressed rhythmic force of high frequency. Wheel vibration is a much more complex matter than blade vibration, but its results are singularly manifested in fatigue failures.

The transverse vibration period for any wheel will depend on the mode. Generally, a disc form could vibrate with a series of nodal diameters and nodal circles, although the periodicities involved in the presence of nodal circles raise this form out of the range of practical conditions. The single exception might be made of a vibration with a nodal circle on the rim and the disc undergoing a "panting" action

* See "Mechanical Difficulties in the Evolution of the Steam Turbine," *Engineering*, February 16th, 1917.

about this. It is appreciated that such an event is possible with definite shaft disturbances of fair periodicity; and Mr. Baumann has referred to this possibility, although there would not appear to be any very clear example. The most usual type of vibration must involve one or more nodal diameters; and probably most actual cases will show between one and four such nodes.

When synchronism with any specific mode arises in rotation the deformation wave velocities may be in agreement with the angular velocity, thus maintaining elastic waves in the disc that are actually stationary in space. In the general references to this subject it is usual to emphasize this peculiarity; and apparently the actual evidence is consistent therewith. It is not impossible, however, to have wave velocities that lead to different results; it will depend on the cause and mode of vibration. If due to jet irregularities it is easily understood that the wave may be stationary in space. If, however, it arises from effects outside this, as from the shaft or from external disturbances, the waves might be stationary in the disc or move slowly relative thereto.

The whole subject is of very considerable theoretical complexity, and it is difficult to establish the probable conditions for any given case. In addition to the awkward forms and loading of actual discs it is to be understood that the natural frequencies, an estimate of which will be required in design, depend both on flexural rigidity and on centrifugal forces—and hence speeds of rotation. Also, there is always the possibility of warping of the disc due to temperature changes with, of course, further influences on the natural vibration period. Research on this important question will require to be experimental in its main part as the problem facing the turbine builder seems much too intricate for theoretical attack.

No matter how the vibration is established, or what its order, the fatigue action already frequently referred to ensues, and searches the high-stress zones of the wheel for surface flaw or local concentration. Naturally, local weaknesses or cracks in the material; sharp edges, as at the balance holes or keyways; or surface irregularities arising out of rough machine finish, are all of importance, and nearly all cases of wheel fracture are traceable to some such faults.

There would appear to be very definite evidence that failure by fatigue under vibration can occur in first-stage wheels under the influence of nozzle governing, *i.e.*, by group jet effects. It is hardly natural to anticipate the effect in such place as, usually, the first-stage wheels are of considerable rigidity, but as against this it will be recognized that the periodic impressed forces are large and definite in their range. Mr. Baumann gives two cases of Metropolitan-Vickers turbines in which the first discs burst and wrecked the machines. The cause is stated as vibration on four nodal diameters causing fracture at and from local weakness in the material. The cure is by increased flexural rigidity of the disc. In another case of this first-stage-disc failure, particulars of which we have recently seen, the same result was met by the same change,

but actually a private investigation carried out after the failure showed that the material had an exceedingly low elastic limit, which may have been the main contributory cause; at least, it hardly represents a "local weakness."

As typical of the usual features of wheel failure the following examples, which have been gathered together from various published and private sources by Mr. Baumann, may be briefly stated.

(1) A 15,000 kw. turbine at New Bedford, U.S.A., wrecked through bursting of last disc, with wheel fracture starting at the edge of a balance hole.

(2) At 35,000 kw. set at Chicago on which the 19th wheel burst, causing complete wreck; crack showing from balance hole near rim, holes roughly machined and with sharp edges.

(3) A 15,000 kw. turbine at New Orleans with l.p. cylinder wrecked by failure of last wheel; crack between two balance holes.

(4) A 5,000 kw. turbine at Regina, Canada, completely wrecked through bursting of 3rd disc; fatigue crack developed from balance hole.

This collection of failures shows two points clearly: one the seriousness of any weakness that can assist the fatigue action; the other that, as all four machines are by the same makers, the problem is extremely acute in some manufacturing quarters at least.

The case of the 30,000 kw. set at Philadelphia reported on by Professor Moore and published* fully a year ago may be cited in further illustration. In this case the wreck seems to have been due to fracture of a low-pressure disc, two large and nearly opposite segmental parts coming away. There were apparently no balance holes, but, according to Professor Moore, the fatigue fracture—which is ubiquitous, and seems to be the first thing looked for—was close to a deep tool mark on the middle of the disc. The importance of surface finish in endurance tests has been well established by laboratory experiments but here, apparently, we have large scale and costly evidence of the same fact. In commenting on this failure in an article of later date, Mr. C. H. Smoot† gave the facts of a case very similar in its results, and of special interest in that the two ruptured segments were not the only indications; in addition, a short-length crack extending clear through the disc was shown in another region. In Mr. Smoot's example the wheel was deliberately run until failure occurred; and apparently the actual critical speed agreed fairly with that calculated by a somewhat rough process on the basis of one nodal diameter.

In all, then, the evidence is quite sound as regards the influence of vibration, and rather tragically conclusive as regards effects. It is also definite that weaknesses of form, material and finish are contributory to a very important degree; and the discussion in this section will have made it fairly clear that in these rather serious failures, turbine manu-

* See *Electrical Review*, January, 1922.

† *Power*, December 27th, 1921.

facturers are confronted with a certain difficulty that will require fair patience and much investigation for solution. That the difficulty will be overcome is certain, but the influence on the design and development of the large unit may be much more profound than is at present realized.

Gearing.—In modern high-power gearing we touch a subject showing difficulties in every way as important as the various turbine faults that have been considered. The latter are, on the whole, fairly well understood, and certain clear steps may be taken towards minimizing or eliminating them. But in gearing the main difficulty lies in the fact that the true and definitive point of view is by no means clear; the failures have not been traced to a fundamental fault that can be recognized and remedied.

This is truly shown in the lines of argument that are variously followed in the controversy so well sustained for some time back. We have some engineers blaming the steel makers for the quality of material provided; and these makers retaliating with reflections on the engineers' comprehension of the essential stress actions. We have the school that resurrects the dynamical problems of torsional oscillations; and those who refuse to admit such influences as general or fully explanatory. There is the explanation of pitting as due to the failure of lubrication at the instant of pure rolling action, and the opposing attitude that at this point the lubrication is most effective. And almost a perfect jumble of discussion whirls around such centres as the respective merits of the floating and rigid frames, the D or $\sqrt{D}$ principle in tooth loading, tooth and alignment faults, excessive bearing clearances, etc.

Through the fog of argument only one little ray of light persists. and its colour is red—denoting the continuation of the danger. If, however, a closer examination is made of direction signs, and the illusions natural to fog strictly ignored, there appears a significant fact that seems to have been treated rather shabbily, viz.: that the failures are mostly confined to double reduction gears. This is, of course, well known, but its incidence on the difficulty, and its importance as the sole principal evidence, have not been kept clearly in view. It is not, of course, denied that failure has occurred in single gears; but the essential fact of the very high proportion of trouble in the later system to that in the original must be definitely emphasized. It would be ridiculous here to try to present any adequate criticism of all the various attempts at an explanation of this problem; but for the suitable inclusion of the subject in our general list of difficulties we may endeavour to follow out a simple and logical point of view.

Single-reduction gears have been built in great numbers for naval vessels and are fitted in quite a large number of merchant ships. The original successes of the type have been amply supported by continued experience. Doubt may, at times, have been expressed as to the economical improvements, but there is little doubt regarding the reliability. It was only in a comparatively few cases that this part of

a plant caused uneasiness on trial runs or had to be returned for extensive repair. Consideration of the conditions obtaining in geared destroyers, for instance, under full-power running, gives the assurance of safety, and we have Commander Tostevin's testimony in this matter to show that there has been very little trouble in naval work generally. Double gears, on the other hand, are more essentially a fitting for the merchant ship, and the tendency to use them had become very pronounced by the end of the War. The satisfaction with the single gear is well shown by the fact that in the step forward there was, simultaneously, a fair step up in loading scales. The well-known Parsons gearing coefficient went to as high as 250 for trial conditions. At the early signs of trouble with the new gears it was reduced, while the definite experiences of repeated trouble still further reduced it, until it fell back to the low value of early single-reduction gears. We are more or less at that stage at present, and still the difficulty persists.

Obviously, then, the inherent serviceability of the single gear and the undoubted unreliability of the double gear cannot simply be explained on a loading basis. Something more profound must be at work; but the widely different manifestations in the two cases prove that the weakness must be inherent in the change rather than in the system. Theoretically the conditions of action are not affected by multiple steps in the transmission and, so far, no theory of any factor in the system has been successful in demonstrating a real and outstanding increase in the chances of trouble; and, keeping strictly to the evidence, it is clear that any true explanation must establish such increase as its main substantiation.

The main features of the trouble are simple and well known. Considered generally, there is tooth pitting and fracture, the latter mainly showing in pinion teeth. In cases of fairly successful running pitting seems to tend towards a limit, and, as it is fairly general in high-duty gears, the fault will probably not receive the closest consideration so long as the much more serious tooth failures persist. In many cases fracture would seem to occur at the edge of the helix width, but there are also cases of failure in the more central parts. Apparently, however, the fracture always shows definite fatigue signs. Since fatigue action rapidly discovers weakness of material it might appear tnat the material should be blamed; but since the gear action and its faults are presumably much the same in all cases there should, on this count, have been more evidence of single gear failure. Hence the hypothesis of the poor war-time and post-war material; and while there may be a little truth in this, we can hardly believe that the steel makers have reduced the quality of their output to such an extent as would be necessary to provide a full explanation on those lines.

The trouble—seemingly arising out of a high-speed high-power action—shows closely akin to, say, wheel fractures in turbines; and naturally a welcome must be extended to any explanation based on vibration causes. This represents the theme of, perhaps, the most notable contributions to

the subject, but the impression is common that, in such efforts, we have a treatment of the special rather than the general event. Indeed, the whole of marine experience discounts any belief in torsional oscillations as more than an occasional difficulty, or, otherwise, the very even history of the reciprocating engine would have contained more tales of disaster. That this view is sound is indicated by several of the recent discussions of the matter, while it is also clear that the idea cannot indicate that increase in the "odds against" that would appear necessary.

This is, in fact, the weakness of practically all the different points of view; they do not distinguish the double-reduction gear as especially vulnerable in respect of the quality advanced. It is questionable whether a single one of the many discussions—either implicitly or explicitly—entails a two-to-one chance against the double gear. In spite of such impressive evidence as that of the low "true elastic limit" of nickel steel it could not be held that this was the fundamental cause of disaster, since much the same steel has been used for a long time, and where failure does not occur it is obvious that the gear action is not sufficiently severe to search out the fault in the material. If so then the fault is in the action.

The facts then, generally considered, seem to point to some effect that is enhanced to a considerable degree by the multiplication of the gear. This naturally indicates inherent gear faults, since the only common variable that can increase with intensification of type must lie in the primary characteristics. Now, in tooth cutting, errors are incurred which must be within much the same limits in all individual gear wheels or pinions; there are also errors of alignment that should have standards that fall likewise within certain limits, determined by workmanship. Such errors lead to faults of contact and, hence, to non-uniform load distribution and slight irregularities of action that should be typical of all gears similarly cut and erected, and have limits that, on the whole, are fixed by workmanship.

The question must now be asked: How do these contact faults and irregularities influence the gear duplication? In single gears it was a rule of design practice to keep the calculated contact error within 1 mil. The actual error must, of course, be greater—probably a good deal—but the success of the gear shows that the errors inherent in the action were not beyond the power of accommodation of the gear. Why, then, should the same individual errors be without the capacity of two such gears when conjoined? The answer must be that the more complex grouping of the items spoils the capacity for accommodating errors, or reduces the allowable limit of individual error. In other words, the gear errors have a very definite limit, and a powerful cumulative effect in grouping.

Any irregularity of action will in the actual event require to be met by slight relative displacement of wheel and pinion. With helical gears such displacement involves angular and axial accelerations and freedom for these small movements is necessary. This freedom is easily

possible in single gears, and limited errors therein would be readily met by combined torsional effects and axial pinion movement. But passing to any of the usual double gear arrangements we find that the central part is largely a "locked" element in so far as relative movement to any one set is concerned. The isolation of the separate reductions is, then, not complete and, in fact, from the point of view of gear errors, we do not have two separate single-reduction gears but an entirely new and much more complex arrangement. Even when torsional flexibility is introduced between the first wheel and second pinion, there still remains axial rigidity and the freedom of each set is not absolute but governed by the phase effects of the errors and loading distributions in the two gears. This could lead to high stress concentration—which is of course repetitive and therefore leads to fatigue—and to excessive "shuttle action" of the pinions. The actual failures prove the former, and we believe that the latter has been observed in many cases.

This discussion leads to the conclusion that, for equal safety in the two kinds of gear, the normal tooth alignment and contact errors of the double-reduction types—as commonly arranged—must be reduced well below what is customary in single gears; or, otherwise, the gear arrangement must be built up on the principle of complete isolation of the two gear steps to give, in reality and effect, two separate single gears. Since the error limits of the single-reduction gears probably represent the lowest attainable figures it would follow that attention should be directed to the mode of grouping.

In this section we have endeavoured to emphasize the line of thought along which gear troubles would alone seem capable of full explanation. If, however, it serves to take attention away from some of the side issues frequently raised in this controversial subject, it will perform a useful duty. There can be little doubt that the fundamental cause lies in the very fine limits of error permissible in such work; and it seems reasonable and adequate to suppose that these fine errors have an influence in gear multiplication out of all proportion to the number of wheels.

DISCUSSION.

Mr. R. J. WALKER, C.B.E., *Fellow*: Whilst the Authors of this Paper have give us a very interesting account of turbine failures, I am sure they will not object to a reminder that there is a more pleasing side to the picture. In tabulating failures there is always the danger of losing the true sense of perspective. The instances of failure represent a very small percentage of the enormous output of turbines. The turbine has proved itself an extremely reliable prime-mover in whatever field of work it has been adopted. Very little is heard of successful installations after they have passed through their tests, but the failures always clamour loudy for notice. It is probably right and proper that the attention of designers and builders should be focussed on failures just as much as on losses if the most efficient engine is to be attained. Mr. Walker

The Authors' summary of the causes of failure is only in line in many cases with the problems which the designers and builders are facing from time to time. Criticism is easy: designers and manufacturers usually are concentrating their energies in locating and overcoming their difficulties.

In standard types of turbines most of the difficulties which the Authors refer to in the Paper have been overcome many years ago. In departures from standard types even with present-day knowledge of mechanics and material, there is sometimes an element of experimental venture. At Wallsend we had recently an interesting instance of the durability of a marine turbine—the engines of the "King Edward," the first turbine vessel built for commercial purposes, 21 years ago. In those days the astern power provided was a much smaller proportion of the ahead power than has usually been adopted for a considerable number of years past, and last season it was thought desirable to bring the astern power of the "King Edward" more up to present standards. The l.p. and astern turbines were returned to the works and an alteration made to the astern portion. The blading of this turbine was in perfect condition. There are plenty of marine turbines running of which the same remarks as to the satisfactory condition of the blading might be recorded, and in recent years blading troubles in marine turbines have been practically eliminated.

Turning to that part of the Paper relating to gearing troubles I should like first of all to refer to the statement that the failures are mostly confined to double-reduction gears. The failures in connexion with nickel-steel pinions commenced in December, 1921, and there were further instances of gearing failures throughout last year. There has been a general tendency to associate these fractures specially with double reduction, but the percentage of failures is no greater with double reduction, than with single reduction. There have been a few cases of fractures of teeth in single-reduction gears. The tendency is probably due to the fact that considerably more double-reduction gears have been

Mr. Walker

fitted to vessels since 1921, than in the case of single reduction—that is for mercantile work—during the same period.

Several theories have been advanced as to the cause of these fractures, such as the teeth being subject to pressures of such a nature or magnitude as was not anticipated in the original design, or excessive inaccuracies in the cutting of the gears, or that the material in some way was faulty. The importance of accuracy, not only in the actual cutting of the teeth, but also in the machine for cutting the gears, has been very fully emphasized in papers and in the discussions on papers relating to gearing, read before this and other institutions; and it is satisfactory to note that manufacturers are fully alive to the necessity of this important factor in the production of gears. Assuming reasonable accuracy in the cutting of the gears and a good bearing surface over the length of face, it is difficult to see how any abnormal stress could arise which would cause actual breakage of the teeth. If the material is sound one would expect it to stand a great deal of punishment without breaking, as there is a very large margin of strength to cover possible inaccuracies in the gear-cutting, lack of exact alignment, and shocks due to rough seas, etc.

In the case of the fracture of some of the pinions referred to, especially those with good-running single-reduction gears, the pressures adopted on the teeth are so low as to give a factor of safety of 100 to 1. This has left no doubt in the minds of many engineers that something is wrong with the material. Mechanical tests taken from directly underneath broken teeth appear to be entirely satisfactory. One can only conclude that if the material in itself is in such good condition there must be some internal strains resulting from oil quenching. Investigations have recently been made as to the possibility of producing cracks in nickel steel by heat treatment, the results of which investigations will shortly be published. So far, the results have shown that it is possible to produce cracks and considerable internal strains, even when the steel is supposed to have been tempered.

Mr. Carnegie

MR. A. Q. CARNEGIE, *Member*: I would like to join in congratulating the Authors on the Paper we have just heard. I suppose that, in one way, it is better that a subject like this should be dealt with by someone who is not actually connected with the design and manufacture of turbines, but as the Authors point out, it is much more difficult for them to deal with it fully, owing to their not having access to the mass of data that is available to those actually engaged in the work.

I do not know that I can altogether agree with the Authors' statement that "usually a progressive step is followed by a period of consolidation, and that deceleration is now the more obvious tendency." I agree that the designers of some impulse turbines have undoubtedly overshot the mark, and are facing the most serious difficulties, owing to the large number of turbine disc failures. The remarkable record of failures given at the end of Mr. Baumann's paper, to which the Authors refer, has probably not been read by many members of this

Institution, but the outstanding feature of the table is that while the troubles of reaction turbines have been limited to failure of oil supply, rubbing of blade tips and labyrinth packings, all of which can be called minor and capable of being remedied very quickly, the troubles of the impulse type have been of a most alarming nature, involving the fracturing of discs, and in most cases complete wreckage of the turbines and very serious risk to human life. Mr. Carnegie

The impulse turbine is popularly supposed to have large clearances: there was never a greater fallacy. True, there is usually a generous radial clearance over the tips of the blades, but the clearances in the eyes of the diaphragms are exceedingly fine, and the axial clearance between the sides of the discs and the diaphragms is very small—and, as the bursting records show, extremely dangerous. When the discs begin to flutter or vibrate axially, they come into contact with the diaphragms, and the severe local heating tends to increase the vibration. It must be remembered that the contact that occurs under these conditions is not one between fine edges, but between broad metal surfaces, so that the fusing of the metal and the ultimate bursting of the turbine is not surprising.

In connexion with the disc trouble, I propose to read you a letter that was written by Sir Charles Parsons to the late Mr. F. Sargent, Consulting Engineer to the Commonwealth Edison Company, of Chicago, in September, 1918, which I believe contains the very first suggestion that lateral vibration was causing the trouble in America. It has now been established beyond any doubt, that Sir Charles's judgment was correct.

Heaton Works,
Wednesday, 11*th September*, 1918.

Dear Mr. Sargent,

I have heard of several failures of the discs in large turbines of the Impulse type, and so far I have not seen any explanation which appears to diagnose the case satisfactorily.

We were particularly interested in the article which appeared some time ago in "Power," describing the accident to the large General Electric Company machine at Boston, and we have recently received particulars of a rather similar break-down which occurred in this country to a much smaller Impulse machine.

In our large turbines we build up the drum of discs threaded on to the shaft, and the stresses are very similar as far as I know to those in the turbines that have failed. I should be, therefore, grateful for any information you can give me.

If the keyways fastening the disc to the shaft were square I should expect a dangerous point to lie at the corners, and that a crack would start from the key through the hub—which is the most highly circumferentially stressed part of the disc.

On the other hand, I understand that a segment broke out from the outer part of the disc, and this would point to a weak part of the rim—some sharp corner—perhaps the window through which the blades were inserted.

It however, seems possible that a longitudinal vibration of the rim of the disc may occur at certain critical speeds. These may consist of a

Mr. Carnegie

waving to and fro of the rim relative to the boss in a sinuous curve, which in time would cause fracture.

I made a large molecular air pump in 1913 for X-ray vacua; saw-steel discs 18 inches diameter and $\frac{1}{8}$-inch thick, without teeth, were mounted on a shaft and ran in compartments at 5,000 revolutions per minute. Serious oscillations set up in the periphery of the discs, which then touched the casing and became red hot by the friction and cracked. At all lower speeds, the discs ran perfectly. An ordinary circular saw never reaches its critical speed.

It is possible we think, if we knew the dimensions, that according to the methods of Lord Rayleigh, we might estimate the critical speed of the turbine discs which have failed.

With kindest regards and best wishes,
I remain,
Yours very sincerely,
(*Signed*) CHAS. A. PARSONS.

F. Sargent, Esq.,
1412, Edison Bulidings,
72, West Adams Street,
Chicago, Ill., U.S.A.

I remember having a conversation with Mr. Sargent in this country, shortly after one of the early failures, in which he told me that during the investigations following the bursting of a large disc, it was found that all the balancing holes in the disc had stretched and become slightly oval. Other discs from the same turbine were examined, and they were found to be oval too, but the distortion in the other discs was in the wrong direction. This was rather a puzzling discovery, until some discs intended for turbines then in course of construction (but which had never been run) were examined and their holes were found to be oval too. It was ultimately found that the drilling machine was incapable of drilling a circular hole. I believe that quite a lot of mathematics had been expended upon these holes, before the proper explanation was arrived at.

I would just like to remove the impression conveyed by the Authors about the fine clearances in reaction turbines. Since the introduction of end-tightened blading in 1912, the radial clearances have been of the order of $\frac{3}{16}$ inch to $\frac{1}{4}$ inch, and although they work with fairly fine axial clearances between the blade shroud and the corresponding stationary barrier, the construction of the shaft, even where discs are used at the exhaust end, is such as to make axial vibration impossible, because the blades are fixed either into the solid body of the shaft, or into discs which butt up against one another, and so have no room to vibrate. Even in the dummies and glands where labyrinth packing is used, we allow a radial clearance of not less than $\frac{3}{16}$ inch between the shaft and the metal housing, and any fine labyrinth clearances are invariably taken against fine-edged, soft, metal strips.

In regard to blading, it is probably news to many members present that blades are now being produced at Heaton Works by a rolling process, in which the blade and its base or packing piece are rolled out of one solid piece of metal. The base can thus be serrated so as to be fixed with absolute certainty, and without caulking.

(Mr. Carnegie here passed round two blades made by this process. He explained that no machine work had been done, except where serrations were seen and at the bottom of the base; the blade surface had merely been cleaned up on a buff wheel.) Mr. Carnegie

Blade stripping is a very rare occurrence in reaction turbines to-day —largely due to improved methods of fixing and the better methods of manufacture that are now available—but even if the worst should happen in a reaction turbine, no damage is done to the shaft or casing, and complete repairs can usually be carried out in two or three weeks.

I was looking at a large impulse turbine yesterday, in which someone had considerately left a file and a chisel. These two articles came out of their hiding place when the turbine was running, and will necessitate the replacement of practically every blade in the turbine, as well as the manufacture of an entirely new set of diaphragms, as the nozzles were cast into them.

In regard to the Authors' remarks on material for turbine blading, I would just like to say that manganese copper has given excellent service, but it must not be used where the stresses or temperatures are too high. Stainless steel has to be used in a tempered condition, and will not stand any subsequent heating, such as brazing, without disturbance of its heat treatment. Stainless iron appears to offer great possibilities owing to the fact that it can be used in a fully annealed condition, when it is much less brittle than stainless steel. I hope Dr. Hatfield may have something to tell us about the latest developments in connexion with these alloys.

One other point: the Authors mention on page 426 blade vibration arising from irregularity of the circumferential distribution, due to the stopper pieces used at the cylinder joints. This is a very old trouble, and the proper cure has been found in making the stopper blades of the same section as that of all the other blades. There is, then, no necessity for an additional binding wire.

Dr. W. H. HATFIELD, F.R.S.: I should like first of all to congratulate Prof. Mellanby on what must be considered a most judicious and unbiassed statement with regard to troubles experienced in connexion with turbines and gearing. Personally, I have considerable experience with these things; I am familiar not only with such minor defects as occur from time to time in our own work, but I am also familiar, I think I can say, generally speaking, with the troubles which have occurred up and down the country. Therefore, in speaking to-night, I do not want you to think that I am speaking simply from the experience of our own manufactures, but of the problem as a whole. Dr. Hatfield

Now, I should like to emphasize what Mr. Walker has said in this respect, that when you consider the application of turbines to reduction gearing you must bear in mind the successes as well as the failures. When you have in mind the large number of sets in operation, the

Dr. Hatfield

percentage of failures is relatively small. Looking through Lloyd's annual report this year, I think there are something like five million tons of merchant shipping afloat, and, speaking now particularly of gearing, we who are in touch with this subject know very well that the number of failures may be counted on four hands.

It is, however, necessary that we should consider very seriously these failures which have occurred. I know some of you gentlemen do not agree with the views I hold with regard to the failures of reduction gearing, but I have here eight slides which would have left you in no doubt as to the accuracy of my views. Unfortunately, I cannot show these slides on the lantern. You will have to take as read a good deal that the slides portray; but, speaking first of gearing, I would like to say very definitely—I will not say dogmatically, because I can hardly be dogmatic as a scientific man—but I am in complete accord with the Authors in their diagnosis of the origin of the trouble of reduction gearing.

As a metallurgist, and observing the use to which you put steel in your turbines and reduction gearing, I can only say that I am amazed at your daring. Just think what you are doing. It is fundamentally important for an engineer, if he is constructing a device which is to be highly stressed, that he should know the materials and know the stresses he is imposing upon those materials. Now, in the first place, it must be conceded that from the mathematical side alone the subject is not worked out and you cannot know, as Prof. Mellanby has pointed out, what the stresses are which you are imposing upon the material. At the same time you can approximate to knowledge on the subject. A man should also know the properties of his material, and that, I think, is what I have been asked to speak upon to-night.

Numerous gentlemen have discussed this question of the stressing of reduction gears with me, and I think I am safe in saying that it is their view that the teeth of a pinion are never theoretically stressed if the designs are successfully carried out, to more than $1\frac{1}{2}$ tons per square inch. Now, it is quite clear these designers are using a high grade steel, which is a strong ductile material, and they are including the factor of safety at 30 to 1. Assuming that the designer carries out his designs accurately, and the metallurgist makes the steel satisfactorily, we are up against the problem that steel is asked to resist minor stresses, and yet these very important units fail in service.

What is the explanation? The Authors speak of inaccuracy in technique. Gentlemen, in my opinion, that is the cause of your troubles. Had I been able to show you the slides I have with me I would have shown this one. It is a pinion in which every fourth tooth is broken off; the teeth in between are badly mauled. Now, it is quite clear that the ability of the steel maker is hardly equal to the task of making every fourth tooth defective. In this case the cause of the failure was found to be unsatisfactoriness of technique. Again, I would have shown you another slide, which shows how the fracture actually takes

place at the root of the tooth; and this slide, which is a micrograph, showing that the crack travels through the mass of the steel indifferent to the particular constituent with which it comes into contact. There are no sulphides, and yet the crack is propagated from the root of the tooth. I would also have shown a slide, in which we have a large pinion —the slide which the President is now looking at shows inefficiency of technique, and that stresses beyond the designer's intentions had been imposed upon these teeth. Dr. Hatfield

Most of you have seen these teeth that have been broken; you have seen that the fracture is a brittle fracture, and a labourer can come and put a sledge hammer on the next tooth and bend it over. Now, how does that come about? I was for some time at a loss to understand it, but it is my business in life to carry out investigations, and we set in hand some repeated-blow impact tests. We have a machine—in its original form it was a Stanton testing machine—in which a hammer 4·7 lb. in weight fell from a height of 2 inches on to a test piece which rotated, and the blow was given every 180 degrees. We designed a test piece which had a rectangular central portion, and on the under side we cut a notch which bore some relationship to the radius between the teeth of the reduction gearing. Now with that test piece held firmly in position without rotation, we let the hammer fall at the rate of 90 blows per minute on top of the notch. On a two-inch fall we broke the specimen in 17,000 blows. By decreasing the height of the fall on a different test piece cut from this material, we found the number of blows required to produce a fracture became naturally much larger, but the fundamental fact which I wish to get home is that although a two-inch fall broke that test piece after 17,000 blows, we were still able to break the test piece from the height of a quarter of an inch, but after a million or two blows; that means, speaking relatively, that relatively slight hammering action in connexion with these teeth is going to cause failure.

And what produces a hammering action? It is, of course, inefficiency of technique from the engineering point of view.

I do not think I will say much more in connexion with that particular point, except that I consider I have very definite experimental evidence that shows not only that there has been very distinct inaccuracy of technique, but also the effect which that unsatisfactory technique may have in actually producing a rupture of ductile material, with the result that it fails as a brittle substance. One might say, of course, in passing, that I think it is a known fact that of the 600 warships in service during the war only two teeth broke, and the steel which was employed for these gears was precisely the same steel as that largely employed—almost, I may say, generally employed—in gearing for pinions at the present time.

Distortion Effects.—I came across some three or four years ago, a failure in a turbine in which I was led to believe that the internal stress actually left in the forging during manufacture was in some

Dr. Hatfield

sense a contributory cause of trouble, changes in temperature being responsible for additive stress already in the turbine disc. When forgings are hardened and tempered there is always some residual internal stress, particularly so when water-quenched from the tempering temperature to obtain a high Izod value.

Blading.—I was extremely interested, as one who has had much to do with this question, in what Prof. Mellanby says, and naturally I welcome the views which he holds. I should say that monel metal and stainless steel are perhaps the most likely materials for blading. Monel metal has a very low yield point and fatigue range, and in highly stressed blades that would be a serious matter.

With regard to cast iron, I would like to ask for a little information. In the course of my scientific experience I have carried out many investigations with regard to cast iron, and I have always kept a large question mark in my mind wherever I have read of the growth of cast iron at temperatures so low as 300, 400 or 500° C. I say it with much diffidence, but at the moment I am not prepared to accept as a fact that cast iron grows at such low temperatures; that is, that it decreases its specific gravity, for that is the test of growing. I am quite prepared to accept that a casting, generally unannealed, when it comes into service tends to deform, and I should appreciate it very much if the Authors can give any definite evidence of the growth of cast iron.

In conclusion, may I ask you, speaking now more as a metallurgist, to study steel in a little more detail than perhaps some of you have been wont to do. I know you buy steel and you are content with the maximum stress as 45 tons and the elongation as 20 or 30 per cent. Well, gentlemen, the maximum stress and the elongation and even the yield tell me very little and give you very little data upon which to base your designs for work such as that which we are considering to-night. It is necessary if you are to have anything like a complete knowledge—anything like a sufficiently complete knowledge for your purpose—that you thoroughly understand the fatigue range of the various steels with which you are dealing, the fatigue range under alternating stresses and under such conditions as the tests which I have described; and it is particularly desirable that you should do so because I had this matter emphasized to me some time ago in visiting large turbine works. We were considering a rotary shaft and we estimated the reverses of stresses which could be anticipated as taking place in this shaft within its normal lifetime, and it was expected that it should stand alternating tension and compression three thousand million times. You know you are asking steel to do a great deal. Steel is a commercial substance: it cannot be considered as a homogeneous substance. It is distinctly heterogeneous, and I am sure that Mr. Walker appreciates that fact. Nevertheless, as engineers you are using a heterogeneous substance as the basis of your manufactures, and you have no right to treat it as anything else. But learn to know what it will do and learn to know the stresses which you may impose upon it, and then I am sure all your troubles will be dissipated.

Mr. CHARLES BLOW, *Member*: In connexion with turbine blading a short reference has been made to stainless steel having been used to a very small extent. I have here a sample of blading which has seen two years' service, and will show what this stainless steel can stand. Mr. Blow

After the period of service mentioned you will notice that the blade is bright with perfectly sharp edges showing not the slightest signs of attack either from corrosion or erosion, and it has bent cold through an angle of 180° without any indication of fracture.

(The sample submitted by Mr. Blow was passed round the audience).

VOTE OF THANKS.

The PRESIDENT, Sir Archibald C. Ross, K.B.E.: I am sure we have all benefited by this very interesting discussion, and when Dr. Hatfield tells us that we should try and find out more exactly what steel would do and how it would behave, my own feeling is that we might possibly ask steel to do a little less by devoting ourselves to improving the technique of double-reduction gearing as much as possible. I think there are certain details which we might improve, and my own firm has for some time past taken a great interest in this subject. On a recent occasion we took diagrams showing the fore-and-aft movement of primary and secondary pinions in a large set of double-reduction gearing, and were surprised to see the extent and frequency of these movements, which must produce heavy pressures and stresses on the teeth that might be avoided by more accurate technique. There is nothing to be ashamed of in failures and set-backs which have been experienced, and I think we should not be down-hearted but strive to overcome them. We shall get there all right in time, and there will be more rejoicing over one double-reduction-gearing sinner than over ninety-nine just single-reduction gearings. It is not so much what we engineers and metallurgists aim at that is required as complete reliability from the shipowners' point of view. We have learned a good deal to-night, and I would like to thank the Authors for their valuable paper. The President

CORRESPONDENCE.

Mr. A. F. AINSLIE, *Member*: Less than twenty years ago it had not occurred to anyone to thin the tips of turbine blades and now, to-night, we are told it would be considered ridiculous to neglect that general principle. "Nothing is permanent but change, and the supreme test of wisdom is the willingness to give up an excellent thing for a better." Mr. Ainslie

In a maze of complications it is wise to take short clean steps, and in complex calculations to be guided by physical concept, to which the clearest indications are often to be obtained from dimensional reasoning. For, while there is a distinct and sharp limit to what can be derived from the theory of dimensions, it is an infallible guide in most cases of intricate analysis.

Mr. Ainslie

From this standpoint it can be shown that the practical rule for the loading of gears must involve the circumferential speed as well as the diameter of the pinion, and it is interesting and instructive to consider from this point of view such marine gearing designs as have been published. The load coefficients so found vary from less than fifteen to over ninety—surely an amazing variation in the value of this fundamental coefficient—and it is not surprising, therefore, that troubles have been experienced and cases known where the widths have been as much as five times too great. It is quite certain, too, that some recent gearing sets could have been made one-half—perhaps one-third—of their weight and cost.

An inspection of these published particulars also shows how the surface speed was increased, and this, and the improvement in the accuracy of cutting, are the main reasons probably for the rapidity with which single-reduction won its position and proved its reliability.

Mr. Calderwood

Mr. J. CALDERWOOD, *Graduate*: I wish first to thank the Authors for their very interesting Paper dealing with a subject on which very little information is available. On the whole, one must agree with them as to the causes of failures that have occurred, but they give a very exaggerated idea of the probability of failure of large turbines.

The Authors' remarks on the subject of gearing do not appear consistent with the earlier part of their Paper, for, after having dealt very fully with the danger from vibration being greater in a turbine than in a reciprocating engine, they dismiss the probability of dangerous torsional oscillations because trouble from this cause has not frequently occurred with reciprocating engines. A consideration of the possibility of dangerous synchronism between natural frequencies and forced vibrations shows that in a double-reduction-geared ship the likelihood of such synchronism is at least ten times as great as in a ship with reciprocating machinery, and about three times as great as in a single-reduction installation. A comparison of the percentage of gear failures and reciprocating-engine-shaft failures will, I think, show them to be roughly in the above proportion. I do not suggest that all gear troubles are due to torsional oscillations, but I think that the majority of failures that have occurred could have been avoided had the system been so arranged as to eliminate any possibility of the occurrence of resonance at any speed near to that at which the machinery would normally be running on service. This view is confirmed by the fact that all cases of gearing failure, in which the natural frequencies have afterwards been calculated, have shown resonance to be occurring at the normal speed. For example, in the "San Fernando" the frequency of propeller torque variation synchronized with the lowest natural frequency of the system. In the "Melmore Head" the speed of rotation of one of the high-speed pinions synchronized with a natural frequency. No statement as to the cause of failure in certain of the Cunard liners has been published, but it is remarkable that two ships in which normal

tooth pressures were used were successful while two similar ships in which the tooth pressure was reduced have experienced serious trouble. It seems to me that the only possible explanation is that the increased size of the gear wheels in the latter case so altered the natural frequency as to cause one of the critical speeds to coincide with the normal running speed of the machinery. Mr. Calderwood

If the Authors calculate the magnitude of the forces caused by the summing up of tooth errors, which they suggest as the principal cause of trouble, they will find that these are not great enough to cause dangerous stresses unless their effect is magnified by resonance. The dangerous range of speed when such resonance occurs can only be reduced by the use of flexible shafts between turbines and high-speed pinions and between first- and second-reduction gears.

It is probable, as the Authors state, that a certain amount of trouble has been due to the greater difficulty of alignment of double-reduction gears. This difficulty is best overcome by the use of separate gear boxes for first- and second-reduction gears with flexible shafts connecting them.

Prof. A. G. CHRISTIE (Johns Hopkins University, Baltimore): The Authors have presented a very comprehensive review of turbine troubles. It contains thoughtful analyses of the difficult cases with which the engineer has had to contend. One finds it easier highly to commend the Paper itself than to add materially to the conclusions presented therein. The following comments must, therefore, be considered merely as a contribution covering certain American experiences. Prof. Christie

Distortion is still a very serious source of trouble, particularly with our larger turbines. Operators have found that very definite steps must be followed in warming up a large turbine and putting it on the line. This takes an hour on many large units whereas some of the old vertical Curtis turbines could be started from cold and put under load in less than ten minutes. When high pressure and temperatures over 650° Fahr. are used, efforts are made to maintain constant load on the turbine. Sudden changes of load or complete loss of load on throttle-governed units, may result in the delivery of highly superheated steam to low-pressure sections which were formerly at relatively low temperatures and serious distortions may easily occur under these conditions.

The stresses imposed on the casing by pipe connexions are now quite well known and one can adequately provide for them. Flexible expansion joints of rubber between turbine and condenser are now widely used in America in preference to bolting these tightly together and supporting the condenser on springs.

Larger clearances in labyrinth glands are now the rule and greater freedom from trouble is secured at the expense of a slight sacrifice in efficiency.

Cast iron is not used in America where temperatures exceed 450° Fahr. Cast steel is largely replacing cast iron as a diaphragm material. Defective material in discs and blade rings has caused serious accidents.

Prof. Christie

One leading manufacturer uses a method of magnetic exploration to locate hidden defects such as slag inclusions, seams and other injurious conditions in the turned discs.

Blading is still a source of trouble from both corrosion and erosion. Corrosion has been particularly noticeable during periods when the turbine is standing still with some steam leaking into it. American engineers do not care to inject oil with the steam because it decreases the effectiveness of heat transfer of the surfaces in the condenser. Erosion has been generally due to wet steam. One case has been reported where the erosion was traced to the shot-gun effect of boiler compounds carried over by the steam. This will be eliminated in newer stations where distilled water from evaporators is used for boiler make-up.

Monel metal and 5 per cent. electric-furnace nickel steel are widely used in blading subjected to high stress. Nickel-copper alloys and phosphor bronzes are used in certain Parsons blading. The Authors refer to the failure of aluminium bronze as a blading material. This may have been due to boiler compounds for dummies of aluminium are very quickly destroyed in plants in our Middle West where alkali water only is available.

Operators make frequent over-speed tests to see that the emergency governor is functioning properly. This has a very serious effect on certain classes of low-pressure blading as it may cause the blade stress to exceed the elastic limit and the blades to take a permanent elongation thus decreasing end clearance. This is quite liable to occur in old blading or with over-speeding, particularly on sudden loss of load with high initial steam temperature, when high-temperature steam is admitted to the low-pressure sections. It is well known that the elastic limit decreases with increasing temperature. Hence elongation may also occur from windage heat if the turbine is allowed to be driven by the generator for some time after the steam flow through the turbine has stopped.

The vibration troubles encountered on many of our large turbines have been stated elsewhere. The reasons for these difficulties are now better known and means of overcoming them can be developed more intelligently. Devices have been developed to vibrate the discs by means of a solenoid both when revolving and when stationary and to measure the amplitude of this vibration. Positive methods of determining critical speeds have also been developed. It is now recognized that wheels must not be run at speeds near their critical speed. The cross-section of discs has been greatly increased and balancing holes have been placed nearer the hub. Great care is taken to turn the discs smooth and to round and polish the edges of balancing holes. Early failures occurred in discs with four nodes where the fatigue cracks ran deeply into the disc. Heavier discs with six or eight nodes produce failures principally in the blades themselves.

Considerable study has been given to the vibration of Parsons blading and lacing wires are now placed in more effective positions on the blade than in earlier turbines. One manufacturer has carried on

exhaustive tests on the vibration and stresses in Parsons blading built up in various ways in order to eliminate trouble from fatigue. Prof. Christie

Lubrication of large turbines has been a source of much study in America. Our wide variety of oils and methods of refining, make this a complicated problem. Progress has been made in studying the influences of acidity, emulsification and sludging as related to the useful life of an oil in turbine service. Standard specifications and tests for turbine lubricating oils are now under consideration.

Many reduction gears have been built which gave splendid service. Others have been very noisy and have fitted badly. A satisfactory solution of this problem does not appear to have been found. The writer believes with the Authors that greater accuracy in tooth generation will aid materially in solving this problem. The Authors' proposal to break the double-reduction gears into two single-reduction gears is interesting and should warrant a trial.

The art of turbine construction has made rapid strides in America during the last few years. A very hopeful factor is the extensive research work that has recently been carried on by our large companies. The results of these investigations must soon be reflected in improved designs and in better operation. However, our power-plant constructors are demanding still larger and larger units which will continue to tax the ingenuity of our designers. The most careful consideration must be given to all possible causes of trouble and this Paper under discussion will be of material assistance in such designs.

Mr. S. S. COOK, *Member*: The Authors in their reference to failures in gearing give an admirable discussion of the various possible causes that have been advanced, but I think they are in error in their conclusion that the root cause of the trouble is the axial rigidity of the intermediate shaft. This shaft has only to move to accommodate the errors in the secondary gears and these are either extremely small or of low frequency. It would be necessary to postulate both very large end movement and a high frequency of such movement in order to arrive at stresses sufficient to produce fracture. If the Authors' explanation held, the difficulty could be relieved and probably overcome by the simple expedient of reverting to the larger spiral angle of 45°. But their condemnation of double reduction is not warranted by the facts. The most striking fact is that these failures are all of recent occurrence. They started in December, 1921, and, curiously enough, with a single-reduction pinion just previously substituted in a gear which had been running successfully for 8 years. There have since that time been three other instances of single-reduction failure by tooth fracture, and it is rather striking that this represents a larger proportion of post-war output in single- than in double-reduction gears. Double-reduction gearing has come into prominence in connexion with this phenomenon because at the present time its output is much greater, but in proportion to their respective present rates of output the failures are actually fewer in double-reduction than in single-reduction gears. Mr. Cook

Mr. Cook

While not agreeing with the axial rigidity hypothesis of the Authors, I am prepared to agree that the effects of errors are likely to be more pronounced in double-reduction gears on account of the higher inertia of the turbines behind the gears, and if the failures were due to stresses arising from errors in gear-cutting one should indeed expect a higher proportion in double-reduction gears; but, as I have said, this is not found to be the case. What we have to seek is not an hypothesis with, to use the Authors' phrase, a two-to-one chance against double reduction, but an hypothesis that will explain why, in recent output, of both single- and double-reduction gears, there has been such an increase in percentage of failure.

Mr. Dowson

Mr. R. DOWSON (Messrs. C. A. Parsons and Co., Ltd.): The discussion by Prof. Mellanby in his Paper on turbine troubles and their causes seems at first sight a rather damaging indictment upon the status of the modern steam turbine as a reliable prime-mover, and it is likely that it will be interpreted by those empowered to purchase machinery or to sanction its purchase (although not themselves engineers), as amounting to a condemnation of the high-speed rotary engine. Fortunately, the economic value of the steam turbine to-day, and the total absence of a rival for large outputs, makes any thought of renunciation of the system practically out of the question; but the troubles encountered with mechanical gearing have forced purchasers to remind us that this mechanism at any rate, has not yet come to be regarded as indispensable.

It appears, further, that the position of the steam turbine in the field of prime-movers has become so firmly established that not a few manufacturers, dazzled by the prospects of good business to be derived from the sale of turbines, have been guilty of under-estimation of the difficulties to be faced in successful turbine manufacture, and their engineers have not sufficiently appreciated the lessons learned by the pioneers. The early researches of Sir Charles Parsons established the principle that where two surfaces, which are close up to one another, are to have high relative velocity between them, one of them must be serrated to give fine "contacts"; and de Laval showed that the proper design of a disc or wheel intended for high peripheral speeds, is one in which there are no holes at all—either central or otherwise. If these principles were found to be essential in the early days when only small turbines were being made, how much more essential are they to-day, when much larger machines are being built? And yet we find many designs incorporating large-diameter wheels, running close up—at the rims—to massive diaphragms, and having large balancing holes through them. In addition, there has been the unexpected difficulty with lateral vibration, the discs being designed to withstand centrifugal stresses, but apparently without any view to obtaining lateral rigidity.

There are certain valuable features of the disc and diaphragm turbine, which have gone far to popularize it, but some of these very features have been indirectly the cause of a lot of trouble; for instance, the inherent

shortness of the design owing to the small number of stages, has led certain designers to altogether unwarrantable attempts to accentuate further that feature by flimsy construction. Mr. Dowson

The failures of material that have been encountered are otherwise mainly concerned with the blading, but it appears that much blame has been put upon the chemical and physical properties of the materials used, to the total disregard of well-known principles of mechanical design, which are, that there must be no sharp corners, and that too abrupt change in shape or cross-section—even with good radii—is wrong.

Such fundamental principles of design should by now be well understood by all responsible for mechanical structures, as they have long since passed out of the category of trade secrets, and failures attributable to their non-observance must be looked upon as a reflection upon the whole turbine industry.

Mr. A. H. FITT (Messrs. C. A. Parsons & Co., Ltd.): The Authors of this Paper are to be congratulated on tackling a very difficult and important subject. They have collected together a great deal of information referring to troubles met with in the operation of steam turbines, particularly large sets, and have put forward possible causes for these troubles, and, in a number of cases, suggested remedies. The result of a paper of this description, and the discussion, can only be to the good of the turbine industry generally. As the Authors emphasize, the turbine designer with the present-day demands for large units of maximum economy, is continually meeting problems that are, in the main, new, and very difficult of solution, with possibly no previous experience of the particular condition on which to base decisions. Mr. Fitt

Steam pressures are ever creeping upwards, and with them temperatures, although total temperatures seem to have reached a maximum of about 700° to 750° Fahr. This limit of temperature is, more or less, imposed by our existing knowledge of materials. With this temperature, and the high speeds now common practice, materials are stressed in some cases to very high limits, and a recognition of these facts should lead to a most interesting, informative and helpful discussion.

If one word of criticism may be directed to the Paper, it is, the writer thinks, on the score of undue pessimism as to the present tendency of turbine design. The Authors state that the rapid developments of the past few years have overshot the mark, and deceleration is now the more obvious tendency. As far as the firm with whom the writer is associated is concerned, this is far from representing the facts of the case. Progress has been steady, and there has been no looking back, so that to-day, they are building turbines for larger outputs, and for higher conditions than ever before in their history. This, then, should show that the Authors' statement does not hold good in all cases. There have been instances where apparently the designer has taken liberties with his materials, and run at such speeds that the materials have been stressed beyond the limit, with disastrous results.

Mr. Fitt

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This trouble has been particularly acute across the Atlantic, where peripheral speeds in general have been in excess of those commonly used in Great Britain.

An instance is quoted of an impulse turbine without balancing holes in the discs—this was surely a very early design, as it is well recognized that there is a certain amount of end thrust, even with balancing holes. A study of the velocity diagrams for the blading of a machine will indicate that the axial forces at inlet and outlet are seldom quite equal. Further, with total admission, a region of lower pressure than in the stage compartment tends to exist between the acting diaphragm and the wheel which will tend to give end pressure unless balance holes are provided. Again, whatever the type of turbine, if dirty steam is used the blades inevitably become, sooner or later, partly choked, generally at the saturation point, and this may seriously increase the axial thrust.

Distortion Effects.—In connexion with distortion effects, it is stated that the cylinder casing is bound down at the ends. This surely is not to be taken literally, as it is the universal practice to allow one end freedom to expand.

M

Attention is rightly drawn to the importance of steam-pipe connexions: it is essential that these be sufficiently flexible to allow for expansion, otherwise trouble in the shape of vibration or possibly damaged connexions, is bound to be the result.

The importance of slowly warming, and running up large sets is now well recognized.

Diaphragms, unless carefully designed to stand the maximum pressure on overload are a possible source of trouble and a wise precaution is to test all diaphragms for deflection before they are built into the machines. Radial clearance is absolutely essential if trouble is to be avoided.

The difficulty of keeping cylinder joints tight with a pressure of 400 lb./square inch, is instanced. Details of the design are unfortunately lacking, but to maintain a joint tight with this pressure necessitates accurate machining, and final scraping of the faces; flanges that are sufficiently stiff, and, finally, adequate bolting.

A great deal is said by the Authors about the material and construction of blades, and the subject is an extremely important one. The evidence from Fenwick's paper can scarcely be called in as applying to present practice, as the experience was gained during the period 1911-1917. The cause of the troubles is well recognized now as being unsuitable materials. It is undeniable that the blades in an impulse machine receive a much more severe test than those in a similar reaction turbine; therefore, the best of material must be used to withstand the stresses and erosive action of the high-velocity steam jets. Five per cent. nickel and stainless steel seems to be the most favoured materials at present.

To maintain blading in good condition and avoid erosion and corrosion it is most important that the steam be clean and free from acids and other impurities. With the highly superheated steam used at the present time, it is no doubt very difficult to keep the steam free from

scale and dust, but steps should be taken to ensure that the steam is as pure as possible when it enters the turbine. This can best be done by a water purification plant where the feed is very impure, and in any case mud traps and separators should, where possible, be placed between the boiler and superheater as they are much better able to deal with the impurities with the steam in a saturated, than in a highly superheated condition. Mr. Fitt

A word of warning is necessary in connexion with softening agents used in boilers—that is soda and similar materials—which tend to make the water froth up. Where frothing occurs the tendency is for mud to be carried over to the superheater and so to the turbine. Again, feed water after treatment should be carefully filtered. The writer recently saw a machine, where this precaution had been neglected with the result that the blading was in a deplorable state due to carbonate of lime being carried over from the boiler. Bronze blades are liable to be attacked by magnesium chloride and calcium chloride. At the higher pressures these chemicals are disassociated, and acid salts given off which have a deleterious effect on the blades. Water, too, is destructive of blading and ample drainage should be provided.

When a turbine is shut down for any length of time it is a wise practice to have two stop valves closed and a leak-off valve between the two. In this way, even if the valves are leaky, the vapour does not find its way into the turbine; hence rust is minimized. Further, glands should be as air tight as possible and this will be further assurance that corrosion will not take place.

In dealing with boiler feed and the effect on blading, it must be recognized that, although the percentage of impurity in the feed water may initially be low, the concentration in the boilers may mean that after a short period a considerable quantity may be present which, unless the boilers are blown down and other precautions taken, may find its way into the turbine and cause trouble. Another possible source of trouble is leaky condensers. This is particularly serious where the cooling water is salt or possibly contains sewerage, or effluents from works. It is a simple matter to test the condenser for leakage, there being suitable electrical instruments on the market, and where the cooling water is impure, it is a very wise precaution to take periodic tests.

Monel metal is instanced as being a very suitable material for blading, and it has in fact many valuable characteristics. The chief trouble is, however, that it appears to have a very variable limit of proportionality. Its use must, therefore, be attended with caution. For high tensile strength the material must be hot rolled and the finished blade must also be either hot rolled or machined.

For nozzle partition plates 3 to 5 per cent. nickel or mild steel may be used. In casting it is essential that, where the plates protrude from the core, they shall be clean and free from rust so as to form a good amalgamation; a loose plate is the alternative. To protect the plates they may be tinned or sheradized.

Mr. Fitt

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The effect of vibration on blades can be serious, particularly if one of the periods corresponds with the running speed of the turbine, when fracture will result. The practice at present is to design a blade so that its period is at least 3 times the running speed of the machine in which it is installed.

Mr. Gardner

MR. F. W. GARDNER (Messrs. C. A. Parsons & Co., Ltd.): In classifying turbine troubles, the Authors have drawn a distinction between those which arise from avoidable errors and those which are the result of inherent difficulties not fully understood, and of such a nature that their effects cannot be predicted. In this latter category they include the effects of distortion under uneven temperature variations. It is difficult to understand why such a distinction should be emphasized, and it is likely to produce the impression that turbine designers are up against a problem in distortion that is not possible of satisfactory solution. There is, after all, just as much scope in the design of a turbine for providing against distortion troubles as there is for providing against, say, faults or imperfections in the lubricating system. Although we cannot absolutely eliminate distortion from materials subjected to severe temperature variations, we can so design the turbine that the inequalities of temperature will be minimized, and we can design the individual parts so that the distortions likely to result from unavoidable temperature inequalities will be of small extent and of such a nature that they will not prejudice the reliability or efficiency of the plant.

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These are admittedly exacting conditions to fulfil, but they are, nevertheless, conditions that can and must be taken into account, and are taken into account by the intelligent application of both theoretical considerations and of the lessons learned from actual experience with plants in operation. It is only necessary to examine the construction of a well designed turbine to see that it bristles with features developed solely to meet these requirements, and in the vast majority of cases they are met quite successfully. These remarks apply not merely to permanent distortion of the turbine structure, but also to the changes and expansions that recur every time the plant is heated up, not only in the turbine itself, but also in connexions, such as steam pipes, which exert constraints upon the turbine structure. Distortion has been responsible for various troubles, and indeed for some very serious failures and breakdowns, but it may fairly be said that these have been due not to an inherent weakness in turbines, but to what the Authors call "errors of provision"—apparent enough when the failure has occurred, and resulting probably in most cases from the subordination of the principle of "safety first" in an endeavour to advance by too rapid strides.

Dummy rings are mentioned as a vulnerable point in reaction turbines, but there need be no cause for anxiety on this score—so far as axial-flow reaction turbines are concerned—if proper precautions are taken in the design. Most cases of dummy trouble have been traceable

to some definite fault, either of design or workmanship. Further, it must be remembered that the dummy portion of the turbine is not in general subject to sudden fluctuations of temperature, such as are likely to cause local distortions. The warming up at starting is gradual, and once hot, the temperature on all the dummies remains sensibly constant under all conditions of load. Moreover, the dummy depends primarily for its efficacy on an axial clearance, which is adjustable, and so arranged that it remains large while the heating up is in progress and is not set to a fine clearance until a steady temperature is attained. Mr. Gardner

In the section of the Paper dealing with blade materials manganese copper does not seem to have received the attention it deserves. Manganese copper was introduced at an early stage when the increase in superheats rendered brass blading unsuitable at the high-pressure end of a turbine, and this material is still very extensively used, and gives excellent service with temperatures up to 600° Fahr. It is probably superior to those copper-nickel alloys with a low percentage of nickel, which seem to have found favour both on the Continent and in America. At temperatures above 600° Fahr. a slow oxidization takes place in the surface layer of metal with a consequent gradual reduction of the blade section which imposes a limit on the life of the blading. Monel metal stands up well under high-temperature conditions, and is well suited for moderately stressed blading. The Authors have dealt, however, with some difficulties with this material, and it is probable that its limitations will prove to be more fundamental than was at first supposed, and although its tensile strength is high in comparison with the other available non-ferrous alloys, more is to be hoped for from ferrous metals for blading where the stress conditions are severe.

Scientific treatment of feed water, and the use of closed feed systems have considerably widened the range of utility of mild steel for turbine blading, and it is essential that as full attention be given to the feed system as to the blading material itself, if a complete solution of the difficulties is to be achieved. This, fortunately, is now generally appreciated by those responsible for the operation of large power stations, and the problem is receiving the consideration it requires. With the improvement of methods of feed-water treatment and their general adoption on the one hand, and with further experience with the use of ferro-chromium alloys for blading material on the other, there is reason to believe that the rather exacting requirements can be satisfactorily met.

Mr. H. L. GUY (Metropolitan Vickers Electrical Co., Ltd.): I would like to congratulate the Authors on the excellent use they have made of the material available to them, labouring as they did under the difficulties which they have recognized and outlined in the opening paragraphs of their paper. May I, however, point out that while there are difficulties in the way of the onlooker in discussing such a subject, there are also difficulties in the way of those who are intimately concerned in the manufacture of steam turbines. Mr. Guy

Mr. Guy

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The Authors are indebted for much of their matter—as, of course, they acknowledge—to the classic paper recently published by Mr. Baumann. In that paper, Mr. Baumann traversed much of the ground covered by the Authors, but, contrary to the impression which this paper and the discussion at the meeting may leave, he did not merely catalogue and describe a series of difficulties, but explained the methods by which we have overcome them successfully. Naturally, since our experience is in the main limited to one type of turbine, since Mr. Baumann rightly dealt with matters coming within his immediate and personal knowledge, since the number of land turbines of the impulse type is many times as great as that of reaction turbines, since such a review of their own special difficulties has not been made by the adherents of the opposite school of thought, the material available to the Authors was not only much more exhaustive in regard to impulse turbines, but was to their hand in a properly collected form. Certain other matters mentioned in the paper have been more recently dealt with in so far as they were encountered in our experience, and again our method of treatment outlined in a paper recently presented by Mr. Jones and myself to the Society of Naval Architects of Japan. I will therefore limit myself to those points that are not dealt with in one of those two publications, and to attention to those matters upon which the discussion indicated that reference is desirable.

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We certainly agree with the advice contained in the conclusion of the third paragraph on page 416. In operating any high-speed machinery, any sudden increase or change in vibration merits immediate investigation. In this connexion, one cannot too strongly commend to operating engineers the alleged motto of the Royal College of Surgeons: "When in doubt, open out."

We also agree that damage arising from leaving things inside a turbine can be classed as "Avoidable Faults." In this connexion, I find it difficult to understand the significance Mr. Carnegie intended to attach to his description of the damage resulting from leaving a file inside a large impulse turbine—fortunately, not one that we are responsible for. If the suggestion, and his belief, are that a file can be left amongst the blading of a 20,000 kw. reaction turbine without very serious damage to the machine, and he is prepared to put his beliefs to the test, I am not only prepared to present a file for the experiment, but to nominate where it shall be placed.

The Authors have discussed in an interesting fashion the problem of the suitability of various materials to withstand satisfactorily varying temperatures and the action of corrosion or erosion. With much that they say we are in agreement. In the vast majority of cases, we have found 5 per cent. nickel steel an eminently satisfactory material for turbine blading. Those of you who are familiar with Mr. Fenwick's paper will remember that, out of a variety of materials, 5 per cent. nickel steel alone gave completely satisfactory results. This does not mean that all steels containing 5 per cent. nickel are equally satisfactory,

or even that the desirable qualities can be ensured by a specification covering its chemical constituents and physical properties. As in the case of most other materials, much depends on the history of its manufacture into bar form, and its subsequent manufacture into finished blading. Mr. Guy

That the practice of injecting kerosene is undoubtedly beneficial providing the right kind of kerosene is used has been amply demonstrated in service. We have carried out a number of investigations in this matter and have found that the suitability of a kerosene for this purpose varies with its iodine value. The relation between the protection afforded in this way and the iodine value is shown by the following values of relative corrodibility deduced from our researches :—

	Iodine value.		
No kerosene	43	24·4	5·6
1·0	0·94	0·65	0·49

These ratios were practically the same for corrosion in all the steam atmospheres in which the experiments were made, although the corrosive elements in those atmospheres differed widely in their nature. We recommend that the kerosene used for this purpose should have an iodine value not greater than 20. Contrary to the impression which frequently finds expression, we have heard of no instance in which the injection of kerosene into a turbine has given rise to any difficulty of any sort in the boiler, nor can we imagine how such difficulty could arise.

I have indicated that in a small percentage of cases 5 per cent. nickel steel does not completely fill the bill, and in such cases the trouble was usually found to be due to the causes mentioned in the second paragraph on page 419, or allied to them.

Because of the promise of entire freedom from corrosion, combined with great resistance to erosion, which the use of rustless steel provided, we became interested in this material from its discovery, and have carried out continuous experiments with it since the middle of 1914. During this period the material has been progressively improved, for while the original material resisted corrosion and erosion in a remarkable way, it was difficult to handle, and those difficulties have only been overcome by continuous research and experiment in the manufacture of the material, its treatment, and use in the shops. At the present time, we have such blading in about 50 machines, in operation; many of them have been at work for a number of years and the results have been uniformly and entirely satisfactory.

In this connexion it may be of interest to mention our experience of its use in marine work, particularly as there is a prevalent idea that it corrodes very rapidly in the presence of steam generated from sea-water. We have carried out a prolonged and elaborate series of tests on the relative resistance to corrosion of 5 per cent. nickel steel and a number of rustless steels manufactured by a number of makers, and treated in different ways, in an atmosphere of steam generated from

M Mr. Guy

sea-water up to a pressure of 200 lb./square inch gauge. Further, definite quantities of oxygen were introduced into the steam. The results of these tests were that the resistance to corrosion of the rustless steels was from 4·6 to 14 times as great as that of the nickel steel, the difference depending on the composition and treatment of the rustless steel.

In addition to these laboratory tests, we have recently inspected rustless-steel blading in some marine turbines which have been in operation for over four years, and their condition was entirely satisfactory. But this could also be said of the nickel-steel blading.

The Authors refer to the problem of disc vibration, a matter which is in some measure dealt with in the two publications to which I have referred, and in this connexion conclude "Research on this important question will require to be experimental in its main part as the problem facing the turbine builder seems too intricate for theoretical attack." You may be interested to know that no knowledge appears to have existed on this matter even "in the seclusion of formal mathematics" M other than the analysis of Kirschoff in 1850 dealing with the limited case of parallel discs stationary, without hubs or holes through them, and without loading at the rims, until the attention of turbine designers was directed to it in about the year 1912. The experiences at that time which were at once diagnosed as resulting from disc vibrations—were unimportant and easily manageable, but directed attention to the matter, so that mathematical and experimental knowledge has grown apace since. The most notable mathematical contribution was that of Professor Stodola, published in the "Schweizerische Bauzeitung" in 1914. Stodola there developed an approximate analytical solution applied to rotating turbine discs based on the work both of Ritz and Rayleigh.

Stodola later developed a more accurate method of attack directly applicable to rotating turbine discs of taper profile loaded with blades at the rim and having hubs with holes through them at the centre. Although this analytical method is intricate and laborious, it has provided a most valuable means of mathematically studying the problem. Some results have been published in the paper by Mr. Jones and myself, to which I have referred. We have also developed experimental methods of determining the critical speeds of actual finished discs, and find the correspondence between test and calculated values remarkably good. We have now at our disposal both a mathematical method of attack and a simple experimental test of our conclusions, and I have no doubt that the time is not far distant when this problem will be relegated by the Authors to the category of "Avoidable Faults."

We turned with much interest to the Authors' remarks on gearing. We think the rhetorical licence they take scarcely justified, and their generalization unfair to some workers in this field. The Authors, have, however, developed a constructive argument in the matter, which, if I have understood it aright, entitles them to admission to what has

been facetiously termed "the Manchester school." Let me relate what these views are, and in this connexion I do not think I can do better than to repeat one of our earlier statements. Mr. Guy

"At the time of our early investigations, no information was available as to the magnitude and disposition of errors in the gear teeth. We therefore found it necessary to design and make gear-measuring apparatus whose readings could be depended upon to within $\frac{2}{10000}''$ . . . . . The errors revealed (in gear cutting) were far greater and more erratic than had been expected. It was clear that variations in angular velocity resulting from those errors would be irregular and not (truly), periodic owing to their disposition and the continually altering phase arising from the use of hunting teeth. A study of these records . . . . . convinced us of the determining importance of these errors. The inertia forces arising from such errors in gears which are really rigidly connected to rotating masses having considerable kinetic energy, can be and are of a seriously destructive order . . . . . These inertia forces can be reduced to allowable magnitudes by providing appreciable torsional elasticity between the points of engagement of the pinions and the rotating masses to which they are attached."

In order to accommodate an error in a certain engagement in the series one of the shafts must be accelerated or retarded. In rigidly connected gears such acceleration or retardation results not only in a considerable increase in pressure on the teeth at the engagements considered, but the resulting angular displacements in the remaining gear trains to which they are attached produce markedly increased pressures at their points of engagement.

A mathematical investigation we made of the conditions in a gear of the design and proportions common at the time showed that the accommodation of an error in, say, the port second-reduction pinion gave rise to a pressure of x lb./inch on the teeth, the forces transmitted to the first-reduction pinion were such that the pressure on its teeth was increased by x lb./inch. In the same way, the pressures on the teeth of the starboard second- and first-reduction engagements were affected by the accommodation of an error on the second-reduction port engagement.

The Authors have arrived at the same conclusion as ourselves, that herein lies the main difference between the single- and multi-reduction gear.

Having arrived at these conclusions, our method of dealing with them was embodied in some gear designs which were completed in May, 1920, in which the gears were accommodated in three separate gear boxes in order to achieve the necessary isolation of the respective trains. In this arrangement, torsional flexibility was provided between the second-reduction pinions and the first-reduction wheels; also between the first-reduction pinions in the turbines. Axial freedom was also provided by slidable couplings between the second-reduction pinions and the first-reduction wheels. Also between the first-reduction pinions of the turbines. A number of gears of these designs have now been at sea for some time, and no difficulties of any kind have been experienced with them.

Mr. Guy

While we have frequently emphasized the ideas just mentioned, we have indicated that there are other matters relating to gears requiring special arrangements for their accommodation.

The Authors have overlooked the fact that we did not join in and, in fact, strongly resisted the general movement towards reduced pressures, because we were persuaded that no corresponding advantages would result. We have throughout used the same material specifications. We have so far been entirely free from tooth breakage, excessive wear, hammering or torsional oscillation troubles in any of the gears we have manufactured.

Mr. Jude

Mr. A. JUDE (Belliss & Morcom, Ltd.): I would congratulate the Authors on the painstaking way in which they have endeavoured to dig out a little history. In spite of their modest introduction I think it is quite a happy arrangement that someone outside both manufacturing and operating interests should place a paper of the kind before your Institution.

I do not think that the scarcity of news of troubles arises so much from a policy of "hush-hush" as from the principle or maxim of not washing dirty linen in public? This is perhaps more our national characteristic than, shall we say, that of America. *Here*, the more spectacular breakdowns do not usually get much farther than a brief "daily-press" notice in which turbines, generators, etc., are all cheerfully mixed up and labelled "Turbine"; on the *other side* a good "bust up" is apparently regarded as good advertisement. After all, why not? The other fellow is in the same boat—and well, you know, they can blame the professor for teaching them the wrong way!

I do not agree at all with the auxiliary oil pump "cut-in" principle. If anything happens to the main oil system, the proper thing is to shut down and find out what is amiss. It might be a burst pipe for all the driver could know until it is too late. Let the thing be thought out rationally and it will be seen that the odds are overwhelmingly in favour of a "cut-in" making a wreck instead of saving the plant.

The cut-in appears to originate from association with some low-oil-pressure systems—5 to 10 lb.—where it may be that under some circumstances it is difficult to know whether or not anything has happened to the oil system, old and commercial gauges close to the zero reading not being very accurate instruments. I have always adopted 30 to 40 lb. pressure and have had practically no trouble at all. I have never yet fitted a cut-in.

The chief difficulty with the Michell bearing is to get the oil into the vital spots. Otherwise it is by no means a panacea for thrust troubles. I have many cases where a single plain collar has done a lot better. On the whole, the Michell has done very well although perhaps not so well as in the propeller-thrust application, when it works in a bath.

With the exception of a few early examples of cast-iron diaphragms between Curtis stages I have provided steel-plate diaphragms in my

designs. These are flat and of a thickness such that the combined deflection of spoke and plate does not exceed $\frac{1}{32}$-inch in small sizes, and about $\frac{1}{16}$-inch in large ones. The blades are usually of standard brass and clipped in without screws or other loose pieces. But we can make the blades of anything, steel, monel, etc., as the occasion may require and they can be renewed very simply and without scrapping the diaphragm. We do not have any trouble at all with these diaphragms. Mr Jude

The practice of making the diaphragm packing abutt the bosses on the wheels is bad. I do not do it. I put sleeves between the wheels and then, if anything happens, neither the wheels nor the shaft are upset by getting red hot, which otherwise they might do.

Nine-tenths of the blading troubles (if they can rightly be called troubles) in modern turbines is due to steam not being pure steam. I do not know that we have any particular right to expect it to be pure, but my experience is that the majority of blades come to an untimely end by being knocked about by large foreign matter, which no reasonable strainer can stop. I have tried a good many steel blades in all sorts of places and conditions, and, generally speaking, the only jobs in which they will stand up for any length of time are those which receive more or less greasy steam. Unfortunately there is generally sufficient other matter to form a sticky mud. Such conditions upset all beautiful calculations and experiments on critical speeds of blade vibration—and so we go merrily round in circles like well-behaved blades themselves.

The survival of the fittest is the 70-30 brass, and it is only high temperature or stress conditions which should, in my opinion, lead one into other more expensive and elaborate materials. Whatever is used it has got to be replaced sooner or later and one should think of customers' pockets on these occasions.

After all is said and done, it is not the spectacular happenings, such as the Authors have mostly described, which annoy—not to say trouble. It is the hundred and one little things which are very largely matters entirely outside theory, design or intention. A large proportion are "personalities." We find just the same statistical thing with turbines as with the thousands of engines my firm has built—troubles accompany men, and often men are marred by place.

Then there are the troubles arising from misfits of various kinds, defective materials, bad design—shall I say silly design—in *little* things; things which filter through all drawing offices both from fundamental and passing causes; things of which we are all guilty at times, a good many arising from our topsy turvy regime whereby drawing offices are regarded as the place for the young engineer instead of for the old. I should say that more engineering firms have been killed off by junior, or rather, young draughtsmen than by any other means.

Then there are the broader things that are the basis or argument—in short, the *raison d'être* of our technical institutions.

Against these things there is no practicable remedy and the root reason is that we are human, we are in a constant state of flux and we are not ideal machines—and do not want to be.

Mr. Jude

Let us who have a hand in design tell the turbine user bluntly that the chance is x to y and therefore a positive value, that wheels will burst, shafts break, thrusts fire, etc., etc., and that he must take his share of the burden. We shall then hear less of that word "trouble" and more of the words "adventure" and "experience."

Messrs. Scotts'

SCOTTS' SHIPBUILDING & ENGINEERING CO., LTD., GREENOCK: In their paper Prof. Mellanby and Mr. Kerr have presented the Institution with a judical summing-up of the causes of turbine troubles and failures, which will form a valuable contribution to the literature on the subject. Coming from gentlemen who are in a position to take a detached and dispassionate view of the subject, their conclusions are all the more weighty.

In dealing with "Minor Features," the Authors refer to ruined bearings due to the failure of oil supply, and emphasize the necessity of a stand-by oil pump which should start automatically. To do this, however, the stand-by pump would require to be hot, and in fact would in practice be kept running slowly with consequent detrimental effect on the steam consumption. But in any case it could only deal with the situation if the failure of oil supply was due to stoppage of the working pump. If the fault lay in a fractured pipe or leak in the system, the presence of the stand-by pump would not prevent disaster to the bearings. In marine practice this situation is now generally met by a governor which performs the double function of cutting out on a failure of the oil supply, and on a rise in revolutions. There appears to be no reason why a governor of this type should not be applied also to land installations, even if its function was limited only to the control of the forced lubrication system.

In the section dealing with "Distortion Effects," the Authors indicate many of the troubles which may arise from this cause, and give examples arising mainly from temperature changes consequent on irregular heating up, rigidity of pipe connections, changes in load conditions, etc. In view of the common present-day practice, in marine installations particularly, of supporting the main condenser from the casing of the low-pressure turbine, it would be interesting to learn whether any distortion troubles have been traced to this cause, and what provision is made to counteract the distortional effects to be expected from such a practice, especially in turbines of the reaction type, where the casing is relatively long and the tip clearances are fine. As in this section also the Authors refer to distortion of the rotor due to local heating, it may not be out of place to mention the case of a single-screw ship where this local heating, with its resulting troubles, was the direct outcome of the condenser-tube failures so prevalent at the present time in ships built within the past year or two. In the case mentioned, as the vessel was engaged on short runs, condenser-tube failures led to frequent testing of the condenser, salt water being used, and in the process finding its way into the turbine casings. An examination of the

rotor after failure disclosed the fact that most of the wheels were coated with a rust scale, in some parts $\frac{1}{32}$-inch thick, and that pieces of this scale had broken off and become lodged in the packing of the diaphragm between the ahead and astern turbines, inducing a frictional resistance which soon resulted in local heating and a slight bending of the rotor spindle. There is no doubt the scale was formed through the salt water not being thoroughly drained away after testing the condenser. There does not seem at first sight to be any connexion between a leaky condenser tube and a bent rotor spindle, but it is there, and it would be of interest to learn whether any other cases of rotor distortion could be traced to a similar cause. Messrs. Scotts

In treating of "Vibration and Fatigue" the Authors deal very fully with what may be the most fruitful and is certainly the most elusive of all turbine troubles, *i.e.*, vibration of individual parts of the rotating element. Many mysterious cases of blading failures are possibly due to vibration of the wheel carrying them, or to a combination of wheel and blade vibrations. At the same time it is to be noted that many of the troubles cited by the Authors under this head have been experienced in turbines having very high blade speeds (in the neighbourhood of 750 feet per second mean blade speed). In marine work such figures are quite unknown, and the chances of vibration troubles either in discs or blades should be minimized. However, a case somewhat similar to that cited by the Authors might be worth recording, in which the fifth-stage wheel of a low-pressure impulse turbine having seven stages in all, in a single-screw vessel, shed its blades on three separate occasions. The blades forward of the stage which failed were of the same section as that stage, while those aft were of heavier section. After the second failure, the blades in the fifth-stage were renewed the new blades being of the same section as before, but slightly shorter in length; but they also failed. Since then the ship has been running for about twelve months with this wheel bare, with no apparent difference in performance, but a new wheel of slightly more rigid design and with heavier blade sections has been provided, and will be fitted at the first opportunity, with, it is hoped, beneficial results. After three failures it was felt that the wheel was somehow responsible, yet it is a fact that other four vessels fitted with identical machinery—three of them sister vessels and the fourth a twin-screw—have given no trouble. As the Authors say, this but emphasizes "the sense of insecurity and ignorance which persists throughout the whole matter."

On the question of gearing every engineer will readily subscribe to the need for the most absolute precision in the cutting of teeth, and in alignment; but when all that is allowed for, these factors cannot account for a first-reduction pinion breaking like a carrot, between the two helices, while the teeth remained intact after eighteen months' service; nor in cases where two or three short lengths of teeth located together in a small patch have broken away after a few weeks' service. In one case of this sort at least, the pinion, since the breaking of the teeth, has been

Messrs. Scotts'

kept continuously in service—about fifteen months—with satisfactory results. Cases like these, it would seem, can only be explained by faulty material or by unsatisfactory heat treatment, which in practice comes to the same thing. On the other hand, absolute precision of gear cutting cannot, even at the very best, be expected. To meet these unavoidable imperfections both of material and workmanship, the solution with greatest chance of success would appear to be, as the Authors suggest, to isolate as completely as possible the two gear steps—to give, in reality and effect, two separate single gears.

In concluding these remarks on this most interesting Paper, it can be said that the Authors' examination of the subject has been fairly complete, and that their conclusions are in general substantially correct.

Dr. Walker

Dr. W. J. WALKER (University College, Dundee): The section of the Paper dealing with vibration and fatigue is most interesting and suggestive, and the question whether the formulæ

$$F = F_0 \left(1 - \left(\frac{s}{T}\right)\right) \text{ (for non-ferrous metals)}$$

and

$$F = F_0 \left(1 - \left(\frac{s}{T}\right)^2\right) \text{ (for steels)}$$

represent conditions at high temperatures is, in the writer's opinion, a very pertinent one. The formulæ, as the Authors state, "emphasize the fact that the alternating-stress value must narrow as the mean steady stress increases." Although, for non-ferrous metals the strength variation with temperature is on the down grade for all temperatures above the normal, and for steels maximum breaking load occurs somewhere about 300° C., it is quite possible that this increase in strength may have little effect in rendering temperature variations of much less importance for steels undergoing stress fluctuations, than for non-ferrous metals, since, accompanying this increase in strength (about 20 per cent.) there is a corresponding diminution in elongation (about 40 per cent.). In Johnson's "Materials of Construction" a case is cited of a railway axle which only withstood one-third the number of blows at 300° C. that it withstood at 20° C. From the writer's observations of tests on metals at high temperatures, the tendency for the metal to take up external vibrations, *i.e.*, its synchronizing capacity (if one may use such a phrase) appears to increase with temperature. This side of the subject does not appear to have hitherto received attention, and is one which might be worth close experimental investigation. It is possible, for example, that the formulæ already quoted might be conveniently given the empirical form

$$F = F_0 \left(1 - \left(\frac{s}{T}\right)^n\right)$$

where n is a function of temperature probably diminishing as temperature increases, and having distinct values for each metal.

MESSRS. G. AND J. WEIR, LTD.: Referring to the remarks as to the use of Monel metal for turbine blading, it may be of interest to the readers of the paper, and to the Institution generally, to know that makers of drawn turbine blading have recently successfully solved the special problems involved in dealing with Monel. Excellent blading is now being produced by a number of British makers, although for some time the best results were obtained by Continental makers who, by studying more closely the special treatment required by Monel and at once suitably adapting their processes, have arrived at the desired results much more quickly than British makers. Messrs. Weir

Some difficulties have arisen in connexion with the character of the hot rolled bar from which the Monel blading is produced, but these have not been insurmountable or of major importance. Occurrences such as have obtained and affected the immediate adoption of the blading have followed two definite lines and been traced to two main causes. In the first place, the longitudinal internal cranks which caused trouble were traceable to the fact that in the earliest stages of the production of Monel practically the whole of the cast ingot was worked up into bar. The procedure now being followed is similar to that which has had to be followed in the case of steel, viz., to cut off a part of the top of the ingot before forging it. At present the amount so cut off has been increased to 30 per cent. with considerable advantage, and, as in the case of steel, this action has resulted in the practical elimination of internal longitudinal cracking.

Surface cracks which sometimes occur in finished Monel drawn blading have been traced to the cracking of the ingot or bar surface, caused by the play of an oxidizing or carbonizing flame, or flame containing sulphur. Such a flame breaks up the surface of the Monel and the cracks so caused persist in the drawn bars to a greater or less extent according to the heat treatment to which the material has been subjected. Special attention has lately been devoted by producers to elimination of surface cracks. The surface of the cast ingot and also, in some cases, of the rolled bar has been turned before proceeding to drawing operations, and the heat treatment when annealing has been carefully watched. This action has resulted in a decided improvement in the finished blading, including the elimination of surface cracks.

It will be interesting and reassuring to users to know that the causes of imperfections which have obtained in some Monel bars have been so definitely located that there is no evidence of inherent unreliability of the hot-rolled or cold-drawn material. Monel hot-rolled bar has been repeatedly subjected to the 120 feet/lb. Izod test, and no case is known where the sample has been broken. 107 to 115 feet/lb. Izod figures unbroken are common results. No case of failure by short-grained fracture, such as sometimes occurs in the bronzes, has ever been known to take place in hot-rolled Monel bar. These facts should have an important effect in giving confidence to those who have occasion to use Monel for turbine blading.

Messrs. Weir

A careful study of the U.S.A. reports of fracture of turbine blading in warships will, we think, be found to show that the initial causes of breakdown were in no case attributable to the failure of Monel under working stresses, but that constructional defects caused the first failure and against the subsequent wrecking forces which resulted Monel stood up admirably, bending double without breaking.

Makers of drawn Monel blading are now, in the finished blading, guaranteeing a minimum of 40 tons ultimate tensile and 33 tons yield, with a 26 per cent. elongation on 2 inches. The above figures are at normal room temperatures. Monel blading has the further special advantage that at a temperature of 750° Fahr. these figures for ultimate and yield would not fail below 34 tons and 22 tons respectively. Turbines driving our rotary pumps and using steam of 750° Fahr. are working with the original Monel blading, fitted to them over three years ago, still in good condition.

[AUTHORS' REPLY.

AUTHORS' REPLY.

We are gratified by the quite considerable discussion that the Paper has created and by the entry therein of so many eminent experts. On the whole, however, the discussion hardly meets our expectations as in the majority of cases there is a lack of that precise information we had hoped to extract. The remarks made are less contributory to the subject —as the circumstances really required—than merely reiterative or critical of the Paper. Excepting, probably, the subject of gearing, the contributors do little more than corroborate or vaguely amplify our general discourse. The Authors

Naturally, there is a reluctance to present exact facts of failures and troubles, but it is certain that the sources of information open to us in compilation are restricted when compared with those available to most of our critics. We may, then, note Mr. Jude's remarks and choose whether this restraint springs from a " policy of hush-hush " or is the result of a " national characteristic " of modesty. The impression derived from Mr. Jude's amusing paragraph is that this modesty is a virtue. If so, it is an exceedingly convenient virtue in this cause, but unfortunately not exhibited in all the operations or policies of the manufacturing world. The blatant public exposure of the details of a breakdown may not suit the manufacturer but, to the scientifically minded, it is superior to the usual meaningless press " sandwich " of news, from which the meat has been carefully extracted—or completely spoiled by condiments.

Several critics take us to task for " undue pessimism " and failure to recognize the great successes in these all important departments of engineering. But surely this aspect of their remarks merely results from their search for opening paragraphs with " bite " in them. It must be quite clear from our introduction that we had, specifically, to deal with troubles; and, consequently, a curtain should be partially drawn over the " glorious triumphs." What is pointed out as a fault here is hardly so. It is rather difficult to picture failures adequately without dulling the main atmosphere somewhat. The procedure is, in fact, artistically essential. One might as well blame a committee report on asylums for making sad reading because nearly 100 per cent. of the population are quite sane; but sane people must surely recognize that a report on asylums must be sad.

An interesting feature of the discussion is noticeable on contrasting Mr. Guy's and Mr. Carnegie's contributions. Here we have the eminent exponents of two different lines talking over the fence that divides them. Mr. Guy seems to us sound in his contention that Baumann's famous paper deals with impulse turbines in an open manner, and with a completeness not equalled by those responsible for other types of design; and we most heartily acknowledge our indebtedness to this fine work. At the same time Mr. Baumann in his treatment and in his " table of

The Authors

disasters" does not in any way hide what he does know regarding the demerits of the other systems. Mr. Carnegie scores in his exposure of the great fallacy of the "big clearances" in impulse work. Those originally attracted by this apparently rational claim probably by this time appreciate its exact degree of righteousness. It is not, however, our place to dissect these details of controversy and award points; but, after noting what Mr. Carnegie has to say regarding the large impulse turbine that endeavoured to digest a file and a chisel, we think, Mr. Guy is quite justified in challenging a reaction turbine to partake of a similar diet. We can only say that if the test is undertaken we should be glad to receive a fragment as a memento; preferably a fragment of the file as being one of the rarer elements of the resulting mixture.

In the details of the discussion we find many interesting points, which need not be gone into fully as they are very clearly dealt with by the various contributors. We might endeavour, however, to review the main aspects.

Mr. Dowson emphasizes the importance of well established principles of design and aptly points out that modern turbine wheels depart quite considerably from the guidance laid down by De Laval. That the departure is almost unavoidable merely demonstrates that the greatest care must be taken in estimating the effects of it. The balance holes in disc wheels represent probably a deliberately introduced weakness, and Mr. Fitt is apparently surprised that there should be cases in which these are absent. Actually, however, their omission is quite common in marine work.

Mr. Jude, with his distinctive touch, tells us how errors and faults are frequently the result of a persistent "silliness" in small things. This, of course, amounts to a neglect of the minor principles; but there are undoubtedly many annoyances that arise as he expresses it out of "personalities."

Messrs. Scotts' and Mr. Jude's contributions both refer to the stand-by oil pump principle and make the necessary qualification that such is only a safeguard against failure of the main pump, but not against pipe failure. Such remarks are sound, and the statements of both parties on oil systems should be noticed. Prof. Christie's exceedingly interesting discussion of American experiences includes a note showing the careful attention apparently being given to the properties and accurate specification of turbine oils.

Mr. Gardner appears to consider that our classification of distortional difficulties is hardly fair. As he sees it these can be as definitely overcome as any other difficulties. We take leave to doubt it. The factors involved are too varied and variable; while the history of the subject shows that distortion from one cause or another has always been a serious difficulty, and still remains a menace. It is true that a well designed turbine "bristles" with features developed solely to meet these requirements, but it does not at once follow that they are fully met. The point

that we raised regarding the similar manifestations in modern land turbines and much earlier marine sets is rather significant in this respect. The Authors

Mr. Fitt is right in remarking that the words "casing bound down at the ends" should hardly be taken literally. The sliding freedom usually provided at one end is not exactly what we meant here by freedom. Apart from the influence of the holding down bolts, both ends are in a sense bound by the heavy extraneous formations.

Messrs. Scott produce an interesting case of local heating arising out of condenser-tube failure showing that real causes are sometimes more profound than they at first seem. Mr. Carnegie's story of the drilling machine that bored oval holes is a further example of the difficulty of hunting for causes. Messrs. Scott also raise the question of the underhung condenser. Truly, this has every appearance of a troublesome arrangement, and yet it does not appear to have proved so in actual practice.

It would be practically impossible within reasonable limits of space to notice all that has been said about materials as nearly every contributor refers to this matter. The views expressed are generally in fair agreement with the remarks in the Paper itself. The favoured materials for the more extreme blading applications appear to be Monel Metal and Stainless Steel. Dr. Hatfield, however, emphasizes the low yield point and fatigue range of Monel Metal and this would seem to be of importance. It should, for instance, be kept in view when reading Messrs. G. & J. Weir's very full and interesting account of manufacturing difficulties and the way in which they have been overcome, although from this account the impression is obtained that this blading material has now reached a high state of excellence and reliability. Judging from Prof. Christie's remarks it is very widely used in America.

Mr. Guy's statement on Stainless Steel shows that it has now reached a stage of development at which it appears highly suitable for blading. His relative figures for corrosion disclose a valuable margin of superiority in this respect; and Mr. Blow's concrete example provides very definite support. Mr. Guy and Mr. Carnegie give slightly different impressions regarding this steel, the latter seeming to hint that its condition is too easily disturbed by necessary handling. We should also have been glad to have heard a little more about Stainless Iron than is contained in Mr. Carnegie's passing reference.

The assurances given by Mr. Carnegie and by Mr. Gardner that Copper-Manganese has distinct merits in cases of moderate stress and temperature are welcome. We had fancied that this material had gone greatly out of use, and are rather surprised at Mr. Gardner's figure of 600° F. as a working limit.

Mr. Jude shows an attachment to 70 : 30 brass for all conditions other than the highest temperatures and stresses. His point of view, that the cost of replacement should be considered, appears quite sound; but the frequency of replacement must be an equally important factor.

A number of commentators emphasize the importance of pure steam

The Authors

and the difficulty of obtaining it. Mr. Fitt's remarks on this and on the influence of water softening agents should be noticed, and attention might also be directed to what he and Prof. Christie have to say on feed-water treatment. This last has become an exceedingly important matter in modern power plants.

Mr. Guy provides very definite information on the value of kerosene injection and his figures as based on iodine values are of great interest. Mr. Jude also indicates the advantage of greasy steam, but qualifies this by his observation on the readiness with which a "sticky mud" forms. According to Prof. Christie the process of oil injection does not appear to be supported by American engineers, seemingly on account of its effect in the condenser; but surely the influence on the heat-transmission rate must be exceedingly small.

Dr. Hatfield appears to be dubious regarding the supposed growth of cast iron at comparatively low temperatures. In this he adopts an attitude similar to that affected by a large number of metallurgists, few of whom appear to have made a serious experimental study of the changes that take place in cast-iron with rise of temperature. From our own experiments, which have been carried on over a number of years, we can assure him that changes in volume, structure and strength—sufficiently great to call for serious consideration from engineers using cast-iron—do take place at the higher temperatures he mentions.

It would seem fairly clear that we have in no way overstated the importance of the questions of vibration and fatigue. Prof. Christie's remarks regarding wheel vibration are worthy of attention as showing the great care which is being taken in form and finish, and in testing, to avoid the troubles due to this cause. It is of course somewhat natural that the study of the subject should be quite intensive where the troubles have been most extensive. The case quoted by Messrs. Scott in illustration of blade failure due to wheel vibration would appear to be covered by Prof. Christie's statement that heavier discs with six or eight nodes produce failures principally in the blades themselves.

Mr. Guy, in dealing with disc-wheel vibration, conveys the impression that the methods of attack on the problems are theoretically and experimentally adequate. We are certainly interested to learn that Prof. Stodola has developed a full and satisfactory method of calculation, and this should be a great aid in design. We must confess to being rather behind in the subject, as Lamb and Southwell's recent paper represents all that we have seen on the theoretical side; and, taken alone, it hardly indicates that the problem is completely solved. Mr. Guy also tells us that difficulties due to disc vibration were encountered and clearly recognized as early as 1912. If so, the general explanation contained in the letter produced by Mr. Carnegie as passing from Sir Charles Parsons to Mr. Sargent in 1918 must have been somewhat superfluous.

Dr. Walker is alone as a commentator on our remarks on the incidence

of mean stress and high temperature on the fatigue limit, and hence of the importance of the latter in connection with high speed parts subjected to high temperatures. The problem is not simply one of the existence of a fatigue limit but of its probably rapid reduction by increasing stress and temperature. Dr. Walker's remark on "synchronizing capacity" should, in particular, be noted. In connexion with this whole matter, however, it is necessary to point out that in Prof. Lea's recent paper* a case is cited in which a very considerable increase of the fatigue limit occurred at a high temperature. This serves excellently to upset preconceived ideas; but we are afraid it does not replace them with anything very definite. The Authors

A considerable part of the total discussion centres on our short section on "Gearing" and there seems no abatement either in the interest created by this subject or in the variety of the points of view thereon. Dr. Hatfield takes the attitude that the gearing is subject to hidden but severe stresses and that "gearing technique" is inadequate. Mr. Walker seems to consider that the material is to blame. Messrs. Scott lean towards the same hypothesis; and their contribution is enriched by a concise reference to actual cases of failure. Mr. Cook admits the likelihood of greater disadvantages in double gears due to normal gear errors, but disagrees with the point of view advanced by us. Mr. Calderwood thinks we dismiss the torsional oscillation difficulty in too contemptous a fashion. Mr. Ainslie believes that a simple use of dimensional theory will give clear ideas. Mr. Guy deplores our "rhetorical licence," but welcomes us as adherents of the "Manchester school."

The subject seems to create such confusion of thought that we feel some form of "licence" is necessary to deal in it fairly. It is certainly impossible to analyse the various statements made. They are practically all—including our own—expressions of opinion and may be collectively or individually right or wrong. The exact details of each case of failure would be required for fair discussion, and the contributions have not provided much in this line. We feel, however, that an impartial reader would recognize that Dr. Hatfield has most ably supported his attitude by definite evidence.

Both Mr. Walker and Mr. Cook bring out that the gear failures are as relatively numerous in recent single reductions as in double reductions; and hence that our charge of inherently greater unreliability in the latter is invalid. Further, it is stated that the failures started in December, 1921. We must bow to the greater knowledge of these gentlemen in such a matter but we confess to some perplexity. There were several years of practically untroubled experience with single gears while the present difficulties coincided with the advent of the double gears. Firms of considerable experience in the line only had trouble with the

* "The Effect of Repetition Stresses on Materials," *Proc.* I.C.E., February 8th, 1923.

The Authors

earlier cases of the later type. To our mind the most pregnant comment on this question is contained in the frequent proposal of s.r. gears where d.r. gears would otherwise be fitted, showing distinctly that builders have more faith in the former. If the chances of failure are the same in both cases why revert to the obviously less economical? Again, if the first trouble of the present series only occurred in December, 1921, why the rather sudden and considerable revision of design procedure of a much earlier date? We believe that there were clear signs of trouble eighteen months before the date given. Perhaps only what Dr. Smith has called "roarers" were experienced at first; but whether "roaring" or "crashing" there seems to have been quite sufficient noise to intimidate design long before the end of 1921.

Taking the general evidence as available we must admit a difficulty in believing that the failures of the last year or two can be entirely—or even largely—due to poor material. It would surely be a strange coincidence if a conspicuous deterioration of a steel of standard specification occurred in phase with an advance in the use of the steel. Let us look at the matter concisely. If in a given short period of time we build, say, 100 d.r. gears and 10 s.r. gears and we have 20 per cent. failures in both classes, it hardly appears certain that the equality of the percentage is significant of the cause of failure. If, however, it is held to be a true criterion we have two points of view, namely, either materials are bad during the period, or there has been an influx of new and inexperienced gear cutters. But if the actual numbers are taken into account, and viewed in the significance of the evolutionary step, the question of material does not enter as a main point; and we are left with the possibility of bad gear cutting during the period, or have to recognize inherent faults in the newer system.

If—as we suppose—the fault lies in the more complicated system in such a way that the normal errors of the simple arrangement are more searching in their effects, then we can recognize that a flaw in material will more readily be discovered. The evidence will be against the material but the true cause will be deeper—another case, in fact, of the drilling machine that bores oval holes. It is quite possible—as Mr. Calderwood contends—that the torsional oscillation problem is more pronounced in double gears; but it seems to us that this also is a question of the margin left after the gear faults are accommodated. We believe that some bad gears have been greatly improved by a continuous process of refinement. Gears that on channel trials were a nuisance to the population *ashore* have ultimately been made quite serviceable. If torsional oscillations can be suppressed in this way the trouble is not fundamentally that of critical speeds. It should also be noticed that Mr. Guy claims that his firm, by originally tackling the matter along such lines as we have hinted at, have managed to avoid all difficulties.

Surely it must be allowed that art is made more difficult by complication and that the call on technique grows rapidly with elaboration of

form; that it is, in fact, essentially harder to juggle with three balls than with two. And the growing improvement in the gearing situation is almost certainly the result of finer attention to details and a clearer appreciation of requirements; in short, an improved technique. The Authors

We may close by a short reference to one or two of the more general remarks embodied in the discussion. Dr. Hatfield makes an eloquent appeal to the engineer to study more carefully the steels he uses. This is rather like a charge of ignorance, perhaps prompted by a feeling of annoyance at the engineer's attitude towards the steel-maker, but largely genuine in its reference to the neglect of the important question of fatigue ranges. At the same time this problem has not been very conclusively treated by the metallurgist and the engineer has to do the best he can with the materials and knowledge available. Mr. Dowson refers to the manufacturer who takes up construction in an important line without proper contemplation of the problems. Perhaps much the same remark might be made in connection with gear cutting and fitting during the last few years. Prof. Christie in his closing statement tells us that still larger units are being demanded in America. We wonder whether this means larger than the successful sizes or larger than the maximum. If the latter, it negatives our statement that deceleration is the more obvious tendency; but we do not mind being wrong in that respect.

The Paper, discussion and reply comprise rather a lengthy dissertation on difficulties and troubles that may lead the uninitiated to wrong opinions. But a thought of the great achievements will remove depression and reprove the poor in faith. The necessary knowledge has to be sought along the difficult roads, but the ultimate success transforms—to follow Mr. Jude's preference—the "troubles" of the journey into "adventures" in the retrospect.

MEASURED MILE TRIALS AND OTHER SHIP PROPULSION DATA.

By G. S. BAKER, O.B.E., *Member.*

[Read in Newcastle-upon-Tyne, on the 2nd March, 1923.]

Introductory.

§ 0. Before I read the Paper, I would like to explain its position in the general programme of research at the Tank. We are carrying out research work under the more or less general headings of high speed, rudder and manœuvring power, model resistance, screw propeller and the application of the model work to the full-sized ship. This paper forms the first part of one phase in the latter category. A second phase of the same general question, namely, the resistance of a ship at sea, *i.e.*, in waves and in various winds, is being dealt with by Mr. Kent. He is making experiments on ships in waves, including voyages on ships to measure their pitching and the way in which different hull features affect a ship. With these two researches on the propeller and on sea resistance we hope to balance the actions of hull and propeller, and this is the first of the attempts to deal with the propeller side of the subject. Another aspect of the problem, namely, the effect of immersion of propeller and of rake, is being dealt with in a Tank paper which will be read before the Institution of Naval Architects at the end of the month. I have to-day been able to arrange with Mr. W. A. Souter and the North Eastern Marine Engineering Company for them to give us some very considerable help in carrying the subject further than has been possible in this paper. They have kindly agreed to fit instruments on a ship in order to measure the things which we want, and to enable us to get a better comparison than we have been able to do up to the present time; and I hope to-morrow to get another local firm to do the same. When this work is completed I hope to give you a more complete addition to the same subject.

§ 1. For the last few years the Tank Staff have been comparing data obtained with ship models and latterly have extended this work to the propellers. All of these comparisons have been made with single and twin screw ships. A number of difficulties have arisen in connexion with these—mainly with single-screw ships—and it was thought that a discussion of the subject might lead to some advancement. It should be

understood that this is the main object here, and it is hoped that as a result of this discussion more attention will be paid to the conduct and meaning of model and ship trials, and that shipowners will appreciate the value of data obtained on such trials and in service running, if these data are accurately taken and analysed. Such analysis as is now generally known, is carried out by the Tank for a comparatively small fee, and part of the aim of the Paper is to stimulate such work.

§ 2. The difficulties and discrepancies met, with each ship treated, have been openly stated. Where a plausible explanation is possible it is given and in § 22 the main discrepancies are discussed, and some general conclusions are given as regards the manner in which many trials are made, and what appear to be genuine differences in behaviour of model and ship.

Complete comparison has not been possible in any single case. We shall approach much nearer to this when it becomes the practice to instal instruments for measuring thrust and torque on the shafts. This has been done in one or two naval ships, and it is hoped that mercantile shipowners will follow this lead, as it will be to their own advantage and lead to a much more settled condition of things as regards estimating powers for ships.

Free use has been made of the papers and data published by Froude and other writers. Most of such writings deal with naval ships, and unfortunately only their general results are applicable to mercantile ships.

§ 3. *Available Data.*—For the model this takes the form of effective horsepower curves, in good cases at several displacements, thrust of propeller at various revolutions behind the ship model, usually at two speeds, and with the model removed at somewhat lower speeds of advance. Latterly torques have also been measured and efficiencies are then available. From this data, wake fractions, thrust deductions and hull efficiencies have been obtained on lines laid down by Froude. For the information of those not wholly conversant with Froude's methods a brief statement of how these factors enter into the general efficiency is given in the Appendix together with an account of the assumptions made in this mode of analysis. All such data are obtained with a perfectly clean model, in smooth water, *i.e.*, under what may be called ideal conditions in all respects in matters external to the ship itself.

§ 4. For the ship the data may be divided into three groups, according to whether it is obtained (*a*) by trials properly conducted on a measured-mile course; (*b*) by similar trials taken over a long course; (*c*) log data derived from actual working on service. Any of these may be associated in rare cases with a shop trial of the engines. The extent to which any such data can be used in scientific analysis depends mainly upon two things:—accuracy in obtaining the data, and a reasonable knowledge of the conditions of propulsion at the time such data were taken.

§ 5. *Causes of Errors.*—Ship data derived from both model and ship tests are liable to errors. For the former these errors may arise chiefly from—

(*a*) An absence of data as regards the skin friction of long surfaces at high speeds. Practically all the analysis here mentioned is based on Froude's data with only very slight modifications.

(*b*) Scale effect in passing from model to full-size ship propeller. This is most likely to show itself as a slight reduction of pitch in ship, or as an actual variation of thrust at given slip. With aerofoil surfaces these variations are not very great above certain speeds, and Taylor's results with propellers support this.

(*c*) Incorrect assumptions as regards wake conditions behind the model and ship.

(*d*) The assumption that data derived from the model in one condition can be used for estimating data for the ship in a different condition. This error has been avoided in all the cases dealt with.

For the ship the errors may be due to :—

(*e*) acceleration on the course;

(*f*) tidal effect;

(*g*) weather conditions;

(*h*) depth of water;

(*j*) absence of all knowledge of the mechanical efficiency of the driving mechanism.

In the case of ship-log data special errors have to be catered for, and these are mentioned in § 8. "Errors of intent" such as the bottling up of steam in the intervening periods between the mile runs, the adoption of different courses in runs made with and against the tide, misuse of marks, etc., need not be considered, they arise only in rare cases and the method of dealing with them is well known. For the same reason errors due to acceleration on the course, and tidal effect on a measured mile are passed over with this brief recognition.

Depth of water may have a very serious effect. Certain data on this are fairly well known, and some additional data are given in § 21 in connexion with a ship's trial. But apart from these possible errors, the chief difficulty is to obtain accurate data in a sufficiently complete form, and with a reasonable knowledge of the conditions at the trial. The object of such trials in most cases is to demonstrate to the satisfaction of the prospective owners the steaming qualities of the ship and the maximum power of the ship's engines. The conditions which hold may depart considerably from those existing either on service or in the model experiments, and render the results useless for analytic purposes. With all of this in mind it is now proposed to examine certain data placed at our disposal for this purpose, with a view to determining what errors may arise in the trials and in estimates made from model data.

§ 6. *General Mode of Analysis.*—This has been the same in all cases, being carried out in particular cases to a greater or less extent as was possible with the available data. All ships have been brought to 400 feet length, for various reasons, the ship type, however, being given for each one.

If the revolutions, speed of advance through the water and dimensions of a propeller are known, the power which it will develop has been calculated either from Froude's model screw data, or from data derived from a model of the propeller when this was available. The power required to turn the screw has been obtained in the same way. The power developed by the screw estimated in this way has been compared with the effective power estimated from the model resistance experiments with due allowance for hull efficiency, and the power required to turn the screw has been compared with the measured or stated powers of the engine, on the shaft or indicated. These give two separate and distinct comparisons, between model resistance and screw thrust and between power to turn screw and actual power. In addition, estimates of the revolutions required to drive the ship, have sometimes been made directly from the tests of propellers behind the model, and a comparison of the ratio $\frac{\text{speed}}{\text{pitch} \times \text{revs.}}$ estimated and actual can then be made.

In estimating the powers delivered by or required to turn the ship's screw, the usual course has been to find the wake fraction which is required either to make the shaft horsepower just balance the power required at the propeller, or to make the effective horsepower agree with the thrust horsepower of the screw. In estimating thrusts of propeller when comparisons of $\frac{\text{thrust}}{(\text{velocity})^2}$ are made, the model thrust deduction fraction has been assumed to hold for ship. The hull efficiency of ship will, of course, depend upon this thrust deduction fraction and the wake fraction adopted as above.

It has been assumed that the effective pitch exceeds the face pitch. In model screws the excess varies from 2 per cent. to sometimes 6 per cent., but in the type here dealt with the excess is usually about 2·5 to 3·5 per cent. In the ship screw, the effective cannot be less than the face pitch (unless the driving face is cut away at the trailing edge which is only done quite near the root in some cases) and its excess over the face pitch will be less than in the model if it differs from the latter, this being the general trend of scale effect in going from small to large models. Froude gives this excess as 2 per cent., this figure being what was required to bring ship results and model data into general line, the type of ship dealt with being comparatively fine lined twin-screw ships mostly of old-cruiser type. Departures from these general assumptions are made for individual ships to which reference should be made for details.

§ 7. *SHIP LOGS.*—The most common ship data are those obtained from a ship's log on ordinary service. From such logs can be obtained the daily run, average revs. per min., draught of ship approximately,

sea and wind. Occasionally powers are given, but there is rarely any indication of the relation between these powers and the running of the ship, indicator diagrams being taken only once every day or two, and no attempt is made to correlate these with the revolutions, etc. Quite often powers are not measured, but are taken from a curve in the possession of the engineer. The only check on power is a relative one, and is obtained by comparing fuel consumptions on different voyages or

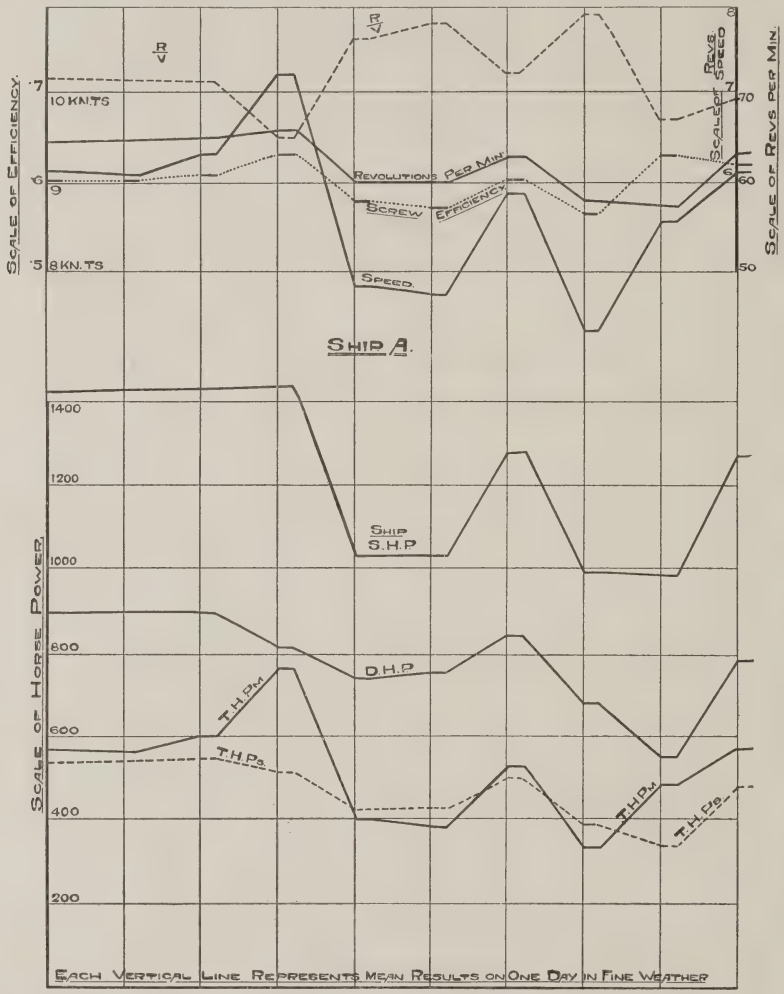

Fig. I. Analysis of Ship Log Data. & S.

on ships reasonably alike in dimensions, etc., and ship's logs are not complete enough for such comparisons.

In arriving at ship speeds care has to be taken to avoid two possible errors. If the ship is steaming in any way except north and south, the "day" may differ from 24 hours to a considerable extent, and allowances must be made for this when working with the daily run. The second error is much more difficult to deal with. There are ocean

currents which, as in certain portions of the gulf stream, average something approaching one nautical mile per hour, and any analysis or attempt to check efficiency of propulsion becomes practically useless when the ship is working in such a current. A knowledge of how to take advantage of or to avoid these currents must be invaluable to the shipowner whose vessels may work where such currents exist. A favourable current shows itself in the analysis by the powers estimated from the model resistance experiments exceeding those estimated from the propeller on the voyage the difference being 80 per cent. in one case analysed. This 80 per cent. did not exist in fact of course, the mode of analysis is one which exaggerates the effect of any false assumption as regards relative speed and revolutions.

§ 8. To compare such data with model experiments, only fine-weather days can be chosen. In two logs for different ships, working on the Atlantic in the early part of the year, out of 90 days, 14 days were really fine in one case and five only in the other.

The analysis of the log of one of these ships is given as a matter of interest, and to show what may be achieved without any great expenditure of labour in analysis. Fig. 1 shows the results obtained.

The displacements on each day have been estimated from those at beginning and end of voyage, assuming it to vary directly with time.

The effective horsepowers (E.H.P.) estimated from model experiments have been increased by 8 per cent. to allow for bilge-keel, wind, and sea resistance on a fine day. The hull efficiency of the model (1·11) has been assumed to hold for ship, and the thrust horsepower ($T.H.P._M$) which the screw deliver for propulsion is then obtained being equal to $\frac{E.H.P. \times 1{\cdot}08}{1{\cdot}11}$. The thrust horsepowers which the screw will develop ($T.H.P._S$) at its stated mean revolutions associated with the ship's mean speed have been derived from model screw data, assuming model wake fraction, and are shown in the figure. The two curves $T.H.P._M$ and $T.H.P._S$ should agree. The $T.H.P._S$ divided by screw efficiency estimated from model screw experiments is shown as a D.H.P. curve. This curve with a small allowance for shaft and thrust-block friction should agree with the firm's S.H.P. given.

The diagram shows a moderate agreement between the powers derived from resistance experiments and the screw data. The second ship mentioned was also quite good in this respect. The differences however are too large to allow of anything but broad conclusions. The analysis also shows a big and consistent discrepancy between the D.H.P. and the S.H.P. in the ship for which powers are given. Allowing 5 per cent. for reasonable friction on the shaft and in the thrust block, this discrepancy varies from 30 per cent. to 50 per cent. of the estimated powers. To bring these two curves together one may increase the wake fraction, but it would have to be increased beyond all reason (it has been taken as 0·5 in the calculations) and even if this were done, agreement would be found only by producing an equally disturbing disagreement between

TABLE I.—SUMMARY OF LOG DATA (§ 8—11). Lengths of all ships = 400 feet.

Ship.	Breadth feet.	Displacement tons.	Block Co-efficient.	No. of Screws.	Average.			Wake fraction.	Hull efficiency.	Relative Rotative efficiency.	Screw efficiency.	(E.H.P.)/S.H.P.	Remarks.
					Knots.	r.p.m.	Power.						
A.	52·4	11,060	0·81	1	8·4	63	shaft 1,230	model 0·5	model 1·12	assumed 1·0	0·60	0·46	Appendage and fine weather resistance assumed 8 per cent. of naked resistance.
B.	52·4	11,400	0·81	1	8·1	62	—	model 0·5	model 1·11	assumed 1·0	0·61	—	
C.	58·5	16,150	0·773	1	12·46 10·25	69·3 58·4	shaft 3,630 —	deduced 0·4 0·6	model 1·16	deduced 0·87	0·62	0·53	Model Wake Fraction, 45 at 12·5 knots. Appendage and weather resistance deduced as 15 to 25 per cent. naked resistance.
D.	64·2	14,020	0·699	2	13·6	80	indicated 6,630	assumed 0·14 to 0·17	assumed 1·0	assumed 1·0	0·70	0·4	Appendage and weather resistance deduced as 12 per cent. on trial and 24 to 32 per cent. on service, hence low E.H.P./S.H.P.

the thrust horsepowers developed by screw and estimated from the model resistance experiments. This could only be set right by assuming the actual resistance exceeded the estimated by about 30 per cent, *i.e.*, some 38 per cent. above naked E.H.P. Leaving this possibility as too unreasonable as we are dealing with fine weather there are two alternatives, first that the propeller efficiency in the ship is 30 per cent. lower than in the model, second that the recorded shaft horsepowers are an equal amount too high. The analyses of measured-mile trials and model data show that the loss in efficiency behind ship *might* amount to 10 per cent. but anything beyond this would be very abnormal, and we are left with the last as the only explanation, the powers are incorrectly recorded.

§ 9. In a third case (ship C) the logs giving the results from port to port over a period of about one year were available. Some of these runs were made in opposite directions from port to port with only comparatively short intervals of time between, and pairs of runs in fine weather have been obtained, this eliminating the question of length of day and ocean currents.

Assuming the model hull efficiency (1·16) to hold for ship, the ship data will agree with the model resistance at 12·5 knots with a wake fraction of 0·60 and a resistance 15 per cent. above the naked resistance: at 10·25 knots with a wake fraction of 0·40 and a resistance 25 per cent. above the naked resistance. On the first voyage s.h.p. was measured on a number of occasions at 12·5 knots, and these powers agree with those estimated as above with open screw efficiency as obtained from Froude's data coupled with a relative rotative efficiency of 0·87. In giving this figure 2 per cent. has been allowed for loss in shaft and block friction; if it be raised to 5 per cent. the relative rotative efficiency becomes 0·90. These results are fairly consistent and show as reasonable an agreement as could be expected between ship and model, if the relative rotative efficiency is accepted.

§ 10. A fourth set of data for a somewhat faster ship (D, Table I) has also been analysed. These data consisted of complete voyage summaries for some eighteen months, daily runs on certain of the later voyages, and measured-mile data of the contract trials. Effective horsepowers from model but no screw-propeller experiments were available. The steam-trial data were analysed so as to determine the wake fraction necessary for the screws to propel the ship, assuming (as weather was fine) the air and appendage resistance as 8 per cent., and hull efficiency of unity. This gave wake fractions 0·14 to 0·17 for different speeds, screw efficiency 0·70, and required a mechanical efficiency of engines (quadruple expansion) of 0·86 to 0·89 to make the power required to turn the screws agree with the i.h.p. given. All this was fairly reasonable, and when running on fine days the same wake fraction, etc., has been taken in the analyses of the logs.

The data for the complete voyages were naturally rather crude, and it was found that the thrust horsepowers delivered by the propeller when

divided by the effective power estimated from the model, varied from 0·93 to 1·3 in fine weather to from 1·46 to 2·16 in rough and heavy weather. In fine weather this ratio ought to be about 1·08: certainly never less than unity, unless the hull efficiency is much higher than usual for this type. When the powers required to turn the screw are calculated, and assuming mechanical efficiency of 0·82 for the engines and transmission, the estimated i.h.p. is found to be 25 per cent. lower in fine weather and 11 per cent. lower in rough than those given in the logs.

§ 11. The firm concerned was good enough on two voyages to see that the indicator diagrams were taken properly and that revolutions were taken at the same time of day. The results on these two voyages have been treated a little differently from the others. Only fine-weather data have been used. The revolutions at the time of taking the diagrams were used for calculations, and the speeds of ship which would enable the propellers to deliver a thrust horsepower 12 per cent. in excess of the effective horsepower (to allow for air and appendage resistance) have been found. But even when this is done, the discrepancy between the estimated indicated powers (after full allowance for air, etc., resistance) and the reputed powers varied from 12 per cent. to 20 per cent. This discrepancy, it should be noted, is not between model and propeller, but between propeller and engine.

TABLE II.—ANALYSIS OF LOG — SHIP "D" IN FINE WEATHER. (§ 11, TABLE I) TEST OF ACCURACY OF LOG I.H.P. ASSUMING I.H.P. IN LOG GIVES AVERAGE POWER FOR THE DAY, FINDING SHIP SPEEDS AND WAKE FRACTIONS REQUIRED SO THAT PROPELLERS JUST PROPEL THE SHIP.

I.H.P.	Revs. per Minute.	Speed in Knots		Wake Fraction.	
		In Log.	Required for balance as above.	Required for balance as above.	Required in steam trials.
3,500	94·5	12·3	13·4	0·46	0·14 to 0·17
2,940	90·5	11·7	12·5	0·37	

If it be conceded that the i.h.p. in the log was actually developed, and assuming that Froude's propeller data hold for the propellers, it is possible to find a ship speed and wake fraction so that the propellers just propel the ship. The reasonableness of the results must be the justification or otherwise of the assumption made. Table II. shows what is required for this. In view of the steam-trial results it cannot be believed that in fine weather such large wake fractions existed for the ship, and therefore the initial assumptions must be wrong, *i.e.*, either the horsepowers in the log were not developed, or Froude's data for screws, although holding for the steam trials, fail to hold under similar conditions at sea, which is hardly sound reasoning.

§ 12. *Long Course Trials.*—Passing to trials over long measured courses, two cases are chosen, ships E and F. In each case the trial

consists of two runs of about one to two hours duration in opposite directions. In one case speeds over land only were taken: in the other Walker's log was trailed throughout the trial, the log being kept as far as possible from the centre line of the ship.

Ship E.—Length 400, block coefficient, 0·75.

The mean speed was 13·3, power 3,000 r.p.m. 68·75. Model resistance data were available but no model screw data. Nothing could be made of these data as they stood. It was found necessary to reduce the mean speed at least ten per cent. before anything like agreement between engine, propeller, and model resistance was obtained. The wake fraction, even with this reduced speed, was high, and attempts to bring things into agreement in other ways were useless, the speed obtained was too high and inaccurate to be trusted.

Ship F.—Length 400, block coefficient, 0·76.

Tank representatives were present on this trial. The ship's speed taken as a mean of the land journeys both ways, which occupied roughly 3 hours, was 12·4 knots, the log showing 12·5 knots in one direction, 12·9 knots in the other. In the latter case the wind was across the course blowing the log into the outer part of the ship's wake. Taking the speed as 12·5 knots, the propulsive coefficient becomes 0·495 taken on indicated powers. To bring the model data into agreement with the ship propeller results it was found necessary to vary the wake fraction from 0·46 in the model to 0·4 for ship. This also brought the ratio of $\frac{\text{revs.}}{\text{speed}}$ predicted from model into practical agreement with the ship results. To bring the screw-propeller results into line with the indicated powers, a mechanical efficiency of engine and transmission of 0·72 had to be taken. The engine was of normal triple-expansion type. It is probable, however, that this rather low efficiency may be due in part to the relative rotative efficiency being a little lower than unity, *i.e.*, the screw efficiency behind the ship being a little low compared with its efficiency with no ship in front of it. The ship form was a reasonably good one, but the loss in screw efficiency behind ship, judging by some other results, may have been five or more per cent., and this would give a mechanical efficiency of 0·76 vice 0·72 above.

§ 13. *Measured Mile Trials.*—In Table III. are given the particulars of the analysis of several ship's trials. The same general procedure as before has been adopted. The speed and propeller dimensions and revolutions are accepted as accurate. The wake fraction is found which will give a thrust power developed by the propeller reasonably close to that estimated from the model, and at the same time demand such power for turning the propeller as will agree with the s.h.p., or with the i.h.p., using a reasonable mechanical efficiency of engine and transmission. Where hull efficiencies, etc., from model are available these are discussed.

Ship G. is an oil-engine ship of good form, tested for resistance only. The revolutions are high and propellers correspondingly small. Good

Table III.—Measured Mile Trials.

All Ships brought to a Common Length = 400′ 0″. For Explanation of how the Different Efficiencies are obtained see § 6.

Ship	Ship Dimensions. Beam in Feet.	Draught in Feet.	Displacement in Tons.	Ship Screw Data. No. of Screws and Blades.	Diameter.	Face Pitch. Ratio.	Trial Data. Speed in Knots.	R.P.M	I.H.P.	S.H.P.	Efficiency Data Deduced. Naked E.H.P.	Wake Fraction.	Coeff. A.×	Relative Rotative Efficiency π	Mechanical Efficiency of Engine and Transmission.	Screw Efficiency in Open.	Quasi Propulsive Coefficient for Ship on Trial. ⊙	Effective Face Pitch.	Remarks.
G.	62.0	25.2	12,000	2/4	11.75	0.875	13.13 10.72 8.14	141 115 87.5	5,240 3,200 1,790	— — —	2,100 1,020 440	0.25 0.25 0.25	1.11 1.26 1.33	Assumed Unity.	0.70 0.63 0.50	0.64 0.64 0.64	0.575 0.51 0.48	1.02	See § 6—For Mechanical Efficiency in Shop Tests.
H.	53.8	17.3	5,820	2/3	12.2	1.05	24.4	24.2	—	15,740	8,100	0.13 0.10	1.27 1.18	Assumed 1.0 0.9		0.66 0.68	0.52 0.52	1.04 1.03	Propellers slightly wide tipped.
I.	66.1	24.7	13,400	1/4	22.0	0.89	12.7	64.5	4,440	—	2,100 (from 307)	0.92	0.88	0.85	Previously measured 0.845 (allowing 1½ % for shaft)	0.58	0.56	1.02	For further Data see § 15.
J.	60.0	F. 11.25 A. 22.75	8,010	1/4	21.1	1.01	13.44 14.4	67 3 73.1	4,200 5,530	— —	2,170 2,720	0.41 0.39	0.98	0.77 F. 0.82 M.		0.65 F. 0.61 M.	About 0.61	1.02	Quasi propulsive efficiency, based on an assumed mechanical efficiency of 0.8. F—Deduced using Froude's Screw Data. M—Deduced from Tank Screw Data interpolating between deep and very light immersion.
K	60.0	F. 13.1 A. 18.1 F. 23.8 A. 24.8	7,320 11,900	1/4 1/4	21.1	1.00	11.75 12.60 13.24 14.0	7.02 80.0 66.7 72.6	3,040 4,120 4,120 5,320	— — — —	1,360 1,835 2,470 3.010	0.4 0.4	1.0 1.02	1.02	0.97 0.915	Open 0.615 behind 0.63	0.60 From behind efficiency 0.62	1.02 1.02	
L.	63.0	F. 27.2 A. 34.0	16,400	1/4	24.0	Variable 1.25 maximum	13 14	53.6	7,250	—	3.080	0.39	Assumed Unity.		Assumed 0.78	0.61	0.54	1.0	Special Screw with Large Rake Aft. Pitch is given for outer half and decreases towards Boss.
M.	63.0	F. 27.6 A. 32.7	16,200	1/4	23.3	Variable 1.24 maximum	12 75	56.0	7,650	—	2,670	0.33		—	0.78	0.61	0.45	1.0	" "
N.	63.0	F. 8.6 A. 23.1	7,750	1/4	24.0	Variable 1.25	15.3	57.7	8,800	—	3,900	—		—	0.78	—	0.57	1.0	" .
O.		F. 15.7 A. 19.7	7,400	2/4	15.0	1.06 1.12	14.8 13.9	103 94	4,400 3,950	— —	2,330 1,785	0.2 0 2	1.07 1.27	Assumed Unity.	0.79	0.71 0.71	0.67 0.57	1.02	

π Relative Rotative Efficiency is Ratio of Screw Efficiency behind Ship and in open water no Ship present.

A× is the ratio $\frac{\text{Thrust Horse-Power developed by Screw, as calculated}}{\text{Naked E.H.P.}}$ which equals $\frac{1 + \text{Allowance for Wind and Appendage Resistance}}{\text{Hull Efficiency.}}$

agreement can be obtained either with wake fraction 0·20, the ratio $\frac{\text{effective}}{\text{face}}$ pitch of screw being 1·04, or with wake fraction 0·25 the above ratio of pitches being 1·02. With either of these the A ratio in Table III. varies from 1·11 at 13·13 knots to 1·33 at 8·14 knots, the mechanical efficiencies being as given in the table. It was not known when these results were analysed that the mechanical efficiency of the engine had been measured in the shop, and was given as 0·728 at normal full load, which compares very well with the 0·70 in the table, which includes transmission loss. A little fuller ship model gave a wake fraction of 0·2 and hull efficiency varying from 1·04 to 0·98 with different circumstances, and this agrees well with the trial analysis.

The low-speed result is instructive, first from the point of view of the effect at different speeds of given weather conditions which presumably did not change seriously in the short time of the trial, and second as regards mechanical efficiency. At the lowest speed the sea and wind resistance on the rather high superstructure had three times the effect that it did at the highest speed. This can be accounted for if the wind is taken to be down the course and of velocity about 13 knots.

§ 14. *Ship H.* is a fairly fast turbine ship tested for resistance only. The analysis has proceeded in this case by finding the wake fraction which will demand a s.h.p. to turn the shaft only 1 to 2 per cent. below the measured s.h.p., assuming the hull efficiency and relative rotative efficiency were unity. The wake fraction is then 0·13, and excess of power delivered by screw at the trial revolutions over the effective power was 27 per cent. Of this some 10 per cent. is due to appendages, the rest is lost either in excess air resistance, low hull efficiency or low crew efficiency behind ship. It should be noted that, as with ship G, this 27 per cent. excess cannot be adjusted or reduced by a small variation of pitch as might be feasible on the ship. This would only demand a slightly different wake to balance things. In this case 1 per cent. off the pitch requires the wake fraction to be varied from 0·14 to 0·16.

It is interesting here to examine the possibility of the screw efficiency being lower behind the ship than in open water. The second line for ship H in Table III. has been given to show the effect. The wake fraction must be lowered and the appendage plus air resistance becomes 18 per cent. vice 27 per cent. of the effective power. There is nothing to guide one in judging which is right except the reasonableness of these results. This is a twin-screw ship of fairly fine lines and the hull efficiency should not depart seriously from unity. If this is accepted, then the excess of the A coefficient over unity is entirely due to appendages, and 27 per cent. is high for this in fine weather. One can only suspect that the second line in the table is nearer the truth and that the relative rotative efficiency is down a little.

§ 15. *Ship I.*—This ship has been put in this series, although the data were not taken on a measured mile, because of the care taken and the probably accurate results obtained. It is one of the ships on which

Bauer has been able to measure the engine mechanical efficiency and the propeller thrust. The whole of this work of Bauer is worthy of study, as it is the most complete of its kind on a mercantile vessel.

Results were obtained on three different occasions, but as these all agree together, data for one set only are given. In this case, the analysis has proceeded as follows. There was no sea and no wind. The air and appendage resistance therefore would not exceed 10 per cent. of the tow-rope resistance. Assuming the ship to be of reasonably good form, the measured thrust will exceed the tow-rope resistance plus 10 per cent. by an amount which is a measure of the augment of resistance and gives us the thrust deduction fraction 0·35 in this case. This is clear of any question of wake value or screw efficiency. Wake fractions were assumed, and the powers delivered by the screw and required to turn the propeller calculated from Froude's data and the known revolutions, etc. The power delivered by the screw must be equal to—

$$\frac{\text{E.H.P.} + \text{appendage and air allowance}}{\text{hull efficiency.}}$$

Since the thrust deduction fraction is known, the hull efficiency can be calculated for each assumed wake. Taking appendages allowance as 10 per cent. agreement between these two powers was found, with $w = 0{\cdot}92$, giving hull efficiency $= (1 - 0{\cdot}35)\ (1 + 0{\cdot}92) = 1{\cdot}25$.

With this wake the screw efficiency in open water was 0·58 and to bring the powers on the shaft into agreement with those calculated from the i.h.p. a relative rotative efficiency of 0·85 is required.

As it was possible that no check had been put upon the propeller pitch, the calculations were repeated with the effective pitch raised to 1·1 times the face pitch. For general agreement the wake fraction then becomes 0·7, the hull efficiency 1·1 and the relative rotative efficiency 0·92. In other words no possible variation of pitch will avoid the high wake fraction and consequent high hull efficiency and low screw efficiency behind. Comment on these results will be found in § 22.

§ 16. *Ships J and K.*—These were sister ships, fitted with triple-expansion steam reciprocating engines. One trial was made with ship J, at rather light draught with considerable trim by the stern the propeller tips being just immersed. There was a wind of 13 to 14 knots at about 30 degrees to the course at the time of the trial, necessitating steady helm of about 10 degrees when running into the wind. Two trials—at ballast and deep-load draughts were made with ship K. The weather was fine with a wind of about 11 knots along the course. Tank representatives were on all the trials.

The model was tested at two draughts—closely resembling the conditions of trial of Ship K. There were very considerable differences between the model propeller results in the deep load (with screw well immersed) and in the ballast condition with about one-fifth the diameter of propeller out of water. In open water the value of $\frac{\text{thrust}}{(\text{velocity})^2}$ at about the working slips with ballast immersion was only 0·7 of that in

the deep condition at the same ratio of $\frac{\text{revs.}}{\text{speed}}$, and this factor varied considerably with the actual speed of advance. Moreover it was not found possible to obtain a wake fraction for the model in the ballast condition, *i.e.*, the value of $\frac{\text{thrust}}{(\text{velocity})^2}$ at the value of $\frac{\text{revs.}}{\text{speed}}$ for self propulsion could not be brought by simple change in velocity, on the curve of $\frac{\text{thrust}}{(\text{velocity})^2}$ for the screw in open water. Other tests have shown that actual velocity and immersion have quite serious effects. This is dealt with in some details in another place, but these difficulties made it necessary first for the model experiments to be an exact replica of the trial conditions if the results were to apply to the ship, and second that the application of the tests of the propeller by itself at ballast immersion, to the working conditions behind ship and model should be seriously considered.

§ 17. In view of the surface effect, the analysis for ship J has been effected in two ways. The thrust-deduction fraction model has been assumed to hold for ship, and adopting the e.h.p. for the trial condition, adding 10 per cent. for appendage, etc., the value of $\frac{\text{thrust required}}{(\text{ship speed})^2} = \frac{T}{V^2}$ can be calculated for ship. Different wakes have then been assumed and this ratio altered until a wake has been found which brings the ship $\frac{T}{V^2}$ on the $\frac{T}{V^2}$ curve for the model screw at the correct immersion and the correct $\left(\frac{\text{velocity}}{\text{pitch} \times \text{revs.}}\right)$ in open water. This gives a ship w value and from this the hull efficiency is obtained and hence the A coefficient. From the model screw the efficiency is also obtained, and we then require a coefficient of 0·83 or 0·79 at the two trial speeds to bring the powers into agreement with the indicated powers. This represents the product of the mechanical efficiency of engine and transmission and any loss in relative rotative efficiency. As a check upon this, the thrust powers of the screw at the known revolutions have been calculated from Froude's data, using a thrust coefficient ("B" value) 6 per cent. less than Froude's to take account of the immersion. The results are in very fair agreement with those obtained above. At the two trial speeds the derived wake fractions were 0·4 and 0·37 vice 0·42 and 0·40 obtained in comparison with our own results in open, and 0·62 obtained with the model propeller behind the model hull in a *deeper* condition. The screw efficiencies obtained from Froude's data were 0·66 vice 0·61 in our tests and the derived product of mechanical and rotative efficiencies were 0·78 and 0·75 vice 0·83 and 0·79 respectively. The main discrepancy between Froude's data and the model screw data lies in the open-water efficiency. It should be remembered that Froude's data were obtained with screws fairly well immersed and in this ship trial the propeller tips were only just immersed.

§ 18. For ship K in the deep condition, the necessary wake fractions were determined which would bring the ship $\frac{T}{V^2}$ estimated from the model (with 10 per cent. added for appendages, etc., and using the thrust deduction fraction of 0·2) on the $\frac{T}{V^2}$ for the model screw in open water at the trial revolutions and speeds. At the two trial speeds the w values found are 0·41 and 0·35, giving hull efficiencies of 1·13 and 1·08 respectively with open screw efficiency 0·615. A further comparison between the ship and model was obtained as follows:—The value of $\frac{T}{V^2}$ for ship estimated from the model resistance experiments as above was plotted against, a curve of $\frac{T}{V^2}$ for the model propeller working behind the model hull, at its correct value of $\left(\frac{\text{speed}}{\text{revs.} \times \text{pitch}}\right)$. It was found necessary to assume a wake fraction difference between the model hull value (0·62), and the ship result to bring the two into line, giving a ship wake fraction of 0·4. This of course assumes all the difference lies in wake. To test whether pitch change accounted for any of the difference, calculations can be made from Froude's data of the effect of a small change in pitch on the $\frac{T}{V^2}$ curve to a base of $\frac{V}{PN}$. This has been done for a 3 per cent. change in pitch, with little effect on the final curve. Presumably therefore the wake velocities in model and ship are in the ratio of $\frac{V}{1{\cdot}6}$ and $\frac{V}{1{\cdot}4}$ in this case.

Assuming this wake condition, and obtaining the resulting screw efficiency it becomes necessary to call the mechanical efficiency 0·97 and 0·915 at the two speeds, which are about 7 or 8 per cent. too great. This requires a relative rotative efficiency somewhat less than in the model, or a smaller pitch to balance matters.

In the light condition, abnormal conditions were met in the model Reasons are given in the Appendix for not always applying the results of any screw in open water to the screw behind the ship, and for only accepting the quasi propulsive coefficient for screw behind in certain cases, when the ship is light. In such a comparison of these coefficients the agreement was fairly good, but comparing revolutions predicted from the model, there was a large discrepancy, the ship requiring some 18 per cent. more revolutions than the model gave. Both ship and model results are known to be authentic, and the difference is genuine. It is not feasible that there can be sufficient twist in the flow behind the ship compared with the model to account for this difference in revolutions, and it is believed to be due to the presence of air behind the screw and the effect of this air on the thrust developed, is different in ship and model.

§ 19. Ships L and M are sister ships of coastal cargo type, both being tried in fully loaded condition. The wake fractions required to bring

the ship screw into agreement with the ship's engines are 0·33 and 0·39 assuming engine and transmission efficiency of 7·78. The model wake fraction varied from 0·31 at 12·8 knots to 0·40 at 15 knots in this condition. A sister ship with somewhat different propeller required a wake fraction of 0·44 under the same conditions. Comparing the model-screw and ship-screw values of $\frac{T}{V^2}$ to a base of $\frac{V}{PN}$ (allowing 12 per cent. for air and appendage resistance, and using the same thrust deduction fractions in both cases), the two can be brought into agreement by assuming either that the ship pitch is 2 per cent. less than the model, ship wake about 8 per cent. less, or by taking the air and appendage resistance as 20 per cent. of the naked resistance. A sister ship N was tried in a light condition, with screw centre immersed 0·38 diameter. The model had been tested with screw at two immersions 0·27 and 0·68 diameter. Allowing 12 per cent. for air and appendages, the ship screw gives for model a $\frac{T}{V^2}$ of 0·57, and at the same $\frac{V}{PN}$ value the model $\frac{T}{V^2}$ values are 0·5 and 0·68 at the two immersions given above. Other experiments show that until the screw tips break through the surface the $\frac{T}{V^2}$ will not vary materially at the same speed, and 0·68 may be taken as its value for immersions down to 0·5 diameter. The ship screw gives some 8 or 10 per cent. less thrust than would be predicted from the model, but precise comparison would only be possible with more detail as to the effect of immersion. The same difficulty in defining wake was found with this model in light condition as with the model of ship K, the $\frac{\text{thrusts}}{(\text{velocity})^2}$ to a base of slip for the screw in open water would not agree with the thrusts behind model by simple change in velocity.

§ 20. *Ship O.*—Results of two trials of sister ships are given. Model resistance experiments on the form, and screw experiments with a form not very dissimilar were available.

Using Froude's data powers delivered by and required to turn the propellers have been calculated, and the wake fraction obtained which brings the calculated shaft horsepower into agreement with the measured i.h.p. with a reasonable mechanical efficiency. With a wake fraction of 0·2 the required engine and transmission efficiency is 0·79 and the screw efficiency behind is 0·71, in both trials. The power delivered by the propellers, in the first case, then exceeds the naked effective horsepowers by 6·5 per cent. This is rather low, but could be raised by a slight increase in wake, which would also increase the mechanical efficiency a corresponding amount. In this trial there was no wind and practically no tide so that a low percentage for air, etc., resistance would be expected. In the second case, the percentage for appendage and wind resistance works out at 27. There was a wind of force 3·5 at the time which would account for some of this, but this trial is a rather good example of effect of shallow water. The propulsive co-

efficient from the trial was 0·45, the propulsive coefficient for the sister ship being 0·53. The known differences in condition of trial between the two ships were the higher wind speed and the relatively shallow water on the measured course in the second case, the depth varying from 3·1 to 5·0 ship draughts.

§ 21. Experimental results with models and ships in shallow water have been published from time to time, and some recent Froude Tank results given in Table IV. show the effect in even these depths to be appreciable.

TABLE IV.—INCREASE IN RESISTANCE DUE TO SHOAL WATER AT SPEEDS WELL BELOW THAT AT WHICH ABNORMAL INCREASE IN RESISTANCE BEGINS.

Ship Form.	Prismatic Coefficient.	Depth of Water / Draught of Ship.	Percentage increase over deep water resistance.
P.	0·71	2·4	18·5
Q.	0·71	5·7	4·5 to 8·0
R.	0·74	2·45	23·0

P and R are normal types of mercantile ship.
Q is abnormally broad and flat bottomed.

Schütte's experiments show that a depth of water greater than five and not more than seven draughts is required, to be clear of its effect on resistance for normal vessels. His experiments give an increase of 0·8 lb. resistance per ton at 10 knots, corresponding to an increase of roughly 23 per cent. in water of depth 3·1 draughts. These results indicate an increase of resistance for a vessel of 0·75 prismatic coefficient of 15 per cent. in 3·1 draughts and 5 per cent. in 5·0 draughts, or a mean increase of 10·0 per cent. over the course. This effect, taken in conjunction with the higher wind force, is believed to account for the difference in propulsive coefficient in these two trials.

§ 22. *Conclusions.*—These are naturally based, not only on the work detailed in this paper, which represents about one half of the trial analysis carried out at the Tank in this last few years, but on the earlier work of Froude, with naval ships, Sir John Biles and, more recently, by Mr. Holt and Bauer.

(*a*) First of all, it is fairly clear that detail comparison of form and screw in ship and model can be made at present only on certain assumptions. The measurement of thrust and torque reduce these assumptions considerably, and trials with such complete measurements would be invaluable.

(*b*) The manner in which trials are carried out, and in particular making only one run each way over the mile at each speed, does not lend itself to accuracy. A delay of only half an hour between the two runs may affect the mean speed by 0·2 to 0·4 knot on some courses, and it must be remembered that when resistance varies (as it quite often

does) as the cube of the speed, this discrepancy causes the estimate of resistance to be 10 per cent. out, and the estimate of developed power to be wrong in the *other* direction.

(*c*) If a ship is normally working with its propeller well immersed, trials on the mile in a light condition will serve little or no practical purposes. The emersion of the screw prevents it from absorbing all the power it would when deep, and it is run at higher revolutions than is attainable when deep.

(*d*) Comparison of such light-draft trials with model results show that the revolutions on the ship are higher than those predicted from the model, the actual excess not being very definite yet. Froude's method of analysis breaks down for model and ship in this condition, when the stern is fairly full.

(*e*) Comparison of model and ship in deep condition are fairly good in general, particularly with twin screws and fine-ended ships. But with single-screw ships the model wakes are high compared with the ship wakes, the excess probably depending on the fullness of the stern. This affects the hull efficiency and, to a small and usually not very important extent, the screw efficiency. But too many assumptions have to be made for it to be possible to define the accuracy of power estimates for ships based on model tests. In several cases given it appears quite good and other writers have given similarly good results. In some cases where large discrepancies were apparent, a searching analysis has shown the cause to lie in matters apart from the estimate, and it is believed that, except in the respects already mentioned, the model data can be used safely for estimating ship data.

(*f*) It is difficult to make anything of the normal ship log unless more than the usual amount of trouble has been taken by the ship engineer. Without such trouble it is useless and misleading. With care it could be made a valuable means of watching the efficiency of the ship, and of determining the weak points in its propulsion. The logged powers exceed the calculated powers by a far greater percentage than one would expect. The main part of this excess does not appear to be due to resistance, but either to loss of screw efficiency (even in fine weather) to loss of engine efficiency, or to the taking of diagrams at special times when the engines have been temporarily worked up.

The Author's thanks are due to many firms who have been good enough to place data at our disposal and to assist us in other ways. It is proposed to continue this work in certain definite directions in the future, and now that a number of ships are being built which have had both propeller and form tested, this comparison work can be extended and made more definite. In this we necessarily rely on the assistance and co-operation of builders and owners, and we feel sure that this will be extended to us in the future as it has been in the past.

APPENDIX.

FROUDE'S TREATMENT OF PROPULSIVE EFFICIENCY.

Froude assumed that—

(1) When a screw is working behind a ship, complex as are the actual motions of the wake water, the net effect of this state of motion upon the screw is practically identical with that which would be produced by a uniform forward current.

(2) The equivalent uniform speed of the wake past the screw is that at which the screw in open water, will develop the same thrust as it did at the same revolutions behind the model.

(3) The energy obtained from the screw behind the model, in the form of work done is the product.

$$\text{(thrust of screw)} \quad \left(\begin{array}{c}\text{uniform velocity of wake water past screw}\\ \text{determined under assumption 2.}\end{array}\right)$$

At the time this system was published (1883) these assumptions were justified by Froude in a statement that, working with them, and comparing the torque required to drive the model screw calculated from open water experiments, and the torque actually required behind the model "the agreement though not in all cases quite exact, is so nearly exact as to be almost covered by the limits of error of the observations. The difference, such as it is . . . . shows that the actual turbulent wake left by the model increases the efficiency of the screw more than would the hypothetical uniform wake, by an amount varying from nil to 2 per cent." The efficiency account can therefore be stated as follows:—

$$\left.\begin{array}{c}\text{Quasi propulsive}\\ \text{efficiency}\end{array}\right\} = \frac{RV}{S} = \frac{R}{T} \times \frac{V}{V_1} \times \frac{TV_1}{S_1} \times \frac{S_1}{S}$$

Where R is the resistance of ship when towed at speed V.

T is the thrust of screw behind ship when propelling it at speed V.

V_1 is speed in open water at which the screw will develop the same T as behind the model at the same r.p.m.

S_1 is power required to work the screw in open water.

If $\frac{R}{T}$ is written $= (1 - t)$

$t = \frac{T - R}{T}$ measures the excess of screw thrust over ship resistance and is called the thrust deduction fraction.

If $\frac{V}{V_1}$ is written $= (1 + w)$

$w = \frac{V - V_1}{V_1}$ and gives the fractional excess of the velocity of the ship over that of the water past the screw and is called the wake fraction.

The efficiency can now be written:—

$$\begin{array}{c}\text{quasi propulsive}\\ \text{efficiency}\end{array} = (1 - t)(1 + w) \left(\begin{array}{c}\text{screw efficiency}\\ \text{in open water}\end{array}\right) \left(\begin{array}{c}\text{ratio of screw efficiency behind}\\ \text{model to that open water}\end{array}\right)$$

The product $(1 - t)(1 + w)$ is usually called the hull efficiency (h).

The effective horsepower when divided by the $\left(\begin{array}{c}\text{quasi propulsive}\\ \text{efficiency}\end{array}\right)$ will give the power required in the propeller shaft at the propeller not allowing anything for appendage or wind resistance, foul bottom, etc.

In fairly-full-ended, single-screw, cargo vessels the fore-and-aft flow at the water surface over the propeller tips is from aft to forward, and the presence of the screw when well buried does not always eliminate this flow. The actual slip angle must therefore be large at the surface and quite small at the keel, *i.e.*, the propeller works with a high thrust at low velocity near the top of its disc, and low thrust at high velocity near the bottom. Some of the water feeding into the propeller near the surface, comes from the sides or from aft of the propeller at the surface. For this water the wake fraction must be a large positive number, and if the water is thrown sideways may be infinity. On the other hand at the bottom of the disc the wake fraction must be quite

small. For this water near the top of the disc wake fraction really has no meaning and it can be varied to any extent provided the deadwater remains, and the work done will remain the same. The velocity through the screw is not dependent on the ship velocity at all in this part, but on the thrust of the screw, and the work done depends on the intake velocity induced by the thrust. In open water at all ordinary speeds and slips, the flow is always aft over and around the whole disc, the propeller works with practically the same thrust round any annulus, and the intake velocity is the same all round this annulus. To induce water to move *forward* over the advancing propeller, as it does behind the ship model, it is necessary to reduce the speed of advance to a low figure, and maintain the thrust quite high, and these conditions not only bear no resemblance to ordinary open screw running, but do not give the same curve of thrust constants as is obtained with normal working conditions.

Froude recognized that his assumptions needed justification by experiments, in all probability his experiments did not deal with ships like the ordinary cargo tramp. If the change in velocity over the disc varies steadily, and if this change is such that the thrust can at all parts be written as proportional to the slip angle, then the reduction of slip angle at one part balances its increase in the opposite part, and all Froude's assumptions hold good. But when the slip angle at any part exceeds that at which thrust varies as the square of velocity or as slip angle, the first assumption is bound to break down, and assumptions 2 and 3 also break down when the intake velocity in one part, is dependent on conditions quite different to those regulating the intake velocity in other parts. For these reasons the high wakes obtained with full ships by Froude's methods do not necessarily define the alternative uniform velocity behind the ship, and their use in calculating hull efficiency is open to question. It should be noted that high hull efficiency obtained with an incorrectly high wake, necessarily involves an incorrectly low screw efficiency behind, and a low relative rotative efficiency.

DISCUSSION.

Prof. Welch

THE CHAIRMAN, Prof. J. J. Welch, *Vice-President*, introducing Mr. Baker: I feel that the Tank authorities do a very wise thing in allowing their Superintendent to visit the various technical institutions in succession, so as to give the result of the research work done at the William Froude Tank; and I feel that whilst the institutions benefit by this, perhaps it is not altogether a disadvantage from the Tank point of view, for it gives Mr. Baker the opportunity of bringing the advantages of the Tank before a large number of people, and I notice that Mr. Baker has taken advantage of his opportunity on the present occasion. We are sure to have an excellent paper, and I hope the discussion will be worthy of it; I hope particularly that if there are any owners or owners' representatives present this evening they will give us the benefit of their experience.

After Mr. Baker had read his paper, PROF. WELCH said: I am sure that you will agree with me that we have had a very interesting paper—a paper full of facts and figures which I imagine it would be impossible to discuss exhaustively, at any rate in the time at our disposal. There is so much in it that needs considerable ventilation and so much that is of great value. I daresay you noticed, in his opening remarks, the offer that Mr. Baker made to read a second paper, and I am sure we

Prof. Welch

will all look forward and appreciate such a paper when it is ready. You would also note with much appreciation the enterprise of some local firms in assisting Mr. Baker in searching for definite information. Now, I do not propose to say much in the way of criticism or extended remarks. I feel we have, in Table 4, quite an interesting addition to our knowledge as to effect of shoal water on resistance, and Mr. Baker now discloses the fact that these are results which have been obtained at the Froude Tank. I think we shall gather from a general review of the Paper that Mr. Baker feels that the data ordinarily found in the engine-room log is not sufficiently complete to become the basis for scientific analysis; and he also comes to the conclusion—and I hope some people who have had experience of trials will make reference to it—that it is not easy to defend the common practice of running a ship at a very light displacement on trials.

Mr. Williams

Mr. H. G. WILLIAMS, O.B.E., *Member of Council*: Mr. Baker has given us a valuable and important paper in which he impartially and judicially surveys the unknown regions which lie between certain well explored fields of research. There is a large mass of reliable experimental information as to the tow-rope resistance of model ships up to about 30 feet in length; another body of reliable experimental information about the power and efficiency characteristics of small screw propellers up to about 16 or 18 inches in diameter when working in open water; and a much smaller mass of reliable experimental information about the effects produced on one another by model ships and model screws working in combination. There is practically no experimental information as to the tow-rope resistances of full-sized ships or as to the power and efficiency characteristics of large propellers in the open, though these quantities can be inferred with probably little error by applying the two laws of comparison, which are indisputably true when taken separately, but of somewhat doubtful application under the assumptions which have to be made as to their combined effect.

Trial results of ships give us a further large mass of information, mostly unreliable and incomplete, about the working of full-sized ships and propellers in combination. On the face of it, the naval architect who has to guarantee ship speeds on the lowest possible power has to do a good deal of guessing, and Mr. Baker's paper throws some light on this dark process.

Like, I suppose, most people interested in the powering of ships, I have made a practice of analysing all the trial results having any pretence to reliability that I have been able to get hold of, very much on the lines of Mr. Baker's analysis. That is to say, from the speed of ship and the revolutions and dimensions of the propeller one calculates on the basis of Froude's or Taylor's model screw data the thrust horse-power and the delivered horse-power, endeavouring by assumptions as to hull-efficiency elements and relative rotative efficiency, to strike

a balance on the one hand between e.h.p. and t.h.p., and on the other between d.h.p. and s.h.p. Mr. Williams

Like Mr. Baker, I have come to the conclusion that the power account can rarely or never be balanced on any assumption that one can call reasonable in the light of existing experimental information. I invariably find that the simplest way of striking an approximate balance is to assume a large loss between s.h.p. and d.h.p. Such a loss is inherently probable when one considers the flexibility of the ship and the probably imperfect alignment of the shaft. The fact that one finds little or no systematic difference in the propulsive efficiency between ships that are propelled by engines whose power is stated in s.h.p. and those which are propelled by engines whose power is stated in i.h.p. is consistent with the assumption that the loss in transmission from the engine to the propeller is large compared with the difference between the i.h.p. and s.h.p. of the engines.

A long time ago, in dealing with ships of moderate power driven by electric motors, I took the opportunity to measure the power required to run the propeller in dry dock up to quite high revolutions, keeping the stern bearings fully water lubricated as they would be in normal running, and found that in these ships it took to run the propellers under these conditions $\frac{1}{6}$th to $\frac{1}{4}$th of the power required to run the propeller at the same revolutions when propelling the vessel. By assuming that the power required to run the propeller in dry dock was the difference between the b.h.p. and d.h.p. when propelling the ship, the trial results became consistent with the results of model experiments on tow-rope resistance and hull efficiency and with standard model propeller data; and I have found corrections of the same order of magnitude necessary in general to get consistent analysis results out of the results of ship trials.

I should apologize to Mr. Baker for having spoken on his paper without having had time to study and comment on the actual analyses which he gives; but I gather that the burden of his song is the great need for accurate and scientific measurements, particularly of screw thrust and shaft torque on ship trials, and I heartily agree with him.

Mr. F. H. ALEXANDER, *Associate Member*: From this paper one gathers that, in spite of all the valuable work done on ship resistance and on screw propellers in the various experimental tanks, there is still a gap to bridge between model and ship. It appears that as far as ships of high speed are concerned, this gap is less noticeable than in relatively low-power ships. This leads to the inference that there is a factor concerned which does not vary in proportion to speed. To determine the factor experiments on full-sized ships are needed. Measurement of thrust and torque on trial and in service must be measured and related to the resistance of the ship. Mr. Alexander

It is good news that Mr. Baker is able to report success in obtaining support from some of our local progressive ship-owners, and hopes soon

Mr. Alexander to be able to carry out some experiments of this nature. Had this not been so, I would have suggested that the Institution take the matter up and appoint a small committee to design a suitable gear for measuring torque and thrust such that it could be fitted into a vessel for a voyage or two, and then be easily removed and replaced by an alternative piece of thrust shafting. It should of course be adaptable to a reasonably large range of powers so as to suit a number of vessels of varying sizes and speeds. In this way, the research department of the Institution might act as a link between experimenters and shipowners, so that the co-operation of the latter might be more easily obtained.

As regards deductions made from trial-trip results or from engineers' logs, and the use of the term "fine weather," I think that quite small waves may have a greater effect on resistance and on efficiency of screw than is usually assumed. The slip angle of a screw is very small, and small changes of speed of passing water make considerable changes of slip value, and of the resulting pressures on the screw blade. The inertia of the moving parts of the engine and the screw tend to prevent instantaneous alteration of revolutions to keep pace with these changing forces, so that the efficiency is reduced, and instead of a steady constant thrust a series of alternating accelerations and easings affect the ship. It is possible that the direction of the run of the waves may have to be taken into account, as a following sea tends to produce effects of longer duration than those due to a head sea; and again, a sea coming from the starboard side may affect a right-handed screw in a manner different from a sea on the port side.

All additional resistances to the ship, such as those due to wind and waves, are relatively of greater importance in slow than in fast ships. The results given last year in Mr. Kent's paper (Institution of Naval Architects) show that even quite low waves may cause considerable increase of resistance. It is quite possible that this additional resistance is not strictly a function of the speed, or of the smooth-water resistance, but is almost independent of either; and that it may even be less in quantity in a high-speed ship than in one of low speed.

If I understand Mr. Baker aright, the values of measured thrust alone are used in determining the wake at screw behind the model. Does it follow that the correct wake is obtained from thrust? If measured torque gives a lower wake speed than measure thrust, why should not the lower wake speed be correct? If there is no independent method of finding which is correct, why not use the mean between them? We are led to believe that correct wake speed is required for correct propeller design; the introduction of the "rotative efficiency" factor does not seem helpful in design, although it does lead to a safer estimate of probable overall efficiency. Can Mr. Baker give any reason why "relative rotative efficiency" should not be as much above unity as below it.

Mr. R. HINCHLIFFE, M.B.E., *Member of Council*: It is with some diffidence that I venture to discuss a paper writen by so eminent an authority as Mr. Baker, and dealing with so complicated a subject. The problems dealt with, however, are of intense interest to all those engaged in the design of ships and their machinery, and especially to those who have to decide the power that shall be installed in a new vessel with a view to obtaining a desired speed on service or on trial. Analyses, such as are given in the paper under discussion have, I feel sure, been attempted by many of those present to-night, often with very disappointing results; personally, I find some small consolation from the fact that Mr. Baker, with all his experience and knowledge, has to confess that all too often his results are difficult to explain, and that he has had the courage to publish them. Mr. Hinchliffe

It was my privilege to be present at the speed trials of the vessel designated "H" in Table III. This vessel was tried on the deep-water measured mile at St. Abbs, and obtained a speed in excess of that guaranteed. Owners and builders had every reason to be satisfied with the results obtained, yet those who had been responsible for the design could not but feel that—if the resistance of the hull was that which model experiments indicated it should be, the propeller efficiency that which the published results of model experiments would lead one to expect, the wake hull efficiency, and appendage and air resistance what experience with other vessels suggested as probable—the results ought to have been even better than they were.

And this is what happens again and again and points to the fact that our knowledge of the problems in connexion with ship propulsion is far from complete, and to the necessity for continued experiment and research in this field.

It is good news to learn that Mr. Baker is continuing his investigations, and that there is a prospect of his being able to read a further paper before the Institution dealing with this important subject. All ship designers will wish him, and all others working in the same field, complete and speedy success in their investigations, so that our methods of speed and power production may become more scientific and less empirical. Until our knowledge regarding this subject is more complete the discrepancies met with will tend to shake our faith in the soundness of the methods we use.

A long experience in the use of model data, garnered from a large number of experimental tanks, has convinced me that such data, as now obtained, are reliable and consistent. When, however, we begin to enlarge these results into the corresponding quantities for the full-sized vessel, our doubts begin to assert themselves. Is the law of skin friction we use correct? Are the coefficients in general use suitable for the actual surfaces met with in commercial vessels? What is the correct amount to add for appendage and air resistance? When these questions can be answered with certainty we shall be able accurately to foretell

Mr. Hinchliffe

the tow-rope resistance of our proposed ship. In this connexion the valuable results that would accrue from a carefully conducted series of towing experiments with a full-sized vessel are such as to inspire a hope that such investigations may be undertaken in the near future.

Turning to the propelling instrument, a similar list of questions remains to be answered. Are efficiencies obtained in open water applicable to the screw when working behind a vessel? Are the results obtained from the published results of Froude's or Taylor's experiments applicable to the average type of cast-iron propeller usually fitted on cargo vessels? What is the effect of thickness and shape of blade section upon efficiency and effective pitch? Are wake and hull efficiencies obtained from models directly applicable to the full-sized ship?

Towards the solution of these and similar problems much progress has been made during recent years by those engaged upon research work, and we can confidently look forward to further enlightenment in the near future. All interested in ship propulsion will look forward to the time when the determination of speed and power can be regarded as an exact science, and when it will no longer be necessary to follow the advice given by Adm. D. W. Taylor—to be a pessimist when assigning power.

Mr. Ainslie

Mr. A. F. AINSLIE, *Member*: I regret very much I have been unable to give this excellent paper the study it deserves and demands, for in Table III. alone "there is life and food for future years." I can confirm the high values for wake fraction which are given for single-screw ships. It is sometimes difficult for people who have been accustomed to deal with finely shaped high-powered vessels having wake fraction from a negative value to 15 and 20 per cent. positive, to realize that for slow full cargo boats the wake is often about 40 per cent. or more, and it is all to the good that Mr. Baker has published these results, and it should give general confidence in the analysis of trial-trip data when in competent hands.

It is no child's play to conduct trials in a manner to give sufficient exactitude, and it requires considerable experience and knowledge of how the various values are arrived at, so that when analysed the results shall not vary beyond what can be termed errors of observation. And because of the many unknown conditions even greater care is necessary when comparing among themselves figures given by different experimenters.

Mr. Baker says "the . . . propeller dimensions . . . are accepted as accurate." For solid screws my experience supports this for all practical purposes, but for built-up propellers where loose blades are bolted to the boss I have found the diameter almost invariably greater than nominal—sometimes as much as six inches—and always required checking. The more people who take a live interest in ship propulsion the better, for, after all, it is the more or less hidden screw which drives the ship.

Mr. E. V. TELFER, *Associate Member*: In this paper Mr. Baker has given us valuable food for thought. His frank exposal of the difficulties that may be associated with the correlation of model experiments and ship trials, must be admired by all. Mr. Baker's hope that the discussion of his paper will result in the advancement of the subject is an honour paid to our Institution which we should do our utmost to justify. Mr. Telfer

To correlate two sets of data both sets should be scientifically beyond reproach; if otherwise, the task is impossible and worse than useless. Some of Mr. Baker's examples illustrate this point very forcibly. The other examples, in which the conduct of the experiments and the trials left nothing to be desired, reveal discrepancies which require very careful consideration. One source of error mentioned by Mr. Baker is the hypothetical nature of our knowledge of the frictional resistance of ships. So far as our ignorance is due to the paucity of plank data at high values of V.L. this has been ameliorated by Gebers, whose maximum V.L. product was 75 m^2/sec as against 46 m^2/sec of Froude. When Dr. Gebers' work on the frictional resistance of ship forms is published, this branch of the subject will be put upon a much more satisfactory basis.

Mr. Baker's reference to scale effect in connexion with propellers is interesting. I must confess surprise that scale effect "shows itself as a slight reduction of pitch in the ship." It is generally supposed, and Gebers has shown definitely, that the efficiency of a propeller increases with its absolute size. This increase in efficiency is equivalent, in the Froude screw, to an increase in the effective pitch for all values of real slip ratio up to at least 30 per cent., *i.e.*, for the majority of slips likely to be met with in practice.

From a consideration of the Lanchester-Prandtl theory of air-screws, in which the performance of each blade element is deduced from that of an equivalent aerofoil of infinite aspect-ratio, the increase of efficiency with absolute size may be easily demonstrated. Mr. McKinnon Wood, in his recent paper before the Royal Aeronautical Society,* showed that the scale effect upon an aerofoil was to reduce both the lift and the drag coefficients, the latter, however, more than the former. The efficiency of the element depends upon, and increases for all normal angles of incidence with, the lift-drag ratio. As the scale effect is to increase this ratio—very slightly, admittedly—the efficiency of the large screw will be increased and thus its effective pitch. The fact that propeller performance depends upon the ratio of two quantities both affected by viscosity, explains the relative unimportance of frictional resistance in the theory of propeller similitude. It would seem that the true influence of scale effect is to admit of the larger of two hydrodynamically similar screws having a smaller geometrical pitch ratio than the smaller screw for equal efficiency. This view is apparently opposed to that of

* Dec., 1922.

Mr. Telfer

Mr. Froude and Mr. Baker, and in view of the controversy which exists as to the precise significance of the 1·02 factor recommended by Mr. Froude, *i.e.*, whether from its derivation it is a pure scale effect factor or an ordinary correlation factor, subject to many influences, is a point which should be cleared up, for the benefit of the subject.

The importance of scale-effect in ship-model performance has long been realized. Froude's pioneer work on frictional resistance, and his segregation of total resistance into wave-making and frictional must be regarded as the earliest solution of scale-effect. However, the full significance of scale effect is presumably not recognized in one and the same definite manner by all experimenters in this and other countries, so far as one is able to judge the present practice of the various experimenters from their most recently published works. This brings me to my chief remarks, which refer to ship and model wake. I think, from a survey of the literature of the subject, that I am correct in stating that Messrs. Baker's, Luke's, Froude's and McEntee's practice in the prosecution of joint screw and ship model experiments, is so to regulate the revolutions of the screw that the registered screw thrust is equal to the registered model resistance. This equality occurs at what is called the point of self-propulsion. Schaffran, however, has pointed out that the thrust so obtained does not correspond to the thrust required in the ship because of the relatively greater frictional resistance of the model. To obviate this scale-effect, the frictional resistance of the model is artificially reduced to correspond to that of the ship by only developing in the screw dynomometer the requisite thrust corresponding to that of the ship. In a self-propelled model this correction is affected by actually towing the model by a force equal to this amount. Schaffran has shown by a manipulation of Froude's frictional coefficients, how the amount of this correction may be calculated, but the correction can be deduced in a simpler form by making use of Geber's recent work on frictional resistance.

In the paper which I had the honour to read before this Institution in December last I showed that when the resistance of a model was expressed

$$r = w + f \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

then the resistance of the ship λ times its dimensions at the corresponding speed became

$$R = w\lambda^3 + f\lambda^{2\cdot8125} = S_s(1 - t) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which S_s is the thrust of the ship's propeller and t is the thrust deduction factor.

From (2) the true thrust required for the model in order to correspond to the ship should be—

$$\frac{S_s}{\lambda^3} = S_m, \text{ and since therefore,}$$

$$S_m(1 - t) = w + f\lambda^{-\cdot187}$$

we have $$S_m = w + f\lambda^{-\cdot1875} + tS_m \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Mr Telfer

From (1) when the propeller is working behind the model and producing a thrust S_m, the resistance of the model will be increased by an amount $t.S_m$, the total resistance then registered being—

$$r_1 = w + f + t S_m \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1a)$$

The difference between (3) and (1*a*) gives the required correction, thus—

$$(r_1 - S_m) = f(1 - \lambda^{-\cdot 1875}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This correction is not a small one; when $\lambda = 25$ the correction approximately equals ·45 f and for $\lambda = 50$ it becomes ·52 f.

Of course this analysis assumes that the conditions of the model precisely duplicate those of the ship. In present model experiment methods this is not the case, butts and laps being present on the ship but not on the model. In view of this difference, equation 2 above becomes for the usual model experiment—

$$R = w\lambda^3 + c.f.\lambda^{2\cdot 812} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2a)$$

c being a factor which represents the difference of a condition between the model and the ship. Thus the model thrust correction in this case is less than in the previous, and becomes—

$$f(1 - c\,\lambda^{-\cdot 1875}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4a)$$

It is probable that when research and comparison with full size trials have made possible a correct assessment of the value c, the Gebers frictional coefficients thus modified for c, will be in close agreement with the coefficients proposed by Mr. R. E. Froude and which were, of course, deduced from premises quite different from the above.

The effect of the thrust correction is chiefly to reduce the revolutions required for self-propulsion. To arrive at the wake factor Schaffran uses an analogous procedure to Froude. The open performance of the particular screw is plotted on a base of nominal slip ratio, the quantities plotted being the thrust constant $\frac{S}{n^2H^2D^2}$, the torque constant $\frac{M}{n^2D^2H^3}$ and the efficiency.

The influence of the thrust correction on the thrust constant is very slight because S varies practically as n^2. Hence the nominal slip ratio, with and without the correction, is practically constant, and therefore also the efficiency. However, the wake corresponding to this nominal slip, with the correction, is much greater than without it. It can be simply shown to equal—

$$w = (S_n - S_a)\frac{V}{nH}$$

where S_n = nominal slip ratio.

S_a = apparent slip.

V = Ship or model speed (m/sec).

n = revs. per second.

H = Face or nominal pitch (m).

In this expression the wake fraction is not the Froude presentation, but the Taylor. The Taylor expression is exclusively used in America and Germany. From this expression it is directly seen that the effect

Mr. Telfer

of decreased revolutions is increased wake. McEntee, in his work on self-propelled models, used Schaffran's propeller analysis, but did not make the requisite correction to the model screw thrust. Schaffran had carried out similar experiments to those of McEntee, and has since shown that when this correction is made both sets of experiments are in agreement. Schaffran's work has been recently substantiated by Dr. Bauer's careful trials of vessels for which experiments had been made.

I have dealt with this question at some length to show that we must reconcile ourselves to high wakes for slow speed single screw vessels, and that the usual wakes given by Luke and others must be regarded as too small. I think that my remarks are relevant to Mr. Baker's Paper, for it seems that his model thrusts have not been modified to correspond to the ship conditions. Had this been so, much higher wakes would have been obtained, with, I think, a closer agreement between model and ship results. This scale effect correction is only the first step towards the increasing of model experiment technique. Other model screw thrust corrections such as for the resistance of butts, laps, etc., on the ship will bring even closer agreement between model and ship results. As to whether the wake of the model obtained in this manner corresponds to that of the ship is a point upon which great difference of opinion exists. Mr. Baker considers that the ship wake is less. Mr. A. W. Johns, Prof. Havelock and more recently Prof. Bragg, apparently from their published works, hold the reverse opinion. Schaffran considers that the wakes are identical; and the agreement of Dr. Bauer's work and Dr. Schaffran's certainly substantiates this belief. Personally, I agree with Dr. Schaffran, and have found analytically that a careful consideration of the Rayleigh Law also supports this belief. If nothing else, the belief is a comforting one, and I would like to secure Mr. Baker's definite opinion upon this controversial and important question.

Passing over the examples given by Mr. Baker and which refer to his own experiments, I should like to comment upon his analysis of Dr. Bauer's work given in example 1. This vessel is presumably the "Cairo," whose particulars were given in the *Werft und Reederei* for 22nd October, 1921. These particulars I translated, and they were published in the *Shipbuilding and Shipping Record* for 25th May, 1922. Mr. Baker is evidently satisfied, as indeed we all must be, with Bauer's conduct of the particular trial. Mr. Baker's independent analysis is very interesting. Bauer's analysis was based on the Schaffran screw, and Mr. Baker's on the Froude. Bauer's wake was 44·5 per cent., and Mr. Baker's $\frac{\cdot 92}{1 + \cdot 92}$, *i.e.*, practically 48 per cent. The open efficiency of the propeller was 57·8 per cent. with the Schaffran, and presumably—from Mr. Baker's remarks—with the Froude type 58 per cent. This is very good agreement. However, the agreement with the relative rotative

efficiency is poor. From Bauer's experiments the actual propeller efficiency behind the ship can be very simply obtained from the wake obtained from the modern propeller results over the thrust of the propeller is known from the expression (in metric units)— Mr. Telfer

$$\eta_p = \frac{S \cdot V(1-w)}{75 P_s}$$

For the "Cairo" with $S = 7{,}810$ kg.; $V = \cdot 5144 \times 9\cdot 75 = 5$ m/sec,

$w = \cdot 455$ and $P_s = 505 \cdot \eta_s$ where η_s is the shaft efficiency,

which as $\eta_s = 98\cdot 5$ per cent. $= 496$

we have $$\eta_p = \frac{7810 \times 5(1 - \cdot 445)}{75 \times 496} = 58 \text{ per cent.}$$

The only reference to model experiment here is in the assessment of wake, and the efficiency so obtained must be looked upon as the true behind-ship efficiency of the propeller. The relative rotative efficiency is thus practically unity, and could hardly drop so low as 85 per cent.

Thrust experiments, as emphasized by Mr. Baker and demonstrated by Dr. Bauer, are of unquestionable importance in the correlation of model and ship data. Dr. Bauer's most recent paper before the German Society of Naval Architects read in November last gives further valuable information. Measurements and model experiments were carried out on twin-screw steamers "Preussen," "Antonio Delphino" and "Sierra Nevada," and show good agreement between model and full-scale result. Dr. Bauer, however, is not entirely satisfied with the conduct of the trials, and hopes to carry out still more reliable work, and thus increase the accuracy of correlation. I sincerely hope that Mr. Baker's Paper will convince shipowners of the advisability of thrust measurement on their ships in addition to model experiments, and finally would like to thank Mr. Baker for the instructive information which his paper undoubtedly contains.

Mr. J. CALDERWOOD, *Graduate*: Mr. Baker's paper is one of very great interest to all who have to attempt to obtain from trial trips reliable information for estimating the performance of future ships. The suggestion is made in the Paper that more accurate values of power will be obtained by the use of torsion-meters and thrust-meters on reciprocating-engined vessels. The exact measurement of power by a torsion-meter is much more difficult than would at first appear to be the case. Actual measurements have shown a torque variation of 20 per cent. in the case of one turbine-driven vessel, whilst a variation of over 60 per cent. has been measured in a reciprocating-engined vessel. To obtain a mean value of the torque, readings must be taken at some 20 or 30 different propeller positions which cannot be done during the course of a run on the measured mile; alternatively, a continuous recording or an averaging torsion-meter may be used, but I do not think that there exists an instrument of either of these types which will give values more accurate than can be obtained by carefully taken Mr. Calderwood

Mr. Calderwood indicator diagrams and a mechanical efficiency estimated from the friction of the engine-tunnel bearing, etc., which can be obtained by drawing a curve of mean effective pressure to revolutions per minute, and producing it to cut the zero speed line.

The very low propeller efficiencies obtained in service are, I think, often due to the cyclical variation of speed of rotation of the propeller. Measurements made in various vessels have shown speed variations at the propeller amounting to as much as 12 per cent. in the case of reciprocating engines, and 4 per cent. in the case of turbine-driven vessels. The exact amount of this speed variation will be different for every type of machinery installation, and this may account for the great discrepancies often observed in the analysis of results from similar ships with different classes of machinery.

Mr. Baker mentions the effect of air resistance and the increase due to wind; I think it would be of great interest if he could give us more information on this point as I do not know of any published method of estimating air resistance that gives accurate results. As it is not always possible to choose a still day for a trial trip, it would be of great value, when analysing trial results, to have available some accurate method of making a correction for the increase of resistance due to wind.

Another subject on which information would be of great value is the effect on resistance of fouling of the ship's bottom and of waves; such information would make possible the useful analysis of many trial-trip and voyage results which are otherwise of no value, and I should like to ask Mr. Baker whether in comparing model experiments with trial results he has been able to obtain any information on this point.

There appears to have been a tendency in some quarters to assume the frictional resistance to follow a known law for all sizes of surface and speeds of motion, and I am glad to notice that Mr. Baker points out that such is not yet the case, and that very little data for friction on great lengths and at high speeds is available. Research on the increase of resistance due to plate joints and other irregularities that occur on the actual ship would be of great value.

In analysing trial results it is usually found that the wake factor is perhaps the least reliable of the results obtained. Even in ships of similar form in which all other results appear to compare favourably it is often found that the difference in wake factor is very considerable. A number of formulæ has been published for estimating the wake factor from a consideration of the Δ co-efficient of the ship, and I should like to know whether Mr. Baker considers any of these to be reliable. It would also be of interest if he could give any figures showing the relation between propeller efficiencies in undisturbed water and in the uneven wake stream behind a ship.

Mr. Calderwood

In a recent number of *Werft, Reederei, Hafen* there appeared an article in which were described the trial trips of a number of boats fitted with a set of stationary vanes behind the propeller, known as a "Contrapropeller." The improvement in performance due to the increased propeller efficiency was (if the figures given are reliable) very remarkable. This is perhaps outside the scope of the Paper, but in view of the very high efficiencies claimed in the above-mentioned German article, it is a subject on which any information that Mr. Baker could give us would be of very great interest.

The comparisons of model and trial trip results given in the Paper show clearly the errors that are liable to occur in the analysis of data obtained by the latter method, and all such information is of very great value to those who have to attempt these analyses.

Mr. McAlister

MR. F. MCALISTER, *Graduate*: From the tone of Mr. Baker's introduction I gather that his main object is the discussion of analysis of trial trips, and I trust that before the evening is over his object will be achieved. I think we have here, though, another of the Froude Tank papers which are distinguished by their frankness of view, and for this reason—their seeking after the truth—stand out prominently in this field of research

I take the liberty of adding my applause to Mr. Baker's plea for the installation of thrust and torque apparatus upon merchant ships, on the principle that one of the subtlest of factors is the r.r.e., and that by the fitting of such apparatus it will give us definite values for this efficiency.

I have been a worker to a small extent in the field covered by the Paper, and the method I adopted was this. In the absence of e.h.p. curves for the vessels I worked on, I made an estimate of this from Taylor's Standard series, and checked the naked e.h.p. wherever possible by comparison with the published particulars of the nearest models run in the various tanks. To this was added 7 per cent. to allow for bilge keels, air resistance, weather, etc., and I observe that Mr. Baker in § 8 adds 8 per cent. Now, taking the i.h.p. on trial, I reduced this to s.h.p., basing my engine efficiency on particulars in Mr. Blechynden's paper before this Institution in 1891, and also on Taylor's values for dead load friction, and on the strength of Mr. Holt's figure of 0·85 for r.r.e., I assigned a nominal value to all my work of 0·90, and further reduced the s.h.p. to, say, p.h.p.

From Froude's memorable 1908 paper the analysis of the propeller was made, assuming 1·02 × face = analysis pitch, and from the efficiencies so obtained I deduced from my p.h.p.'s the t.h.p. downwards from the engine. These were plotted on a base of real slip. On the same base were plotted the t.h.p.'s of the propeller itself from Froude's diagrams, and where the two t.h.p. curves crossed I assumed this to be the point of self propulsion. Knowing the apparent slip on trial, wake can be plotted on the same base from the relation

$$\text{wake} = \frac{1 - \text{Apparent Slip}}{1 - \text{Real Slip}} - 1.$$

Mr. McAlister

All the wake values so obtained lay reasonably well on a line parallel to that of Luke's for single-screw ships as given in the *Transactions* of the Institution of Naval Architects, 1917, and of an average value of 0·025 greater than Luke's. The real slips at which equilibrium occurred ranged from 34·0 to 37 per cent.

The analysis was confined to reliable measured-mile trials on vessels fully loaded to the summer mark. I may have been specially favoured as I had no difficulties as Mr. Baker has had in reconciling wake factors and powers.

May I ask whether the 6 per cent. reduction of Froude's "B" factor mentioned in § 17 is a moderately constant figure to adopt for vessels tried at ballast drafts with the tips of screw just immersed or whether the 6 per cent. was only an individual instance where agreement was reached by using the 6 per cent. figure.

CORRESPONDENCE.

Sir Eustace D'Eyncourt

SIR EUSTACE H. TENNYSON D'EYNCOURT, K.C.B., F.R.S., *Hon. Fellow*: We are very much indebted to Mr. Baker for another most valuable contribution towards what is probably the most difficult task of the ship designer, viz., that of apportioning the correct horse-power and propellers to proposed ships when in the design stage; and I should like to take this opportunity of saying how very important it is that all shipowners and shipbuilders should get all the observations possible from their trial trips, with carefully recorded data and with all the information that they can get from measured-mile runs, both at full speed, and, if possible, at lower speeds. The more of this sort of information that is available the better, as it will then be easier to connect the results of model experiments with those for the full-size ship. The analysis of such trial results provides information as to the application of the model resistance and screw experiments to the full scale. The difficulty and expense of making full-scale experiments, in such a way that detailed measurements similar to those made with models are obtained, are so great that the only practicable alternative is properly conducted measured-mile trials. Mr. W. Froude's classical experiment on H.M.S. "Greyhound," in 1872, and Sir Alfred Yarrow's somewhat similar experiments with a torpedo boat in 1883, provided the necessary confirmation of the application of the results of model resistance experiments to the full scale; but the corresponding confirmation for propellers has not been, up to recently, a practicable experiment. The use of the Michell thrust-block for the measurement of propeller thrust, should enable us to obtain the required information, if it were practicable to make a sufficient number of trials with ships—speed, power, thrust, etc., being measured and recorded simultaneously.

Mr. Baker's paper shows clearly that the only information as to the performance of ships which is sufficiently reliable for correlating model results must be obtained from carefully made measured-mile

trials. The data given in the deck and engine-room logs and those obtained by the use of patent logs are shown to be of little use for the above purpose, however interesting and useful to the shipowner when carefully analysed. Sir Eustace D'Eyncourt

The advances made in the speed and size of ships, especially warships, could not have been made so confidently as they have been, had measured-mile trials not been made.

Capt. WILLIAM McENTEE, *Constructional Corps, U.S. Navy*: The principal impression I get from reading the Paper is the almost total lack of really reliable trial data for mercantile ships run under known conditions, and I presume it is probably one of Mr. Baker's principal objects, in writing the Paper, to bring this point forcibly to the attention of those interested in this subject. In certain similar analyses which I have made on vessels built for the U.S. Navy, where great care is exercised in running the trials over a measured mile, discrepancies have occurred between the ship data and estimates made from self-propelled model tests which are difficult, if not impossible, to explain on any rational system. In other cases, however, the comparisons have been remarkably good. There is small wonder then, where the trial data are not carefully taken, that large discrepancies should be found, as in a number of instances cited in this paper. Capt. McEntee

In making comparisons between the results obtained from model tests with screws and the trials of the full-size ship, there is one source of possible error which the Author does not mention. As I understand it, the model-screw apparatus used at the tank which is under the direction of the Author, has the screws supported independently of the ship model; in other words, the model screws are not driven by shafts extending from the inside of the ship model, as is the case at the Washington tank. The model and the full-size ship lack similarity in that respect. How much, if any, it affects the results in comparisons it is, of course, quite impossible to say, and could really only be determined for a particular ship model by making screw tests by the two different methods.

In view of the considerable number of unknown factors entering into the problem, the advantage to be derived from following Froude's method in estimating the difference between the true pitch and the face pitch of the propeller blades is not clear to me. The method followed at the Washington tank is to deal only with primary data in comparing the model and the full-size ship. A series of self-propelled runs is made with the model at different speeds, observing the torque, thrust, the r.p.m. and the speed. From these data the estimates for the ship are made.

As the method may be of interest in connexion with the subject of this paper, the following short description of its essential features is given:—

Capt. McEntee

For the model, let—

r = resistance of model and appendages in lb.

t = thrust on propeller shaft.

n = r.p.m.

q = torque in lb. feet.

v = speed in knots.

For the ship let R, T, N, Q and V be the corresponding quantities. If the ratio of the length of ship to length of model be l, then at corresponding speeds, $V = \sqrt[2]{l}$, and if all of the resistance varied in accordance with Froude's law, we should have—

$$R = r \times l^3$$

$$T = t \times l^3$$

$$N = \frac{n}{\sqrt{l}}$$

$$Q = q \times l^4$$

But on account of the relatively less frictional resistance of the ship, R must be reduced by a percentage $\frac{\triangle R}{R}$, which is readily estimated for the vessel by model tests and tables of frictional coefficients.

If the thrust deduction coefficient is assumed to be the ame for ship and model, we shall also have to reduce T by a percentage $\frac{\triangle T}{T}$ in order to get the corrected thrust. This is readily done, since $\frac{\triangle T}{T} = \frac{\triangle R}{R}$

Similarly, a correction must be applied to N and Q. To get these corrections, $\triangle n$ and $\triangle q$, we must know the slip at which the screw actually works behind the model. This is obtained from separate tests of the model screw run in the open or in free water, from which tests the thrust coefficient, $\frac{t}{n^2d^2}$, and the torque coefficient $\frac{q}{n^2d^5}$, are obtained and plotted on a basis of nominal slip, *i.e.*, slip computed from the geometrical pitch surface of the propeller blades.

With these coefficients computed from the self-propelled model data, the corresponding slips may be taken from the above plots. The slips as determined from the thrust and the torque measurements are usually in close agreement, and the mean value is taken as the true nominal slip, though theoretically, perhaps, the slip determined by the thrust coefficient alone should be used.

As explained before, we have made a percentage correction $\frac{\triangle T}{T}$ to the thrust, and we now wish to apply a corresponding correction $\frac{\triangle N}{N}$ to the r.p.m.

Since $N = \frac{V \times 101{\cdot}3}{P\,(1-s)}$ $\qquad \frac{\triangle N}{N} = -\frac{\triangle s}{1-s}$

Capt. McEntee

we compute values of $\dfrac{\dfrac{\Delta N}{N}}{\dfrac{\Delta T}{T}}$ and $\dfrac{\dfrac{\Delta Q}{Q}}{\dfrac{\Delta T}{T}}$

from the propeller coefficients and plot them on the basis of slip. Thus, for any particular slip for any given value of $\dfrac{\Delta T}{T}$, $\dfrac{\Delta N}{N}$ and $\dfrac{\Delta Q}{Q}$ are readily found. For example, suppose in a particular case $\dfrac{\Delta T}{T} = 4$ per cent. and $\dfrac{\dfrac{\Delta N}{N}}{\dfrac{\Delta T}{T}} = 0{\cdot}25$, then the correction to N will be

$$4 \times 0{\cdot}25 = 1 \text{ per cent.}$$

In other words, at a given speed of ship in this particular case, a reduction of 1 per cent. in revolutions will reduce the thrust by 4 per cent. In a similar manner the correction to be applied to Q is obtained.

The s.h.p. is estimated from the corrected values of N and Q.

It is to be noted that this method is free from any particular value of thrust and wake coefficients, and involves only the assumption with regard to them that they are of equal value for ship and model. It also has the marked advantage that in case of disagreement between the trial data and the estimates made for the model tests, it shows at once whether the variations of power and r.p.m. from the estimates are mutually consistent.

For example, in the above case, suppose the trial r.p.m. comes out 3 per cent. higher than the estimates, and the s.h.p. 12 per cent. higher. This would be consistent, as we should expect the percentage variation, if any, from the estimates to be in about this ratio. It would also indicate that the resistance of the ship was under-estimated by about 12 per cent.

Rear-Admiral Taylor

Rear-Admiral D. W. TAYLOR, *Constructional Corps, U.S. Navy, Retired*: It gives me great pleasure to accept the invitation to join in the discussion of Mr. Baker's paper. The subject is one of prime interest to those of us who believe that model tests are of great practical value and that in the end model tanks must be judged by the accuracy with which they enable the performance of full-sized ships to be predicted. From this point of view this paper is at first sight not very encouraging, as it brings out a number of discrepancies which seem difficult to explain. But upon consideration we find that there are factors to be considered which give the matter a different aspect. Before taking them up, I think particular attention should be invited to the difficulty of obtaining accurate data as to the performance of ships in service. We need concurrent and correct values of power, revolutions and speed. These are not easy to obtain, even on speed trials with an ample staff. Under such conditions it is easy to obtain curves giving relations between

Rear-Admiral Taylor

power, revolutions, and speed, but anyone who has attempted to reconcile such curves, obtained apparently under identical conditions, for sister ships or for the same ship tried at different times, will soon conclude that results of even scientifically conducted trials cannot be regarded as exact. So it is no wonder that reports from a ship in service contain discrepancies, without regard to any comparison with estimates from models.

This paper deals with rather slow, full, merchant vessels. If the Author were permitted to give similar information dealing with vessels of war for which good trial data with clean bottoms were available, the discrepancies would be very small. Why the difference? In the first place, the curves of effective horse-power for these vessels were not deduced from experiment in any existing model tank. By far the larger part of the e.h.p.—I presume 80 per cent. or more in some cases—consists of the frictional e.h.p. estimated for the full-sized ship from the results of tests of friction planes made by Mr. William Froude over fifty years ago in his 300-foot model tank in his garden at Torquay. Froude's largest plane was, I believe, 19 inches wide and 50 feet long, and his maximum speed about 8 knots. While Mr. Froude's coefficients of friction have been substantially confirmed by later tank experiments, naval architects have recognized for years the desirability of new frictional coefficients established by large-scale experiments and not by inference from small-scale experiments.

The necessary experiments need not be very elaborate or costly, and may, I hope, be made before many years; but until they are made we have present a factor of uncertainty which is liable to cause discrepancies—though, I believe, nothing like the whole of the large discrepancies disclosed in this paper.

There is a second factor of uncertainty—also small. Recurring for a moment to elementary principles, how do we estimate the effective horse-power of a ship from the resistance of a model? We deduct from the resistance of the model its estimated frictional resistance. This leaves us the residuary resistance of the model. From it, by Froude's law, we calculate the residuary e.h.p. of the ship. We estimate the frictional e.h.p. of the ship as indicated above and add it to the residuary e.h.p. of the ship to obtain her total e.h.p. Now, the residuary resistance of the model is made up of the wave-making resistance and the eddy resistance. There seems no question that the former follows, with beautiful accuracy, Froude's law. We know that the eddy resistance cannot follow exactly Froude's law, but it does follow with usually sufficient approximation for practical purposes, particularly as it is a very small element of total resistance in most cases.

But in full-sterned ships we are liable to have eddies and dead water behind the actual ship that have no counter-part in the model, and mean added e.h.p. beyond that deduced from model resistance. In view of the type of ships dealt with in this paper it is possible that

there were small perturbations in some cases due to this extra eddy resistance. Rear-Admiral Taylor

I regard the preceding as minor factors. But there is a major factor impossible to deal with by model experiment with present facilities, which may readily account for practically all the discrepancies noted. I refer to the increased resistance due to fouling. This is a most erratic factor, and it is hardly practicable to reduce it to rule. In 1915 (*Transactions*, Society of Naval Architects and Marine Engineers), McEntee published results of tests of friction of twelve plates that were submerged at a point in the Chesapeake Bay near the U.S. Navy Yard, Norfolk, Va., and removed one by one at intervals of a month and tested for frictional resistance. After a year's submergence, the friction was four times as great as when clean, but the increase was more rapid in the early months. In four months the increase was 175 per cent. of the original, or at the rate of nearly $1\frac{1}{2}$ per cent. per day of submergence.

Of course this fouling is materially affected by time of year and location. In the discussion of McEntee's paper Sir Archibald Denny made a very significant statement referring to the experience of his firm at Dumbarton. He said:

> "We have found here that fouling which does not exhibit any visible growth, is also prejudicial. After launching, the vessels lie in brackish water in our fitting-out basin, and it is our practice, as often as possible, to run a preliminary trial before docking and a complete set of progressive trials after docking, when we frequently get an increase in speed of half or even three-quarters of a knot.
>
> "Examining the bottoms in dock we find no apparent fouling; indeed the surface seems exceedingly smooth and even slippery. By repeated comparisons before and after docking, we have got a figure for the effect of this fouling, namely, that for each day a vessel lies in our dock the skin friction resistance increases at the rate of nearly $\frac{1}{2}$ per cent. per day, and this we have found to be true for periods as long as three months."

Experience with U.S. naval vessels confirms the above, not only as regards the material increase of resistance with fouling, but as regards appreciable increase with little or no growth.

There is another factor which was doubtless present in many of the cases dealt with in this paper. McEntee has investigated experimentally (Society of Naval Architects and Marine Engineers, Vol. 24, 1916) the effect of foulness and roughness of blades upon propeller efficiency, using 16-inch model propellers. When an efficient propeller with smooth blades was varnished and peppered with ground cork its efficiency went down from 72 to 36 per cent. This, of course, was extreme. But he also tested four 16-inch propellers, one of bronze, finished and polished all over as is usual with model propellers, the other three, bronze, cast steel, and cast iron, cast as nearly as possible to the finished dimensions of the smooth propeller and tested as cast. The smooth model showed a maximum efficiency of 72 per cent., the other three of about 63 per cent.

Rear-Admiral Taylor

It is not quite clear to me from the Paper whether the e.h.p.'s of the Paper include any allowance for the effect of fouling. Apparently they do not, but even if they do it is evidently a factor hardly possible to evaluate with accuracy for the full-sized ship in service.

Referring to Tables I., II., and III., I venture to take mild exception to the use of the term "Wake Fraction." Froude in his masterly paper of 1883 seems to have referred the speed of the wake not to the speed of the ship but to the speed of ship less speed of wake, or the speed of advance of the propeller through the water in which it worked. The ratio between the two he characterized by the excellently descriptive term "Wake Percentage," though he usually speaks of Wake Factor merely, and "Wake Percentage" is used in one place only. Many years later, wishing to connect this following wake directly with the speed of the ship, I expressed it as a fraction of the latter, and called this fraction the "Wake Fraction." Not a very happy term, but I carefully explained it at the time for the purpose of avoiding confusion. "Wake Fraction," as defined by me, has been commonly used in the United States, but in England, as in this paper, there seems a tendency to use "Wake Fraction" to denote a different thing—what Froude called "Wake Percentage." There is a very simple relation connecting each with Froude's Wake Factor. We have—

$$\text{Wake Factor} = (1 + \text{Wake Percentage}) = \frac{1}{1 - \text{Wake Fraction}}$$

The Author seems to hesitate at large wakes, but those given by him appear small rather than large to me. Experiments recently made at the U.S. Model Basin with a 20-foot model of 75 per cent. block propelled by a single screw, seemed to indicate a 40 per cent. wake fraction corresponding to $66\frac{2}{3}$ per cent. wake percentage as quite likely. An extensive investigation by Prof. Bragg, the subject of his paper last November before the Society of Naval Architects and Marine Engineers, also showed a number of even larger wake values for extremely full models. In dealing with these tricky wakes I think it is distinctly desirable to use a model propeller very carefully fixed with reference to the model it is driving, in a location accurately known.

The deduced wakes in the tables seem somewhat erratic, but it seems notable that perhaps the most reliable one—that for Ship I.—is by far the largest. A wake percentage of 92 would mean a wake fraction of 48—a large value indeed, but apparently not impossible in view of recent experiments in the United States.

Mr. Mumford

MR. EDWIN R. MUMFORD (*Messrs. William Denny and Brothers, Ltd.*): This paper gives a method of analysis of the measured power and revolutions per minute in terms of speed of any ship for the purpose of estimating various efficiencies, and should prove useful to shipowners and shipbuilders who take the precaution to ensure accurate results in order to make the best use of the method. The results obtained by builders are usually from trials with a more or less clean bottom surface

over a measured distance, in weather that is not too unfavourable to attaining the guaranteed speed. On actual service conditions the

Mr. Mumford

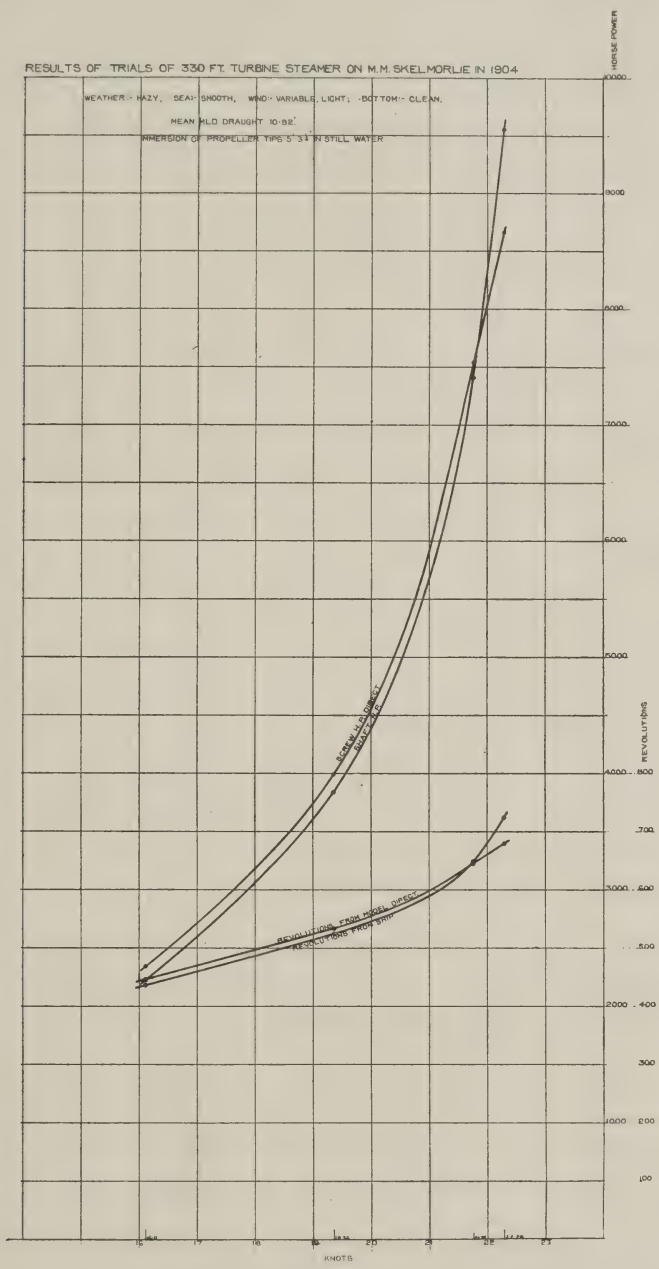

FIG. 2.

draught of the ship in many cases exceeds the draught while the trials were being made over the measured distance, and it should be particularly useful to owners to be able to apply a method of analysis to

Mr. Mumford

such cases so as to compare the efficiencies with those obtained on the measured distance at a lighter draught. The Author of the Paper has necessarily had to make several assumptions in making his analyses, and is careful to point out causes of errors; but many of the causes of error do not seem to be serious, and if the result of the Paper is to persuade owners to obtain a large accumulation of trustworthy results, all the possible errors would be considerably reduced in magnitude. All owners are naturally concerned as to the amount of fuel consumed in relation to the conditions, and that is the ultimate test of overall efficiency; but when it is possible to divide the overall efficiency into its component factors, it is possible to ascertain the cause of an excessive consumption of fuel.

With reference to the causes of error specified by the Author in § 5, it is doubtful whether the absence of accurate data as to the skin friction of long surfaces at high speeds leads to serious error. Froude's results, obtained with thin planes of limited length tried at comparatively low speeds, had to be theoretically extended for application to ship dimensions and speeds; and while this involves a probability of error, the error becomes practically negligible when comparing the results of ships of about the same length and speed. The cost of an endeavour to eliminate this possible error by testing "long surfaces at high speeds" would be of a large order, and the accuracy of the results in their application to the curved surfaces of wave-making hulls would in the end be subject to doubt. It therefore seems advisable to be satisfied with Froude's results which, after all, give a not unsatisfactory measure of the relative skin friction of model and ship. Fig. 2 shows screw h.p. for ship deduced from model results compared with the shaft h.p. as measured by torsion-meters on the ship, Froude's original surface-friction results having been used throughout.

With regard to the screw propeller, there is an entire absence of knowledge of the relative surface frictions of the blades of model and ship, but the error involved by neglecting this factor when both ship and model screw blades are of smooth finish, appears to be of a small order as judged by comparative results. A far more serious error is involved in many cases due to the greater pressure per unit area and greater speed of slip of the ship's propeller than of the model's, while the atmospheric pressure is the same in both cases. The area varies as $(\text{scale})^2$ while the thrust varies as $(\text{scale})^3$, therefore the pressure per unit area increases as the scale. The effect of this would not be disturbing did the "head" on the particles of water in the stream also increase as scale; but this is not so. The atmospheric pressure is the same for the model as for the ship, and in consequence, and in general terms, as the pressure per square inch beyond a certain limit increases, so do also the r.p.m. and shaft h.p. of the ship increase in relation to the r.p.m. and screw h.p. deduced from the model. This discrepancy is sometimes of a large order with surface-breaking

propellers, as also even with well-immersed screws when the speed of slip is sufficiently high in relation to the required thrust to cause a departure from homogeneity in the water entering and leaving the propeller. This break appears to be due to air, water vapour and perhaps other gases, occluded from the water owing to reduction of pressure (see "Note on Results of Experimental Research regarding Cavitation as affecting Propulsive Efficiency" contributed to the shipbuilding section of the 1921 Engineering Conference at the Institution of Civil Engineers, London). Mr. Mumford

An interesting example of the variation of the ratio of screw h.p. (deduced from model screws) to the actual shaft h.p. obtained in 1904 by means of torsion-meters on the ships' shafts is given by Fig. 2. It will be seen that at the lowest speed and power the screw h.p. from the model screws is about 5 per cent. greater than the actual shaft h.p. of the ship, and that the percentage difference becomes gradually less up to about 21½ knots, when the variation increases more rapidly and the curves cross at about 22 knots. At the maximum speed of 22·3 knots the shaft h.p. from the model screws is about 9 per cent. less than the shaft h.p. of the ship. The propulsive efficiency of the model screws is nearly constant over the whole range of speeds, while that of the ships' screws falls rapidly at the higher speeds. In this particular case the tips of the propellers were immersed 5 feet 3¼ inches in still water, the immersion being increased by fall of stern and rise of wave when at speed. The pressure per square inch of disc of the ships' screws rose from four pounds at 16 knots to ten pounds at the maximum speed, and the speed of the slip related to the pitch of the "driving face" (assuming the same percentage speed of wake for ship as for model) rose from 10 feet per second at 16 knots to well over 16 feet per second at the maximum speed. The change in the ratio of the powers was confirmed by corresponding change in the ratio of the revolutions per minute, and it seems possible that what the Author says is "most likely to show itself as a slight reduction of pitch in ship, or an actual variation of thrust at given slip" may, even in the case of well-immersed screws, be attributed to gases occluded from the surrounding water necessitating the higher r.p.m. in order to get the required M.V. (*i.e.*, Mass times Velocity, which equals Thrust). In the case of screws insufficiently immersed to prevent air-drawing from the surface, the discordance between ship-screw and model-screw results is equally serious, even when the thrust per unit area and speed of slip are not excessive for immersed screws. In the case of paddle steamers, for example, the ratio of i.h.p .to shaft h.p. deduced from model paddle wheels appears from numerous records to be of the order of 1·4. Making an adequate allowance for engine friction, the shaft h.p. of the ship must be about 10 per cent. higher than the shaft h.p. deduced from the model paddle wheels.

With reference to wake conditions, there is also dissimilarity. The frictional wake of the ship must be greater than that of the model

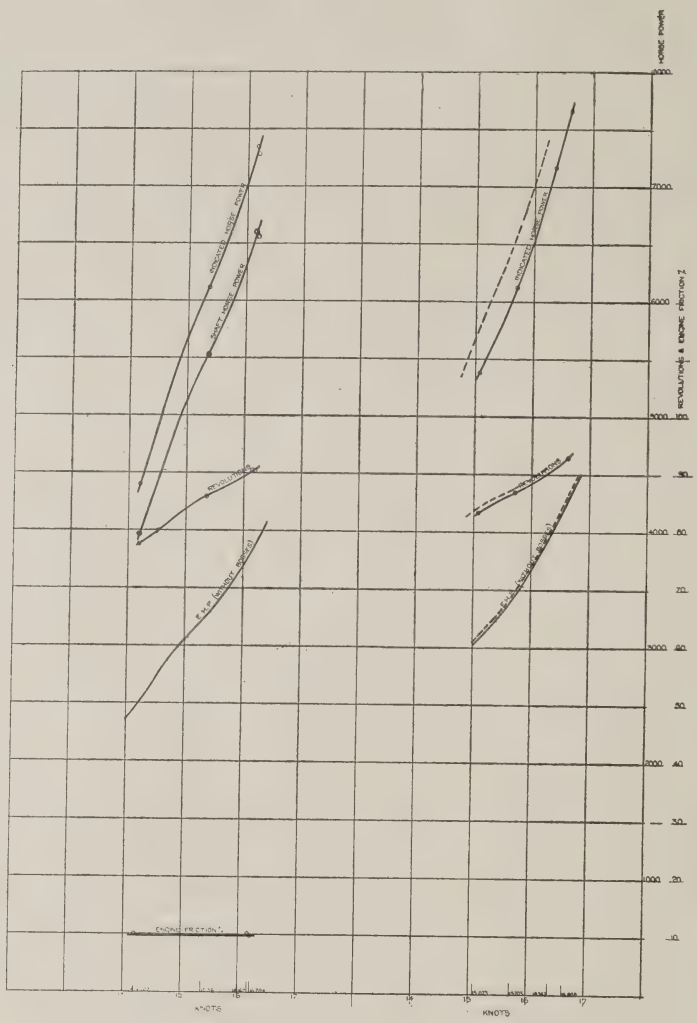

FIG. 3.

FIG. 3.—RESULTS OF TRIALS OF TWIN S. SHIP 450′ LONG ON M.M. SKELMORLIE IN 1912. RECIPROCATING STEAM ENGINES.

I. Weather :—Dull, Hazy. Sea :—Smooth. Wind :—S.S.W. 12 knots.

Bottom Surface :— { Clean, Undocked 28th Oct., 1912.
7 Days Fouling.

Mean Mld. Drt. = 21·5′.

II. The shaft H.P. was measured by means of D. & E. averaging torsionmeters

Weather :—Fine. Sea :—Smooth. Wind :—S.W. 10 knots.

Bottom Surface :— { Clean, Undocked 28th Oct., 1912.
9 Days Fouling.

Mean Mld. Drt. = 21·375′.

This curve of I.H.P. for practically the same Drt. Displt. & Trim as two days before, is 6½% lower apparently owing to a change in the condition of the newly painted bottom surface.

Dotted curves show the results obtained two days previously at a moulded draught of 21·5′.

because of its greater length; but the error involved in assuming the same percentage in each case does not appear to be serious. Mr. Mumford

The condition of the bottom surface of a ship is a very important consideration, not only on account of actual fouling due to slime or marine growth, but also on account of occasional improper treatment of a newly painted bottom surface. It has frequently been found that if a vessel be undocked before her newly painted bottom surface has become dry and hard, a low propulsive efficiency is obtained. Sometimes the condition of the paint surface is so soft that the scour of the water tears it in such manner as to increase considerably the resistance of the hull. Further scouring may remove some of the roughened paint surface, when the resistance becomes correspondingly reduced. An interesting example is given on the accompanying Fig. 3, which shows how the i.h.p. of a clean-painted ship was reduced $6\frac{1}{2}$ per cent. (equivalent to a gain of $\frac{1}{4}$ of a knot) in two days. Considerations of time and cost have frequently compelled premature undocking in our damp climate; but the penalty should be better known, as it is inevitable.

Temperature of the water is another factor that affects comparative results to a small extent. At Dumbarton all results are corrected to a temperature of 51° Fahr., which is the average sea temperature at the Skelmorlie mile. One pair of runs gives a sufficient degree of accuracy where there is no rapid change in speed of current as at Skelmorlie, but there are several measured distances, mainly in the estuaries of large rivers, where the rate of change of speed of current is of so large an order as to involve an error of about $\frac{1}{2}$ a knot if no more than one run each way be taken. In such cases there should not be less than four single runs, and the mean of means obtained from them.

Skelmorlie is also well situated with regard to weather conditions; but the direct effect of strong wind on the above-water structures may be very considerable in many cases, for the loss against the wind is greater than the gain with the wind, because pressure varies as $(\text{speed})^2$. There is in some cases an indirect loss from the wind when it causes the ship to carry a large degree of helm, in order to maintain the desired course. It is therefore of great importance to note the speed and direction of the wind, and make suitable allowance for its effect.

With regard to the mechanical efficiency of reciprocating steam engines, Fig. 2, shows the relation of i.h.p. of a 40 feet twin screw steamer to the shaft h.p., obtained by means of averaging torsion-meters fitted to the shafts. The results show that the power absorbed by the engine friction is about 10 per cent. of the i.h.p. In this case the auxiliaries were driven by separate engines. In another case where the auxiliaries were driven off the shaft of a 360 feet single screw steamer the power absorbed by engine friction and auxiliaries at the running speed of $11\frac{1}{2}$ knots was about 15 per cent. of the i.h.p.

Mr. Mumford

Towards the end of § 6 the Author says the "effective cannot be less than the face pitch (unless the driving face is cut away at the trailing edge . . .)." Cutting away of the trailing edge may be looked upon as an alteration of the nominal face pitch; but it has been found that models of large pitch ratio which have no such modification of the uniformity of face pitch have a less effective pitch at no thrust than the pitch of the driving face. It has not yet been found possible

MODEL SCREW PROPELLERS.

Curve showing slip percentage of face pitch at no thrust in open water, and also the percentage difference between face pitch and effective pitch at no thrust.

The curve is plotted on a base of pitch ratio, and is deduced from experiments with propellers of ·738 ft. diameter each having two elliptically shaped blades having a ratio of projected area to disc of ·164.

The propellers were run at speeds of 200, 300 and 500 ft. per min. and the curve gives the results obtained at 300 ft. per min. The curve for 200 ft. per min. is a little higher, and for 500 ft. per min. a little lower.

Screws of 1·35 pitch ratio, having same diameter but with three such blades as in the above thus increasing the ratio of projected area to disc. from ·164 to ·346, gave results in agreement with the curve.

Doubling the area of each blade lowers the curve about three percentages.

Reducing the area of each blade by one third raises the curve about three percentages.

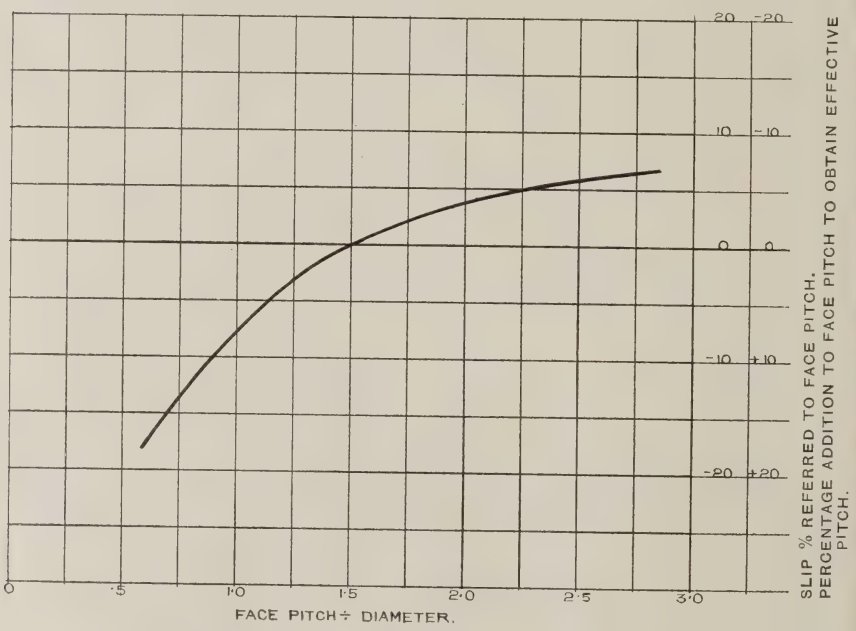

FIG. 4.

to measure the effective pitch when thrusting because that would necessitate measuring the speed imparted to the water acted on, and any instrument designed to accomplish this would cause a change by its interference. It is highly improbable, however, that the effective pitch of any propeller is constant for all thrusts and slips, and therefore it seems that even if it were the case that the effective pitch at no thrust $\left(i.e., \text{ pitch} = \frac{\text{speed}}{\text{revs. at no thrust}}\right)$ could never be less than the

face pitch that the conclusion that the effective cannot be less than the face pitch when thrusting is still not warranted. It is, however, true that for screws of very fine pitch ratio the effective pitch is much greater than the pitch of the driving fact at no thrust, but in the case of screws of large pitch ratio the effective pitch at no thrust is less than the face pitch. (See Fig. 4.) Mr. Mumford

As regards the effect of limited depths of water the published data are not enough to cover the varied conditions with a sufficient degree of accuracy, so that it has been the practice at Dumbarton for over 30 years to use an adjustable false bottom for ascertaining the effect of shoal water. It has also been the occasional practice to investigate the effect of waves by creating waves of desired length by a special simple device.

This contribution to the discussion has dealt at considerable length with what the Author calls "Causes of errors," in order to direct attention to some of the more important causes of discrepancies between model and ship results, and to the relative unimportance of others. When every care is taken to apply model results to ship on the basis of Froude's results, the discrepancy may be covered by a "Coefficient of Comparison" which in most cases is not of a very large order for well-immersed screws. Any endeavour to correct Froude's results, even if successful, would only have the effect of slightly modifying the "Coefficient of Comparison" without affecting the ultimate result.

For ordinary sea conditions an allowance is made which is based on accumulated experience. There is, however, a need for more reliable data than can usually be obtained as to speed and power for various service conditions, and it is hoped that the Author will succeed in his endeavour to secure reports which will enable him to extend the comparison work he describes, and eventually prove its practical usefulness.

Mr. Robertson

Mr. A. J. C. ROBERTSON (New York): Mr. Baker has been particularly generous in this paper in placing before us so many of his failures as well as his achievements in the analysis of ship propulsion. There are, as he indicates, such a number of real difficulties in the striking of a proper power balance sheet that only a stout heart will face them, and frank recognition of these difficulties is, of course, the first step towards overcoming them.

The power put into an engine at sea is almost impossible to ascertain. Indicator diagrams are certainly misleading, and need hardly be considered, and few people have much faith in torsion-meter readings in anything but smooth water. The introduction of oil fuel under the boilers has somewhat simplified estimating, because a much steadier steam pressure can be maintained than with the drops due to removing ashes from coal furnaces; and as a consequence the ratio (power at sea : power on trial) is considerably improved. This improvement is even more marked when we consider Diesel engines, which normally deliver

Mr. Robertson

the same power day after day, and also generally have had exhaustive brake tests in the shops. To the oil engine we may therefore look for a good deal of light on the propeller problem.

In the meantime, probably as good a way as any of estimating power is by standardizing the propellers on the measured mile. At the same true slip and revolutions these will deliver the same power at equal ship's draft, and a power correction may be calculated for other drafts, an approximate expression being $HP = \frac{R^3 \times Dr^{\cdot 40}}{\text{Constant}}$. Unfortunately, we are still very short of useful model propeller data. In the analysis of sea performance we require to deal with slip ratios often much higher than those experimented upon by, for example, Admiral Taylor, and with motor vessels we frequently require pitch ratios lower than Froude's; and for many problems, larger surfaces than either of these experimenters have given us. Then, also, Froude requires us to guess the effective propeller pitch, and I believe this introduces serious errors in many analyses based on his propellers. The effective pitch of all Froude's propellers and practically all of Taylor's propellers is much more than 2 per cent. in excess of the face pitch, and simply to guess this figure is, to put it mildly, not strictly accurate.

As a result of assuming effective pitch for Froude's propellers, the results of analysis of wake and thrust deduction are quite different from the figures arrived at by Taylor's data. In general, Froude's method tends to give considerably lower rotative efficiencies than Taylor's.

Then in any proper propeller analysis the blade thickness and boss diameter must be considered, and further information is required to cover both these features.

The question of how much should be charged to the account of "relative rotative efficiency" is raised by Mr. Baker. Some are in the habit of including all losses of power due to shaft friction, windage, augment of resistance from foul bottom, or ordinary conditions at sea due to roughness of water, in the term. This is in line with the practice of referring resistances to that of the naked model, and where each investigator guesses the effect of tail-shaft friction, windage, etc., differently, it is probably better to do this. Actually, when these are properly allowed for, the rotative efficiency is apparently very near unity.

In the hope of throwing a little more light on the whole subject, I give herewith an analysis of the model tested with its own propeller, three trial trips with different propellers, and the mean results for each propeller in service, in each case of over 100,000 miles, of the American troopships, 18 of which were built and converted to passenger ships. E.H.P. curves were all calculated at Washington on their frictional constants. To render all figures comparable, I have used Froude's propeller data throughout, also Froude's wake fraction in place of the wake percentage used on this side of the Atlantic.

All the trials were run on the Delaware Capes Course, where the water was only six drafts in depth, and the speed in all cases is 17 knots or thereabouts, the draft being about 24 feet. Mr. Robertson

535′ PASSENGER SHIPS.—"PRESIDENT" CLASS.

Ship.	Propeller Diameter.	Face Pitch.	Wake Fraction.	Thrust Deduction.	Hull Efficiency.	Propeller Diagram Efficiency.	Relative Rotative Efficiency.	Quasi-Propulsive Coefficient.
Model ...	17·0′	18·5′	·177	·168	·98	·699	1·0	·685
XA ...	17·0′	18·55′	·153	·190	·933	·698	·85	·554
XB ...	17·0′	18·55′	·157	·203	·922	·695	·85	·545
YA ...	16·5′	16·17′	·241	·212	·978	·680	·85	·565
YB ...	16·5′	16·17′	·249	·225	·968	·676	·85	·556
ZA ...	16·5′	17·0′	·249	·183	1·021	·675	·85	·586
ZB ...	16·5′	17·0′	·243	·187	1·011	·675	·85	·578
ZC ...	16·5′	17·0′	·269	·231	·975	·668	·90	·586
ZD ...	16·5′	17·0′	·265	·240	·961	·668	·90	·578

"A" trial trip, 85 per cent. rotative efficiency assumed.

"B" average sea service 85 per cent. rotative efficiency assumed.

"C" trial trip, 90 per cent. rotative efficiency assumed.

"D" average sea service 90 per cent. rotative efficiency assumed.

X and Y have built-up propellers; Z has solid propellers with faces machined.

No allowance made for tail-shaft friction or windage.

It will be noticed from these figures that at sea and on trial the hull efficiency is distinctly better for the smaller propellers and the higher rotative speed therewith associated actually gives better over-all efficiency. It will also be seen that the solid propellers are apparently distinctly more efficient than the sectional ones, and for this reason it seems reasonable to assume higher rotative efficiency.

The total shaft horse-power of one of these ships may be summarized as follows, on the basis of a total of 10,000 s.h.p. :—

Frictional horse-power, 4,030.
Residuary horse-power, 1,020.
Bossing horse-power, 440.
Bilge keels horse-power, 380.
Propeller losses horse-power, 3,320.
Thrust deduction, 150.
Rotative losses horse-power, 670.

the last item covering tail-shaft friction, weather, etc. The model was tested separately, naked, and with above appendages, frictional h.p. for these being included in the separate items.

Prof. Bragg

PROF. EDWARD M. BRAGG, *University of Michigan*: A large part of the difficulty experienced in reconciling the results obtained from ships and models is due, I believe, to errors involved in the following assumptions:—

(*a*) That the efficiency of propulsion increases directly with the wake factor;

(*b*) That the slip-thrust-efficiency characteristics of a propeller behind a model are the same as in the "open" test.

(*c*) That the pitch of a propeller behind a model is the same as in the "open" test.

The first assumption involves the theory of "wake gain." Some claim that a ship dragging a wake whose velocity is three knots per hour receives as much aid from the wake as it would from a river current of three knots per hour. One has only to consider what happens when the engine stops to see that the conditions are quite different. When the engine stops in the river boat, the vessel is carried along by the current at approximately three knots per hour. The force of gravity contributes a certain amount of energy in addition to that generated in the engine room. In the case of a ship with a wake which the ship itself has created there is no such contribution from outside sources; all of the energy comes from the engine room, and when the engine stops both the ship and wake come to rest.

The statement has been made that the following wake assists in pushing the ship ahead, using the propeller as the intermediary. The wake is present whether the ship is propelling itself or is being towed by another ship. The wake must be delivering its thrust equally in the two cases. If the wake of a towed ship delivers a thrust to the propeller and the latter is free to turn, the pressure on the face of the propeller would cause it to turn in the *backing* direction. I am of the impression that under such conditions the propeller idles in the ahead direction.

The theory of "wake-gain" does not receive much support from the small amount of data which I have been able to gather from published tests of self-propelled models.

In Table V. is given some of the data from Prof. Peabody's paper* of 1911 relative to experiments on the self-propelled model "Froude." This model was 37·6 feet long with a propeller 2·2 feet in diameter, the hub being 5·74 inches long. The data in Table V. is for a speed of model of 6 knots.

It will be seen from Table V. that as the position of the propeller was changed the thrust-deduction factor alone, upon the whole, gave a better indication of the probable power that would be required than did the hull efficiency factor. (The wake values give the velocity of the wake relative to the speed of the ship over the ground.)

* *Transactions* of the Society of Naval Architects and Marine Engineers, Vol. 19.

Prof. Bragg

TABLE V.

Distance of propeller from sternpost ...	1″	3″	6″	18″	30″
Thrust Deduction t	·37	·36	·18	·099	·040
Wake fraction w	·068	·123	·099	·061	·059
Hull efficiency $\frac{1-t}{1-w}$	·68	·73	·92	·96	1·02
Shaft horse-power...	4·24	4·24	3·85	3·94	3·85
Ratio of shaft horse-powers	1·00	1·00	·905	·928	·905
Ratio of $\frac{1}{1-t}$	1·00	·985	·77	·70	·655
Ratio of S.H.P. if power varies inversely as hull efficiency	1·00	·930	·739	·709	·667

In Table VI. is given some of the data which Admiral Taylor added in reply to the discussion of his 1922 paper* on Propeller Position and Propulsive Efficiency. This model was 20·32 feet long, and the propeller was 10·8 inches in diameter. All results are for a model speed of 2·6 knots.

TABLE VI.

Propeller tips below surface ...	6·8″	6·8″	6·8″	0·7″	0·7″	0·7″
Propeller centre abaft sternpost	1·31″	4·06″	7·05″	1·31″	4·06″	7·05″
Thrust deduction t	·27	·26	·19	·37	·30	21
Wake fraction w...	·22	·21	·15	·43	·38	·33
Hull efficiency $\frac{1-t}{1-w}$ ...	·94	·94	·96	1·12	1·12	1·17
Ratio of E.H.P. to propeller horse-power	·58	·60	·64	·60	·65	·70
Propeller horse-power if E.H.P. =1	1·725	1·67	1·56	1·67	1·54	1·43
Ratio of propeller horse-powers	1·00	·967	·905	1·00	·922	·855
Ratio of $\frac{1}{1-t}$	1·00	·985	·898	1·00	·904	·797
Ratio of propeller horse-power if power varies as hull efficiency	1·00	1 00	·979	1·00	1·00	·957

Here, again, it will be seen that in both positions of the propeller shaft the thrust-deduction factor alone gave a better indication of the probable power that would be required than did the hull-efficiency factor which involved the wake. The proper place to take the wake into account is in its effect upon propeller efficiency. The conditions in the wake may be such that a given thrust is developed with less wasted energy than in the "open" condition.

Let us take a simple case to illustrate what I have in mind. The thrust of the propeller is a function of the mass of water acted upon and the change produced in the velocity of the water. The waste energy is a function of the mass of water acted upon and the square of velocity change. Suppose that a propeller in the "open" advances at the rate of 10 knots per hour and throws back the water with a velocity of 3 knots per hour. The thrust will be measured by the quantity

* *Transactions* of the Society of Naval Architects and Marine Engineers, Vol. 30.

Prof. Bragg

$10 \times 3 = 30$, since the mass of water acted upon will be a function of the rate at which the propeller advances through the water. The waste energy is a function of $10(3^2 - 0^2) = 90$.

If the wake has a forward velocity of 1 knot per hour, the rate of feed to the propeller when working behind a ship will be 9 knots. In order to produce a thrust of 30 the change in velocity will have to be $30 \div 9 = 3{\cdot}33$ knots. Since the water has a forward velocity of 1 knot, it will be necessary for the propeller to throw the water back with a velocity of 2·33 knots. The waste energy will be measured by the quantity $9(2{\cdot}33^2 + 1^2) = 58$.

In the open condition the thrust of 30 was obtained with a waste energy of 90; when working in a wake of 1 knot the waste energy is only 58. Proceeding in this manner for wake values of 0·5, 1·0, 1·5, 2·0, 2·5, 3·0, 3·5, and 4 knots, we find the corresponding waste energy values to be 69·6, 58·0, 53·9, 56·6, 63·8, 74·5, 87·5, and 102·0. The gain due to working in the wake is a maximum at a certain low wake value and entirely disappears, upon these assumptions, at wake values which obtain in the case of ordinary single-screw ships.

A large wake must be detrimental to the efficiency of a propeller since it acts to decrease the rate at which water is fed to the propeller, and virtually causes a reduction in diameter.

There is, of course, nothing rigorous about this demonstration; it simply shows in a general way the place which wake should take in our calculations. The wake water of a ship is virtually just as much a part of the ship's body as the rudder or the shell plating. Due to natural laws, a ship is forced to drag along after it a greater or smaller weight of water. In this sense one can just as well speak of rudder-gain or shell-plating gain as of wake-gain. Incidentally, the propeller has to work in the wake, and the conditions there may be such as to increase or decrease its efficiency as compared with the "open" condition.

All of this affects the correctness of assumption (*b*). The conditions of flow in the wake are far from uniform, especially in the case of full-formed vessels. As I pointed out in my paper upon the wake of models in 1922,* in certain positions of the propeller this lack of uniformity may make for greater efficiency; in others it may decrease the efficiency, as compared with the "open" condition.

There are certain experiments which could be performed which would show whether or not assumption (*b*) is permissible. Test a propeller in the "open" and then behind models of different fullness; the wake to be determined not in the usual manner, but independently by using the propeller as the meter wheel on a current meter. A comparison of the curves of thrust and efficiency obtained in these different conditions ought to show whether assumption (*b*) is permissible.

In my experiments upon the wake of models I found that by shifting the meter from the port side to the starboard side of the model, different wake values would be recorded. There was, of course, no difference in

* *Transactions* of the Society of Naval Architects and Marine Engineers, Vol. 30.

the wake of the model on the two sides, but the different angles at which the propeller faces met the water closing in, and rising up around the stern, caused the virtual pitch of the meter wheel to be different in the two positions. In attempting to reconcile the results of trial-trip results with model propeller results, I have come to the conclusion that in full-formed lake vessels the virtual increase of pitch may amount to 10 per cent. Prof. Bragg

I feel that better results would be obtained in reconciling the results of ships and models if only the thrust deduction factor were used. The unwarranted use of the wake factor makes it necessary to introduce a corrective factor called the "rotative efficiency" whose value in general is such that it counteracts the effect of the wake factor in the expression for hull efficiency. The determination of the wake value from self-propelled models, by carrying over the characteristics of the propeller in the "open" condition to the "behind" condition, makes such a value a sort of catch basin for numerous errors, and may cause it to be considerably in error. I think it preferable to determine such values by a current meter. Full-formed vessels in the light condition may easily have wakes amounting to 40 per cent. or 50 per cent. of the speed of the ship. In the Froude system of wake notation this would be 67 per cent. to 100 per cent.

The characteristics of propellers should be determined in the "behind" condition. At the present time we have not sufficient published data to warrant the assumption that the characteristics will be unchanged under conditions which differ so widely from the "open" condition.

Mr. E. LIDDELL, *Associate Member*: To all those who are interested in the study of merchant-ship design from the point of view of economic efficiency the "other ship-propulsion data" presented in this paper will have been of an importance at least equal to that of the measured-mile data. Mr. Liddell

Mr. Baker in his comment on voyage records and trials in the light condition touches on several points that would seem to be of some importance in the design of colliers and tankers, which regularly perform voyages in ballast, and also in that of cargo liners which may have to be run with their holds only partially filled. In the former the propulsive efficiency in the light condition ought to be studied with the same attention as that in the loaded condition, and in the latter it would be desirable to know the propulsive efficiency over a reasonably wide range of mean draught and trim.

The questions one would like to ask Mr. Baker in this connexion are the following:—

If we reduce the depth of immersion of the propeller, is there a critical point beyond which the Froude method of analysis is no longer applicable, so that up to that point we can confidently analyse data in the manner

Mr. Liddell

described in the Paper, or have we to reckon with gradually increasing discrepancies?

Again, if complete immersion of the propeller is secured by trimming the vessel, may we expect consistent analysis results at all practicable inclinations of the shaft to the horizontal?

A factor that would influence the working economy of a cargo liner is the average propulsive efficiency and Fig. 1 is of considerable interest in this respect. Here, one would like to ask whether the vertical lines correspond to successive stages of the same voyage and whether the intervals are roughly equal, so that mean values of shaft horse-power etc., represent averages for a fine weather voyage; also, whether the diagram may be considered typical.

One is further tempted to ask whether the fluctuations in propulsive efficiency would or would not be minimized if the propeller was for a mean draught and trim intermediate between the arrival and departure condition, or if the stowage was arranged so as to cause only small changes in the draught aft during the voyage.

There are two other points of a more general nature one would like to submit to Mr. Baker.

In the first place, what constitutes "fine weather"? After reading Mr. Kent's paper on the influence of pitching on resistance it is difficult to resist the conclusion that only definite information with regard to the amplitude and period of the lift and 'scend and the immersion of the propeller would enable us to decide whether it is worth while to bring the sea performances of ships into line with the smooth-water trial results of their models. In the case of vessels intended for trades in which the weather on an average can only be called "moderate," would not the study of the behaviour of the ships among waves together with rough-water experiments be of greater commercial value than smooth-water experiments and steam trials on sheltered courses?

The second point refers to the conditions of the bottoms of ships the data of which one wishes to analyse. Mr. Baker states that "where a plausible explanation of discrepancies is possible it has been given." We must therefore presume that the log data quoted all refer to voyages undertaken immediately after docking and painting and the remarks that follow are not intended to apply to these ships.

That fouling causes a considerable increase in frictional resistance is beyond doubt: what is less certain is the answer to such questions as these:—Do paint surfaces retain their frictional coefficient as they get softer, and until fouling begins? Does the increase in frictional depend solely on the area covered with marine growths? One might argue, on the one hand, that fouling would begin where the rubbing velocity of the water particles is greatest, as the surface would begin to deteriorate where the eroding agencies are strongest. In that case the rate of increase of frictional coefficient and wake would be greater in the first stages of fouling than later on. On the other hand, fouling might begin

where the rubbing velocity is least, as the smaller the velocity the better the chance for germs to adhere. In that case the curve of increase of e.h.p. etc., would be convex to a time base. Mr. Liddell

The practical problem on which one would like to ask Mr. Baker's opinion amounts to this: As we can hardly examine the bottom of a ship during the voyage, in what manner can we check any assumptions with regard to fouling that we may make in analysing log data?

In conclusion, the writer, in thanking Mr. Baker for this paper, would like to emphasize the great help he has given to all students of merchant-ship design who have to rely on published data for their materials.

Dr. Ing. FRIEDRICH GEBERS: Mr. Baker's very interesting paper shows us the discrepancies between the results of model experiments and those of the trials of full-size ships and the difficulty of bringing these results into agreement. We cannot transfer the resistance of the model directly to the full-size ship; we must use Froude's ingenious idea of separating the wave and frictional resistances, transferring only the first one, but calculating the other for both. Also, we cannot transfer the propeller results in connexion with the model directly by the law of comparison. Dr. Gebers

Even in making experiments as we do in Vienna, with a propeller driven from dynamometers enclosed directly in the models and using the frictional deduction—if every condition is exactly imitated from the large ship—the propeller must have its effect on the current of water behind the model produced by skin friction. Therefore the speed of water relative to the propeller will be greater for the large ship than for the model if we use the law of comparison. Behind the model the water will get a proportionally greater velocity, and therefore we will have lower revolutions and a greater torque and we are not in accordance with the law of comparison.

It is very unfortunate that we do not know the exact amount of the frictional deduction and its influence on the water motion, since we have not an exact knowledge of the frictional resistance of ship-formed-solids and the wake behind them.

Assuming Froude's data, the frictional deduction may be expressed by the following formula:—

$$R_a = \gamma f v^{1\,825}\left(\lambda_m - \frac{\lambda_s}{a^{0\cdot 0375}}\right)$$

γ = the specific gravity of the tank water = 1.

f = the wetted skin in m^2.

r = the speed in m/sec.

λ_m = the frictional coefficient for the model, reckoned from R. E. Froude's data, in kg/m^2 dependent upon the length and the temperature.

Dr. Gebers

λ_s = the frictional coefficient for the ship, reckoned from R. E. Froude's data, in kg/m^2 dependent upon the length and the temperature.

$\frac{1}{\alpha}$ = the scale of the model.

This formula also shows us that the frictional deduction, in neglecting the temperature, is dependent upon—

(1) The scale of the model.

(2) The absolute length of the model.

(3) The absolute length of the ship.

If we assume that this frictional deduction is the most important factor in producing, through its influence upon the motion of water behind the model, the discrepancies between the results of model trials and ship trials, perhaps we have a future foundation for relating these discrepancies; in any case it is very desirable for the estimation of the extent of the discrepancies to know the absolute length and the scale of the model.

The difficulties of the subject of Mr. Baker's paper are very large. Mr. Baker has pointed to the discrepancies in the data of the ship trials: I think that the data of the model trials often are also very dubious. At the beginning of my experiments I was working with propeller dynamometers constructed very similarly to those of Froude, the propellers being run behind the model on dynamometers reaching into the water. Soon, however, it was realized that, to fulfil the law of comparison for similar propellers of different size, it was necessary to wrap the supports of the dynamometer in order that their influence on the propeller race might be truly similar. The difficulties involved in following the trimming of the model, supporting the propellers at the right place, fitting the rudder on single-screw ships, the propeller shaft leading behind through the water, but especially the knowledge that the wraps had an influence upon the resistance, the latter being diminished in many cases, induced me to put the dynamometers into the models themselves, and since 1916 we have made our experiments in this manner with ship and propeller models.

As to the horse-power, the results of such experiments seem to be in a good accordance with those of the ships; the number of revolutions, reckoned by the law of comparison, is too small; the torque, in the same proportion, too large. For instance, if a twin-screw river boat on shallow water of about 4 m. tested on a scale of 1 : 12·5, length of model about 5 m., it is found that the revolutions of the large propellers are 5 per cent. more, the torques 5 per cent. less for a speed of about 12 to 14 km/hour, while the frictional deduction is 13 per cent. of the thrust of model; this is only applicable to similar cases, and it would indeed be very useful if owners would furnish exact data to confirm our experience with the wake after the ship.

The effective pitch of the propeller exceeds, in my opinion, the face pitch more than Mr. Baker has assumed. Dr. Schaffran has found

8 to 20 per cent., the former for propellers with a great pitch and a great area, the latter for propellers with small pitch and small area. I personally found, for a series of similar propellers of different size, about 10 per cent. Dr. Gebers

Mr. Baker's paper is a most important argument for the necessity of gaining a knowledge of the friction of ship-formed-solids and the wake behind them. We are obliged to Mr. Baker for having informed us of the results of his work in this matter and for having drawn our attention to a fact which must surely be of the greatest interest to ship-owners and shipbuilders.

Mr. W. G. A. PERRING, *Graduate*: The task of balancing up model experiments with trial results and ordinary ship-performance results is a difficult one, but nevertheless a very important one. It is alarming that there is so very little reliable data of mercantile ships combined with model ship and propeller experiments in existence, and we are indebted to the firms who have kindly allowed their results to be published and to Mr. Baker for the way he has presented them, making clear the difficulties encountered in the analysis and the need for greater care and accuracy when obtaining the results. The value of such data should be realized by the naval architect, the marine engineer, and the shipowner alike, and all should co-operate in solving the present problem. Mr. Perring

Once more the need for frictional experiments on a large vessel could be emphasized; 60 to 80 per cent. of the resistance of these mercantile vessels is frictional, and it would detract from the value of any reliable data if the frictional resistance of the ship could not be assessed correctly and the true e.h.p. obtained. Mr. Baker has referred to this point in his paper, but what is almost of equal importance is the condition of the ship's bottom at the time of trial—the time out of dock should always be recorded, together with the composition used on the bottom, so that an estimate can be made of the "fouling" effect upon resistance. Of course the better plan is to run trials immediately after undocking, when the condition of the bottom is known with far more certainty. Perhaps part of the discrepancies obtained in boats A, B and C of the Paper are due to the effect of foulness, the true e.h.p.'s being higher for this reason.

In the Appendix Mr. Baker draws attention to the variation of wake over the propeller disc, this variation shows itself in a variation of torque. This is, of course, well known, but it should be borne in mind in obtaining the shaft horse-power by torsion-meter. Messrs. Thorne and Calderwood, in their recent paper on torsional vibration, gave actual value for torque at different relative angles to the shaft. These showed considerable fluctuation, and when measuring shaft horse-power a similar method to the one used by them should be adopted in order that a mean torque may be obtained. It is not unlikely that some of the powers obtained and given in the Paper are high because the

Mr. Perring

torsion-meter has been set up at a favourable angle to the propeller disc.

The adoption of the Michell thrust block will render the measurement of thrust more simple, and will help towards a fuller and better analysis being made; but even then the ship data will not allow of a complete solution. It is only as ship-trial and model-data results are compared along and on similar methods to those indicated in the Paper, that the "scale" and other factors involved will be determined and render design more certain.

Mr. Whiting

Mr. W. R. G. WHITING, M.B.E., *Member*: We are all very much obliged to the Author for placing before us this collection of observational data, together with the steps of the analysis. The Paper is so largely the logical outcome of the data, that the discussion has little to attack, and is rather compelled to set up its own ninepins, for Mr. Baker, somewhat out of turn, to bowl over.

Referring to Ship I, I notice the analysis figure for wake fraction is 0·92. On turning to the original results, which I paid some attention to when they were published, I see the wake *factor* found by Bauer in an extremely convincing fashion from the Schaffran experiments is round about 0·44, corresponding to a Froude coefficient of 0·78. If there is no objection to accepting this figure of 0·78 in place of 0·92, we obtain close agreement between the measured results on Ship I and those derivable from the Froude propeller data. With a uniform allowance of 7 per cent. for shafting losses, the latter data gives 507, 504, 572 s.h.p.'s as the power requisite to drive the propeller as stated, whilst the recorded s.h.p.'s on the three trials were 505, 508 and 568. These figures allow for the metric power unit in which the original results are stated.

Mr. Williams speaks, with an appearance of inevitable resignation, of the divergence between the powers developed by machinery on trial and those that should content a well regulated vessel. As regards shaft horse-power, the measuring gear is commonly installed in the shaft tunnel, where the shaft is subject to both torque and compression, in amounts not strictly proportional throughout the measured range. Thus if the calibration constants of the shaft in pure torsion, as found in the shop, are used, powers consistently too high will be recorded, in accordance with the effect of Poisson's ratio. Another uncertainty must always exist at the outer stern-tube bearing. Whilst no doubt it is frequently, like Cæsar's wife, above suspicion, one has to take such facts on trust. It can equally well fulfil the functions of a drowned friction brake, nor is anyone the wiser.

In passing, I would suggest that as this paper, in common with previous cognate ones, emphasizes the desirability of recording both torque and thrust on trial, thrust meters depending on the compression modulus of the shaft be designed and fitted. This is a very different matter from installing true thrust-weighing mechanism, and if the

measurement of the elastic strain is practicable for torque determination, why not for thrust too? The second point I would make regarding this lack of concordance is that a propeller behind a ship is always and inevitably less efficient than when developing equal thrust in the open. The thrust is maintained solely by imparting every second a numerically equal quantity of linear momentum to the race, and the machinery torque is similarly balanced by the rate of imparting rotational momentum to the race. Now the momentum is the sum of "masses by velocities," *i.e.*, the change of velocity impressed on each constituent of the race. On the other hand the kinetic energy carried to waste in the race varies with the "masses by velocities squared." If the former summation is constant, the latter is a minimum when the constituent changes of velocity are equal. This ideal will be more nearly approached for a screw working in open water than when behind even a fine ship. I have little doubt that the theory of momentum applied concurrently to both linear and rotational races is the foundation on which any fundamental propeller theory must be based. Mr. Whiting

No real advance took place in the knowledge of ship resistance until the frictional and wave components were distinguished, and I believe a great step in the elucidation of such data as Mr. Baker presents will have been taken when the blade frictional losses, substantially constant at a given speed whether thrust is being developed or not, are identified and dealt with as one of the three components of which the complete action is the synthesis.

The suggestion that the time is ripe for a repetition of the "Greyhound" experiments will not, I trust, pass unnoticed. On this occasion these experiments should be on a much larger scale and primarily in the interests of the merchant service. A pair of destroyers towing on a bridle would be able to toy with the heaviest tow that such experiments would call for. As an orthodox running on a mile is an essential difficulty of such towing trials, I would suggest the use of range-finder methods of speed determination, which I find from Messrs. Barr and Stroud's figures can be effected within one per cent. accuracy even in unfavourable circumstances. I am not sure that the tension in the short leg of the tow might not be deduced from observations of the sag, taken either clear of the ship or on a range over the fore part of the towed vessel.

AUTHOR'S REPLY.

With regard to the Chairman's opening remarks on the question of another paper, I think I can promise you in the name of the Advisory Tank Committee that my suggestion will in all probability be fulfilled and that we shall be able to offer you a second paper when we have something a little more accurate than the data available up to the present time. I want you to understand that the object up to the The Author

The Author

present has been this: One hears it said quite often that your model experiments are not borne out by what a ship does; sometimes the doubt is put in the form of a question. Here I have taken the available data at present, which must form the basis of these remarks, done the best I could with it, dealt with it justly and without bias as far as I could, and seen how far ship and model do or do not agree and stated what the discrepancies are in the paper. In replying I do not profess always to clear up the point raised, but have given the best answer possible with our present knowledge and in some cases referred to more detail sources of information.

I have to thank the Chairman for his very kind reference to my work. It is a pleasure for me to come amongst you and to feel that the Tank work is appreciated. In replying to the discussion I propose to take each contribution separately, but it may be pointed out how continually reference is made to the need for accuracy in trial data and of research on a few items such as full-scale friction work, and screw-propeller efficiency. I sincerely hope that these two serious problems will be tackled in the near future and that this nation will retain its proper place amongst the nations as the premier nation in connexion with *all* aspects of mercantile work. The sum of money involved in such research is small, but the amount spent on it will have to be greater than it has been, or others will pass us through their greater efforts.

Reply to Mr. Williams.—The salient point in Mr. Williams's contribution is the apparent discrepancy between the mixture of two reliable quantities (hull and screw data) when full size and of model size. I use the term "apparent" because it is recognized that the differences are not discrepancies but are produced by variation in initial conditions—the variation being unknown. It is difficult to believe that there is any very large loss in transmission between engine and propeller unless it occurs in the external bearing. The heat set up would surely draw attention to the matter. In our analysis some 2 per cent. for shaft losses and a similar amount for block losses have been taken, unless the Michel block is fitted, when its loss is almost negligible. Taking the example he gives of the electrically propelled ships, the assumption that the power required to run the propeller and its shaft with the ship at rest is wholly used up as a difference between b.h.p. and d.h.p. when propelling the ship is open to serious doubt. The rate at which the propeller was doing work when the ship was either moving or in dock depended on the intake velocity or the velocity of the water at the screw disc. This was known more or less accurately when the ship was propelled but was altogether unknown when in dock. The current through the propeller, set up by its own rotation would be considerable, and the energy used up in this way would account for a considerable proportion of the brake horse-power developed.

Reply to Mr. Alexander.—Experiments have been made in the Tank with propellers running through waves, but only in open water. The

waves had only a small effect on the thrust and efficiency provided that no break-down occurred in the actual flow of the water to the propeller nor any churning of the water took place, *i.e.*, the circulation of air right round the propeller disc. At slips above 40 per cent. this is liable to occur in the model as the propeller passes through the hollow of a wave, and in all probability would do so a little earlier on the ship. Fuller information on this is given in the Institution of Naval Architects paper to which reference has already been made. The question of wave resistance must be left to Mr. Kent who has this matter in hand, and I do not want to take his thunder from him. He will be reading a paper during the next twelve months, on his measurements and observations on ships in rough weather. The Author

The thrusts or rather $\frac{T}{V^2}$ behind the model are used for wake determination because they can be obtained with greater accuracy than the torques. The torques can be used for the same purpose and wakes so found are not seriously different from those derived from thrust analysis (see Capt. McEntee's remarks which confirm this statement). Generally they lie within about two per cent. of each other, and quite often are the same. A large difference in such wakes would indicate something seriously wrong in the basis of the analysis. Correct wake velocity is required to determine the correct propeller pitch—unless the designer is working from a ship already tried. It should be understood that the model data for a given screw behind a hull is sufficient to determine whether it is of the right general dimensions, and to give the propulsive efficiency with the screw behind the ship. It is then not a serious matter to compare the results so obtained, with standard propeller data for the actual propeller and for any reasonable variation, and thus to obtain the effect of variation in dimensions of propeller on its efficiency and therefore on the whole efficiency.

As regards relative rotative efficiency, plausible arguments can be given why it should not be seriously above unity but no positive facts. The curve of efficiency on a base of slip is usually convex upwards. If the uniform average slip of a propeller is say, 30 per cent., and through the presence of a hull in front of the propeller, the slip varies regularly from 20 to 40 per cent. its mean efficiency will always be lower because of this convexity of the efficiency curve. Even if the greater thrust is obtained with the greater efficiency, the average gain in efficiency is not likely to be very large. Although we obtain relative rotative efficiencies above unity and Froude also obtained such, it must be remembered that these are dependent on the accurate determination of wake, which is a difficult matter, and that any error in wake is repeated in the rotative efficiency. In the paper read before the Institution of Naval Architects this spring by Mr. Wigley and the Author, reasons are given for discounting to some extent the high wakes obtained in model experiments. In that paper it is also shown that a little cross flow will give an apparent wake of several per cent. which is not real wake from an

The Author

efficiency point of view, and to whatever extent this is present in the model, it makes the wake and therefore hull efficiency appear high and rotative efficiency low. At present the wake fractions can only be regarded as approximate, and we hope to establish them more clearly by the full-scale work referred to by Mr. Alexander. Mr. Whiting's remarks on this subject should also be read.

Reply to Mr. Hinchcliffe.—In his opening remarks Mr. Hinchcliffe has stated the trouble which we are trying to clear up, with a little more detail than was done in the Paper. Disappointment in results necessarily suggests mistakes or incorrect assumptions, and should be cleared up. I think, too, that Mr. Hinchcliffe means something which was also suggested by Mr. Telfer, namely, that to start with inaccurate data with a view to producing accurate comparisons was a hopeless task. That could not be helped; the data had to be examined to see how accurate they were and the results are in the Paper.

His remarks on the usefulness of a carefully conducted series of towing trials with a full-sized vessel are fully concurred in. Such work I suggested to this Institution in 1915, and we shall never be able to answer with any certainty Mr. Hinchcliffe's questions *re* skin friction data, until such work is done. With regard to model screw propellers, we have no reason at present to doubt the applicability of Froude's or Taylor's data to actual ships when the conditions are the same as in the model experiments. When these conditions are varied, the results are also varied and again reference might be made to the Institution of Naval Architects paper already mentioned. Again as regards the difference between an ordinary cast-iron and a polished bronze propeller reference should be made to McEntee's paper published some seven years ago. He found that a rough cast surface had a material effect upon the results (see Admiral Taylor's contribution) but he did not point out—and it should be pointed out—that the roughness on his small cast propeller represented very considerable roughness when his model (including the surface variation) is enlarged to give a 16 feet propeller—much more than any reliable firm would accept, and my own opinion is that the little *smooth lumpy* inequalities in the surface of a cast-iron blade have no effect at all on the propeller, but if these are *rough or have sharp edges* they may have a marked effect as shown by McEntee.

Reply to Mr. Ainslie.—I think that the knowledge of how trials should be conducted is fairly well known, and inaccuracies such as that suggested, namely, six inches on the diameter of screw, need not occur if care is taken. But to some extent the actual trials are regulated by a contract which has not been drawn with a full knowledge of the proper way to test the ship's propulsive efficiency, and the taking of the requisite data with the care necessary for good analysis, means some slight additional expense which is sheer waste unless the analysis is carried out, and this is not often done. There are very few ship owners who take a real interest in this side of the question in running ships and it is hoped that they will

be added to the " more people who take an interest " mentioned by Mr. Ainslie. The Author

Reply to Mr. Telfer.—Mr. Telfer's remarks on inaccurate data I have dealt with in replying to Mr. Hinchcliffe. With regard to Geber's work, the extension of model data to higher VL values is all to the good, but it hardly touches the problem of the use of these data for full-sized ships at full speed. It does not take us *one-tenth* the way across the gap that lies between the two, and I cannot see that this is a much more satisfactory basis than we have at present.

Mr. Telfer's remarks on scale effect bring in matters that are controversial, which do not arise directly out of the Paper and which I must leave to another time. I am hoping that the experimental work with large screws in the Hamburg Tank will help in this matter; possibly we shall get some data from ship measurements. Mr. Telfer's argument from aerofoil data should be taken with considerable reservation. The Committee appointed to investigate this matter for aeroplanes did not come to any definite conclusion. Full-scale work with air screws has shown no material scale effect at all. Moreover, if the efficiency of a propeller is worked out from its elements as suggested by Mallock (many years before Lanchester enunciated his method) using aerofoil data, the result does not agree with experimental facts. This has been tried here with several propellers, and it was found that many corrections had to be made to bring the estimated into line with the actual efficiency. In passing, it should be said that Lanchester's theory of the screw propeller was severely criticized when his paper was read before the Institution of Automobile Engineers, and I have not heard since that it has been recognized as sound. Mr. Telfer's assumption that you can equate a difference found in comparing model-screw with ship-screw performance, to another difference found in comparing different sets of model-screws data, is not sound, and his conclusion must be equally unsound. So far as our analysis has gone, there is no getting away from the type of pitch correction required to make model and ship screw agree in thrust performance (we have no direct comparison of efficiency). The effective pitch in the ship is a little less than in the model, and the ratio of this effective pitch to the face pitch for a screw of normal make is about 1·02 in ship and 1·04 in model.

Passing now to the mode of determining the wake fraction Mr. Telfer gives an algebraic method of calculating the thrust which should be developed by the model screw, and suggests that when the model screw is developing this thrust behind the model the conditions are such as to represent the ship conditions and give the correct wake. But is this really so? The frictional belt and the wake resulting from it are still those of the model with all their higher accelerations (relative to the ship wake) due to friction, and this is one of the chief causes of wake velocity and cannot be ignored. In so far as wake is dependent on slip, its variation with slip can be better studied from the experiment results in

The Author

any case. The data obtained from the experiments with screw in open water and behind model can be regarded as two curves of $\frac{\text{thrust}}{(\text{velocity})^2} = \frac{T}{V^2}$ which can be plotted to a base of $\frac{\text{velocity}}{\text{pitch} \times \text{revs.}} = \frac{V}{PN}$ (see Fig. 5) using the advance velocity of model as velocity for the "behind" curve. When these two curves are known, the wake at any slip can easily be found, and the effect of variation of resistance in general can be studied. The wake so found usually increases somewhat as slip decreases in single-screw ships as suggested by Mr. Telfer. Some years ago we gave in our

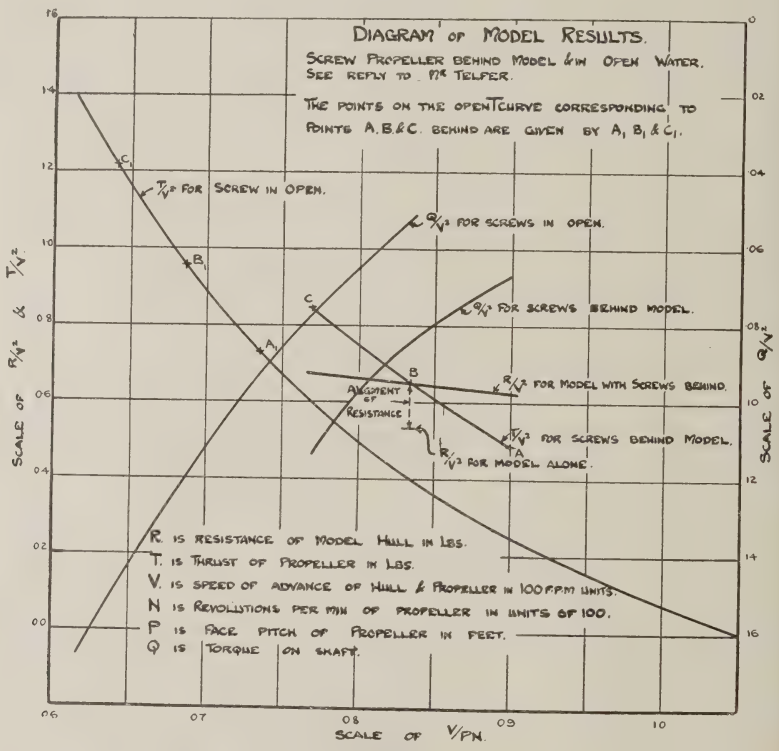

FIG. 5.

reports a wake for ship obtained at lower relative resistance but the procedure was dropped as it was recognized as only taking account of one variation between ship and model and ignoring other and equally important ones. In this paper the ship wakes are given as found necessary *for the ship*, regardless of what was found on the model, and they usually came a little less than for model in single-screw ships, and if the initial data is assumed correct this conclusion cannot be put on one side by any academic argument relative to just one aspect of the problem.

With regard to Bauer's work, my only hope is that we shall be able to proceed along the same general lines, and that we shall receive the same

encouraging support in England as he does in Germany. Like all other experimenters, he sees defects in his own work; but one can only admire it and learn from it how to do better when opportunity occurs. The Author

Reply to Mr. Calderwood.—Mr. Calderwood points out two difficulties in obtaining engine data on trials. One must admit these, but they are not insurmountable. In a recent case we obtained torque readings at five propeller positions during a single run on the measured mile, and by making a little longer run, more could be obtained. It is not so much power as torque that we require to compare with the torque obtained in the model screws, and I must confess a predilection for direct measurement of any data required, if it can be done, rather than making assumptions as regards the interpretation of a mean effective pressure diagram and the mechanical efficiency of the engine.

With regard to wind resistance, I am hoping that Mr. Kent will be able to supply data on this subject. There is a certain amount of data obtained for models in an air tunnel published by McEntee, and by the Advisory Council for Aeronautics, but its application to ships is open to serious doubt owing to the small size of all the features on the model. As regards fouling, some reliable data on this subject are given in my I.N.A. paper of 1916 and need not be summarized here. Mr. Mumford in his contribution to this discussion gives a very good example of a type of "fouling" sometimes overlooked by builders.

I am not in a position to give any information as regards the "contra-propeller." It is of course an old idea in a new disguise. Nor do I think it wise to express opinions on particular formulæ for wake fractions. The chief objection to all such formulæ is that they are usually based on certain ship designs and hold with fair accuracy for all similar ones, but when used for others they are liable to be very misleading unless the differences in clearance, fore-and-aft position, etc., are properly taken into account, and this is not always easily done.

Reply to Mr. McAlister.—I am glad to hear that he has been able to correlate his trial data and model data so well. To some extent he avoided trouble by confining his analysis to ships fully loaded, and of course he had not the difficulty of making his wake agree with a *definite figure obtained in a model,* but was satisfied by it falling into line with other ship results, a rather easier matter. The subject of the falling off in thrust with emersion of the screw, is dealt with in the paper recently read by Mr. Wigley and myself before the Institution of Naval Architects. The 6 per cent. to which Mr. McAlister refers was taken from our experimental curves for the immersion of the trial screw—much larger reductions are experienced in some cases and this forms one of my reasons for suggesting that ballast trials are not good tests of ship and engine.

Reply to Mr. W. R. G. Whiting.—In the analysis of Ship I, several different assumptions were made and results not given in the Paper were worked out. It was not possible to depart very much from the 0·92 wake fraction with any reasonable assumption, and although one recognizes

The Author

that a lower wake would square up matters, our data did not support it, and we could not adopt it.

I have already dealt with the question of high relative rotative efficiency, and am glad to see that Mr. Whiting concurs in our proposal to measure thrust and torque in order to clear up some of these matters, and to make new experiments on skin friction to settle this very knotty point.

Reply to Mr. Perring.—Mr. Perring suggests that the discrepancies in Ships A, B and C, Table I., might to some extent possibly be due to foul bottom. I have dealt with this subject in answering Admiral Taylor who also suggests fouling as a possible cause of discrepancy. In these particular ships close analysis is useless as the data are too inaccurate to be worth it.

Reply to Mr. Mumford.—There are two distinct points of view in the scientific analysis of measured-mile-trial data. Mr. Mumford has taken one of these, and quite correctly points out that Froude's skin-friction data as usually extended to ships, are a quite satisfactory basis for *comparing ships.* But from the experimenter's point of view this is not sufficient, and to estimate ship powers without the use of comparative ship results, we depend on this skin friction data, and have no direct support for it. One admits the cost of full-scale friction work, but quite suitable vessels for this purpose (viz., the later form of destroyers) are now rusting in our harbours, and with some assistance in towing the cost is not insuperable.

With regard to the pressure loading on the screw blades and the possibility of a break-down in thrust, due to this being relatively higher in the ship than in the model compared with the atmospheric pressure. This was held in mind in our analysis, and as far as is known did not occur. This feature is dealt with in the Tank paper recently read at the Institution of Naval Architects. The example given by Mr. Mumford is presumably a ship of the same class as Ship H, Table III., and in that case, there was no reason to suspect cavitation. The variation between the screw h.p. (estimated from model screws) and the actual s.h.p. (measured on the shaft) as speed increased was in all probability due to variation in Froude's " B " factor and efficiency, due to proximity of surface and to increase of slip with speed. For the reason given by Mr. Mumford one expects the falling off in B to show earlier on the ship than in the model. The suggestion that this may be the true explanation of the difference between ship and model screws, which so far has been attributed to small change in effective pitch, is new and interesting, and will be borne in mind in future analyses.

I am glad to see that Mr. Mumford concurs with the conclusion that one run each way on a mile is not sufficient in some cases, and it is to be hoped that this practice will cease. With regard to the effect of the wind when on the mile, Ship J Table III. is an interesting example. As mentioned in § 16 there was a cross wind. Steady 10° helm was

required running into the wind, but no helm running with the wind, and the smoke then apparently travelled with the ship. Assuming the air resistance to vary as speed squared, if there were no wind the mean resistance both ways would be proportional to $(13{\cdot}4)^2 = 180$, but with the wind, the mean resistance $= \frac{(26{\cdot}4)^2 + 0}{2} = 350$ *i.e.*, is nearly double and in addition helm of 10° was carried one way on the mile. In this case there was very little or no tide, and the wind caused a difference of some 0·3 to 0·4 knot in speed. The Author

On the subject of effective pitch, in the paragraph referred to (§ 6) the word "trailing" has been substituted for "leading" as this was obviously an error. The extent to which the effective exceeds the face pitch varies in different propellers. Although this could not be measured when working at any slip, it can be found by a comparatively simple analysis of the curve of thrust plotted on a revolutions base. For any portion of the curve covering a small range of slip, the thrust T can be written in terms of the revolutions—

$$\text{N as } T = aN^2 - bN$$

a and b being constants such that $\frac{b}{a}$ is the virtual value of R_0 the revolutions for no thrust at this *range* of slips. Analysis of this kind with screws well immersed show very little variation of R_0 with slip. The only case in which the effective has been noticeably below the face pitch was a propeller cut away rather crudely at its rear edge on the driving face, *i.e.*, there was a definite reason for it. This statement is based on experience with screws having pitch ratio up to about 1·5, possibly those mentioned by Mr. Mumford come above this limit, or may have special features to explain the change he mentions.

Reply to Mr. Liddell.—The answer to the first question must be given with this reservation that we are not quite certain yet. It appears, however, that a ship screw protruding well out of water is not exactly represented by the model screw in the same condition. Provided it is *immersed* one can proceed with suitable variations of Froude's "B" coefficient. Between these two conditions things are uncertain and certainly when air passes round the disc Froude's method of analysis and his screw data do not apply.

Consistent results might be expected if the propeller is immersed by trimming the ship by the stern, but the effective horse-power might be materially affected by running with heavy stern trim, and large helm required for steering, which would add to the resistance. The inclination of the shaft is never very serious and within normal practice limits its effect on ordinary mercantile vessels is not material.

As regards Fig. 1, each vertical line represents a fine day, the time interval between the lines varied considerably, and the average of the diagram, would give the average for fine weather but not for the whole voyage. A diagram of this character could be drawn with one line for

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each day's running and this would give the voyage average, but before doing this, I think it would be necessary to take steps to ensure that the data was worth the labour, as our experience has shown that much of this data is valueless and in some cases consists of figures put in merely to fill the spaces. There is always a little gain in propulsive efficiency in keeping the propeller under water, and a still further gain due to the fact that, in this condition it will absorb more power than when partially out, and the engineer will give it this power, without any material difference in stoking. The drawback as already mentioned if very great trim is used, of course applies here, and a little sea sense is required to settle the best condition.

As regards skin friction, Mr. Mumford has partially answered Mr. Liddell, and my reply to Mr. Calderwood gives further information. If a paint surface softens and remains flat, I should imagine it will not vary in resistance. But if it scales and roughens (not in mere lumpiness but with sharp edges) its resistance will begin to increase. As a rule fouling begins on the hull where the stream-line motion is least rapid and not in rapid flow, the term fouling here meaning marine growth. It will be found on the sterns of full ships some time before it appears on the bow. The proper way to check assumptions as regards extent of fouling is not to permit it and be certain it does not exist. The loss of money in towing weed about is very large, much more so than may be believed by some owners. A careful study of the times when bottoms are painted and the time the ship remained clean afterwards, would probably pay in this respect, as marine growths are subject to the usual cycles of growth, mature life, decay and reproduction according to temperature and time of year.

Reply to Sir Eustace d'Eyncourt.—One is glad to receive the support of Sir Eustace in the endeavour to obtain accurate data on mercantile ships. The statement in the last sentence of his remarks coming from such a source should be carefully considered by those firms who are about to increase their tonnage. In particular the suggestion that trials should be made at full speed and at a lower speed is most valuable. This would enable the analysis to proceed with so much greater certainty, as each speed represents a new set of data obtained with very little additional expense. Sir Eustace in common with several other contributors has pointed out the use of the Michell thrust block in checking the propeller thrust, and I would like to urge this upon all those who are fitting Michell blocks. The additional cost of the thrust measuring appliance is small, and being practically contained in the block itself, it does not represent another "gadget" for the engineer to look after. When not in actual use for measuring thrust, the block is an ordinary Michell block the ring carrying the pivoted pads then forming one with the casing.

Reply to Rear-Admiral Taylor.—The need for further friction work on actual ships which Admiral Taylor comments on, has been already

referred to and except for the purpose of emphasizing his opinion that these are desirable, further remarks are not necessary. The Author

With regard to the effect of fouling, Admiral Taylor suggests that this may have been important in certain cases. It is known that Ships K, N, O, Table III. had only just been undocked, and that Ship A, Table I., was undocked just before the three-months period dealt with. It is quite true that fouling takes place very rapidly in confined waters into which refuse may find its way, and McEntee's experiments on fouling of plates not moving through the water are most illuminating in this respect. Similar experiments with raft models were made in Portsmouth Harbour by R. E. Froude about 1902, showing equally serious growth but the results have never been published. But there is a considerable difference between such fouling and that which takes place on a ship in motion—at least in the rate at which the fouling increases. It is necessary to make two other points in this connexion, namely, the relation between length of surface and nature of fouling. I refer to this in the paper read before this Institution in 1915, and to the incorrectness of assuming results for model hold for ship, as the roughness represented by a given stage of fouling, say one-inch weed, means 24-inch weed on the ship—an unforgivable amount.

The second point is the location of the roughness on the ship? A clean bow is far more important than a clean stern; this also is brought out in my 1915 paper.

As regards whether fouling was present in the cases dealt with in the Tables, whatever may be said in this direction, still leaves the main discrepancy untouched in many cases. Thus, in Ships A and B, the powers estimated from the hull model and from the ship screw differ from 15 to 25 per cent. Of this some 10 per cent. is due to appendages, and the rest may be due to wind, sea or to fouling as suggested. The possible effect of fouling is not very large here, and these are fairly typical. But the discrepancy between the powers estimated from the screw at its revolutions and those stated to have been developed on the shaft remain unexplained, if the initial data are accepted or the explanations suggested in the Paper are declined.

I have dealt with the question of surface of propeller blade in the reply to Mr. Hinchcliffe. No allowance was made for roughness either in propeller or hull in the analysis, and in so far as it existed on the ship, it must be included in the percentage of hull resistance which could not be accounted for in other ways.

Admiral Taylor refers to my hesitation in accepting very large wake fractions. This hesitation does not arise so much from the largeness of the wake as from one's knowledge that when they are large it is difficult to fix them accurately. The last sentence in the penultimate paragraph of Admiral Taylor's contribution indicates that in the United States these wakes are "tricky" and it has been shown by our work that they are not true wakes when the ship is fairly full, but are produced

The Author

by other factors. Their use therefore in hull efficiency, etc., is doubtful and hence my hesitation.

As regards nomenclature. I think it was a pity Froude commenced giving wake in terms of the velocity of the wake water. I hope to see an effort made by this Institution, to clear up a number of such points and to bring into force common terms in all tanks.

Reply to Capt. McEntee.—The main point in this contribution is the summary of the methods adopted in the Washington Tank to derive ship data from the model experiments. Put into Froude language, the method described for estimating ship data from model data consists in adopting the quasi-propulsive coefficient as found in the model, and using this in conjunction with the estimated e.h.p. to get ship shaft horse-power at the propeller end of the shaft. This method is precisely the same as that used in the Froude Tank for estimating powers. As regards estimate of revolutions, the method given by McEntee is to calculate N from the model results and to correct this for variation of slip in the ship compared with that for the model by the formula—

$$\frac{\triangle N}{N} = -\frac{\triangle S}{1 - S}$$

The slip in the equation should be the analytic or true slip, and not the nominal or apparent slip which it is stated is used at Washington. Thus, in a single-screw ship with a nominal slip of 5 per cent., the true slip will be of the order of 35 per cent. and this will give a 50 per cent. different value of $\frac{\triangle N}{N}$. Moreover, the formula is based on the assumption that the wake velocity does not vary with slip, as in obtaining it, $\frac{\triangle V}{\triangle S}$ has been taken as zero, V being the velocity of the wake water. This is sufficiently true in all "speed" ships and practically all twin-screw ships, but is not always the case for single screws. In applying the model correction to the ship it is tacitly assumed that ship and model have the same wake which is one of the things on which we should very much like to be definite. But the trouble with any such correction formula is to know what is $\triangle$ S in passing from model results, to ship in fine weather results, *before the ship is designed.* When the trial has taken place its nominal value is known, but its real value is not known even then, and I am doubtful whether much help is to be derived from this formula.

With regard to the treatment of slip. McEntee plots to a base of nominal slip computed from the face pitch as is also done in the Froude Tank. This face pitch can be measured and is definite, and is the quantity which the maker must have. But we extend our curves to find the slip for no thrust, and since no thrust must mean no slip (if the skin friction is neglected) this gives a measure of the virtual pitch. The definition of this pitch and the checking of its value at any slip form a quite good method of bringing out peculiar effects in any screw, and in

defining the causes of any unusual feature in the thrust curve, and this is one of the advantages of Froude's virtual slip. It carries other advantages in comparing different propellers with one another, which it is hardly necessary to dwell on here. The Author

Reply to Dr. Gebers.—The first point made is the different relative wake velocities which must exist in the ship and in the model, and Dr. Gebers concludes, as is done in the Paper, that the average ship-wake fraction is less than in the model. One must admit the absence of data as to how allowance is to be made for this, and although it must depend on the factor given by Dr. Gebers, the character of this dependence is unknown. In the Paper the attempt has been made to find it by comparing $\frac{T}{V^2}$ values in ship and model, and when both $\frac{T}{V^2}$ and $\frac{\text{Torque}}{V^2}$ are available we should be able at least to define instead of guess at the difference. In this connexion Dr. Gebers speaks of the model data often being dubious—the context suggesting that his doubts were due to the mode of making the experiments. One must admit that it is possible that the supports to the screw may effect the resistance of the model, and that the rudder being present or not present may make a difference. The former we have tried a number of times, and in the type of boat here dealt with, the effect is negligible—although it is known that in high-speed models it may become appreciable. The latter we have only tried on one model—with doubtful results. The effect appeared to be small, but Dr. Gebers's experience appears to be otherwise, and I hope to test this further.

I have already dealt with the ratio of effective to face pitch in answering Mr. Mumford. It may be useful to mention here the work done by Prof. T. B. Abell in the analysis of Taylor's model-screw results for effective pitch, which showed the latter varying up to roughly 12 per cent. excess of the face pitch. My point in the Paper, however, is that this is for a model screw. In the analysis of the ship data good general agreement is found by taking the ratio of effective to face pitch as being about 1·01 to 1·03, and I have suggested that this difference in excess in the two cases is a " scale " effect, and not a wake velocity effect.

Reply to Professor Bragg.—I am not clear what is meant by assumption (*a*). It is not involved anywhere in my paper. The efficiency of propulsion—or as we term it, the quasi-propulsive coefficient—is determined by model experiment. When it is broken up into component parts, assumptions are made as regards the wake fraction which is necessarily used in defining efficiency of screw and hull. The term " wake-gain " is used usually in connexion with the latter, but in model screw analysis it is invariably associated with screw efficiency, and arises from consideration of the work of the screw. Wake-gain is quite definite in some cases, and Prof. Bragg comes to this same conclusion in his arithmetical example.

I think it is clear from the Appendix to the Paper that these con-

The Author

siderations were fully in mind, and that assumption (a) is not the cause of the difficulties which have been met.

With regard to Prof. Bragg's suggestion that $(1-t)$ is a better criterion of power required than is hull efficiency, I cannot see how the latter can be used *alone* at all, and the former is only a force factor without any correlated velocity factor. The quasi-propulsive coefficient is the only proper term to use for this purpose, and unfortunately these are omitted from Prof. Bragg's tables.

In regard to the use of current meters for the determination of wake velocities, I believe Mr. Froude experimented with these and gave them up as not being reliable enough. Although giving most valuable data which could not be derived in other ways—as shown by Prof. Bragg in his recent paper—such meters cannot possibly measure the effect of the propeller thrust on the general wake, and in single-screw ships this is important as regards the slip of propeller.

Reply to Mr. A. J. C. Robertson.—I agree that we may look to the oil engine (and the geared turbine) for a good deal of light on the propeller problem, as the greater technical skill in design required is involving a greater knowledge of output of engine. With regard to his draught correction I think the case is not so simple as stated, and should prefer to correct the propeller output from such data as we have derived from running propellers at different immersions. Mr. Robertson's table contains some most instructive data as regards an important class of ship, and its addition to my paper will add considerably to its value. I am not clear, however, as to why the wake fractions and the thrust-deduction fractions should vary on one class of vessel as it does in the table. The comparison of sea service and trial-trip quasi-propulsive coefficients is good with all three propellers, and in this case supports Mr. Robertson's conclusion as regards diameter of propeller. One would only add the caution that these are moderately fine-lined ships and the application of this conclusion must not be stretched too much to other types.

In conclusion, although a good deal of the discussion has centred round methods adopted, the broad conclusions have been accepted by everyone. But the Paper will not have served its purpose unless these are carefully considered by those concerned in building and running ships, and put into practice for at least a trial. The Tank Staff will always be most happy to assist in such work, and to undertake analysis of data obtained in reliable trials.

NOTES ON THE ECONOMICAL PERFORMANCE OF TURBINE INSTALLATIONS IN MERCHANT SHIPS.

By T. G. POTTS, *Member.*

[Read in Newcastle-upon-Tyne on the 16th March, 1923].

It is the intention of the Author, in presenting this paper before the members of the Institution, to give a brief résumé of consumption trials which have been carried out in a few geared-turbine vessels under ordinary sea-going conditions, and to deal with a few points affecting the efficient running of turbine installations under such sea-going conditions. The performance of cargo steamers is usually examined on the basis of coal consumed per day under voyage conditions. In the case of a poor performance the responsibility may lie with the main engines, with the auxiliary machinery, or with the boilers, but cannot be placed. It is the object of the present paper to illustrate the usefulness and practicability of making measurements of water consumption, which will in most cases enable losses to be readily located.

The results of three ships have been taken for the purposes of this paper, and for the sake of convenience the respective steamers are indicated by the letters " C," " S " and " T." In Table 1 the general dimensions and particulars of the machinery installation are given for each of the the vessels taken. In Steamer " C " the turbine engines are of the Parsons type of the latest design with reaction blading throughout, consisting of three turbines in series connected to double-reduction gearing, the high-pressure turbine being fitted with " end-tightened " blading. In this vessel measurements of water consumption were made by means of tanks, and also by water meters. In order that the water consumption of the main and auxiliary machinery might be separately measured and over the same period of time, the pipe connexions were so arranged that the air pump discharged through a water meter to a temporary drain tank, and one of the main feed pumps drew the water from this drain tank and discharged to the measuring tank. All exhausts from the auxiliary engines were led to the auxiliary exhaust range and thence to the surface feed heater, and any surplus steam was led to the winch condenser. The drains from the feed heater, winch condenser, oil separator, cabin heating, etc., were led to the auxiliary cascade filter tank. A water meter was fitted in the drain

TABLE I.—GENERAL DIMENSIONS AND PARTICULARS.

Steamer.	"C."	"S."	"T."
Type	2 Deck and shelter deck.	3 Decks.	2 Decks and shelter deck.
Gross tonnage	5,494.	9,773.	8,526.
Length BP., breadth extreme, depth moulded.	425′ 0″×55′ 0″×37′ 3″ to shelter deck.	460′ 0″ × 62′ 6″ × 37′ 9″ to upper deck.	460′ 0″×62′ 6″×37′ 9″ to shelter deck.
Summer draught: displacement	25′ 6¼″ 12,840 tons.	29′ 4″. 18,500 tons.	29′ 4″ 18,500 tons.
Shaft horse-power, designed	3,150 at 80 revolutions per minute	4,500 at 85 revolutions per minute	5,500 at 85 revolutions per minute.
Boilers	3 Single-ended.	4 Single-ended.	5 single-ended.
Pressure	200 lb. per square inch.	190 lb. per square inch.	190 lb. per square inch.
Draught	Howden's Forced Draught.	Howden's Forced Draught.	Howden's forced draught.
Diameter: length	15′ 9″ × 11′ 9″.	17′ 6″ × 12′ 0″.	17′ 6″ × 12′ 0″.
Grate surface: heating surface	168 square feet; 7,863 square feet.	312 square feet; 13,900 square feet	390 square feet; 17,375 square feet.
Number of furnaces	9	16	20
Superheaters	North Eastern Marine Smoke tube type.	Smoke tube type.	Smoke tube type.
Superheat	200°	100°	100°
Machinery, general description	One H.P. and one I.P. turbine in tandem, and one L.P. turbine; all geared by double reduction gearing to one main shaft. All turbines reaction blading; H.P. fitted with "end-tightened blading"; air pump of the Dual type with steam jet augmenter; one centrifugal circulating pump with 2 single engines; pair of main feed pumps, direct acting, with control tank; cascade filter tank for auxiliary condensate only, one multiflow surface feed heater; two single direct acting forced lubrication pumps; one fan 90″ diameter with 2 single engines; two dynamos, one bilge and sanitary single, direct acting pump; one duplex ballast pump; one duplex general service pump; one single fresh water pump; one single direct acting auxiliary feed pump; one vacuum ash hoist with steam ejector; one 35-ton evaporator, and one	H.P. and I.P. turbines with individual pinions, geared to one primary wheel, and L.P. turbine geared to other primary wheel of double reduction gearing to one main shaft; H.P. turbine, impulse reaction; I.P. and L.P. turbines, all reaction. Air pump of Dual type; one centrifugal circulating pump with two single engines; two single direct acting main feed pumps; cascade feed filter tank for main and auxiliary condensates; one multiflow surface feed heater, two single direct acting forced lubrication pumps, one centrifugal oil cooler pumps, with single engine; two fans 84″ diameter, with single engine each; two dynamos with single engines; one duplex bilge pump; one duplex sanitary pump; one duplex ballast pump; one duplex general service pump; one duplex fresh water pump; one single direct acting auxiliary feed pump; [illegible]	H.P. and I.P. turbines with individual pinions, geared to one primary wheel, and L.P. turbine geared to other primary wheel of double reduction gearing to one main shaft; H.P. turbine impulse reaction; I.P. and L.P. turbines all reaction. Air pump, wet and dry, in conjunction with steam jet air ejector; one centrifugal circulating pump with two compound engines exhausting into main condenser; two single direct acting main feed pumps; cascade feed filter tank for main and auxiliary condensate; one multiflow surface feed heater; two single direct acting forced lubrication pumps; one centrifugal oil cooler pump with single engine; two fans 90″ diameter with one compound engine each; two dynamos with compound engines; one single direct acting bilge pump; one single direct acting sanitary pump; one single direct acting ballast pump; one single direct acting general service pump; one single direct acting fresh water pump; one single direct acting auxiliary feed pump; one ash ejector, with single direct acting pump; two evaporators and one Contraflo auxiliary condenser.

pipe between the auxiliary filter tank and the feed-pump control tank. The pipe and valve connexions were so arranged that at all times both main and auxiliary water meters were in operation and the water from either the main or the auxiliary engines or from both could be pumped to the measuring tanks.

The contractors' trials of this vessel had to be carried out with the vessel in ballast condition, and it was necessary that the propeller be fully immersed in order that consistent torsion-meter readings might be obtained. The vessel was specially trimmed by the stern to obtain this condition, giving an immersion of 10 inches at the propeller tip. The contractors' trials were as follows :—

Trial No. 1.—Sunderland to Antwerp, July 21st and 22nd, 1921.

Trial No. 2.—Antwerp to Tyne, August 19th and 20th, 1921.

Trial No. 3.—Progressive Trial off Hartley, August 23rd, 1921.

Owing to the lengthy period the vessel was lying in port between the date of launching and the trials, and also between the first and the second trials, it is interesting to note in Plate IX. the effect of the state of the hull upon the revolutions when the main machinery was developing the same power, and this difference is further shown by the results between the second and third trials due to the dry docking and painting after the second trial.

The results of the water-consumption trials Nos. 1 and 2 are shown on Plate X. plotted on the basis of s.h.p. The actual readings and data of these trials are given in Table 2. The results of the progressive trial No. 3 are shown in Plate XI., plotted to a base of speed and actual readings in Table 3. The e.h.p. shown on this diagram is the effective horsepower as deduced from model experiments (naked hull).

Since the vessel first went into service, over twelve months ago, the engineering staff of the vessel have taken full records of the performance of the propelling machinery on the several voyages. During this period the vessel had steamed a distance of about 50,000 miles.

During the outward passage in ballast, coal and water-measuring trials were carried out by the engineers of the vessel to determine boiler efficiency, the procedure adopted being to ship about 10 days' supply of coal into the cross-bunker, and after the vessel was clear of the port and the steaming conditions were quite steady, a start was made to measure the coal-and-water consumption at the beginning of one of the watches. The trial continued under these conditions until the coal was used up. The data of the results of these trials are given in Table 4.

In Table 5 the coal consumption results of eight outward and home voyages are given (home voyages loaded) during the same period, as taken from the engineers' log.

To obtain consistent readings it is necessary that the steaming conditions should be kept as steady as possible. Unfortunately, with coal-fired boilers and the varying conditions obtaining at sea, it is not reasonable to expect a steady pressure of steam for 24 hours together.

TABLE 2.

Draft Forward	10′ 9″	Propeller Diameter	18′ 6″
,, Aft	20′ 6″	,, Pitch	16′ 9″
,, Mean	15′ 7½″	,, Expanded area	100 sq. feet.
Displacement	7,465 tons.		

	Contractors' Trial No. 1—21st and 22nd July, 1921.								Contractors' Trial No. 2—19th August, 1921.			
	Sunderland to Antwerp.								Antwerp to River Tyne.			
Date	21–7–21		22–7–21		22–7–21		22–7–21		19–8–21		19–8–21	
Pressure at boilers	198		200		193		200		195		182	
,, and superheat auxiliary steam	198	86°	200	111°	193	108°	200	84°	195	83°	182	86°
,, ,, ,, main steam	192	162°	197	174°	186	184°	183	200°	173	197°	173	202°
,, ,, ,, H.P. first expansion	55	198°	81	202°	110	204°	150	204°	150	206°	165	207°
,, I.P. first expansion	7·27		15·8		27		42		40·3		45·5	
,, L.P. first expansion	17·95″		12·45″		5·5″		1·4 lb.		1·13 lb.		2·4 lb.	
Vacuum at L.P. exhaust	28·9″		28·74″		28·46″		27·87″		27·95″		28·26″	
Barometer	30·0″		29·95″		29·88″		29·84″		30·02″		30·1″	
Pressure auxiliary steam (reduced)	108		108		109		109		149		154	
,, ,, exhaust	6		5·5		8		10		5		9·5	
Auxiliary exhaust led to	Winch condenser		Winch condenser		Winch condenser and feed heater.		Winch condenser and feed heater		Winch condenser and feed heater.		Winch condenser and feed heater.	
Revolutions per minute	54·19		61·66		68·58		76·2		73·18		75·18	
Shaft horse-power	988		1,545		2,132		2,933		2,885		3,165	
Temperature of Sea	58°		58°		62°		60°		65°		61·5°	
,, ,, Circulating discharge	72°		74°		76°		78°		86°		84·5°	
,, ,, Wet air pump suction	76·5°		80°		79°		84°		92°		91°	
,, ,, Feed to boilers	103°		103°		158°		211°		216°		204°	
		Lb./S.H.P.		Lb./S.H.P.		Lb./S.H.P.		Lb./S.H.P.		Lb./S.H.P.		Lb./S.H.P.
Water lb. per hour, Turbines	11,920	12·07	16,180	10·47	20,790	9·75	26,750	9·12	26,565	9·207	28,660	9·055
,, ,, ,, Auxiliaries	3,170	3·21	3,430	2·22	3,770	1·768	4,530	1·544	4,485	1·555	4,730	1·494
,, ,, ,, Turbines+Auxiliaries	15,090	15·28	19,610	12·69	24,560	11·518	31,280	10·664	31,050	10·762	33,390	10·549

TABLE 3.—CONTRACTORS' TRIAL No. 3.

Progressive Trial on Hartley measured course, 23rd August, 1921.

Weather:—Wind, fresh N.NE.; Sea, slight swell.

Temperature of sea .... 58° Fahr.

Displacement.... .... 7,465 Tons.

Draft, Forward .... 10′ 9″
,, Aft .... 20′ 6″
,, Mean .... 15′ 7½″

Propeller Diameter .... 18′ 6″
,, Pitch .... 16′ 9″
,, Expanded area 100 square feet.

Direction of run	North.	South.	Mean.	North.	South.	Mean.	North.	South.	Mean.	North	South.	Mean.
Speed in knots	10·1237	11·3421	10·7329	11·5606	12·0805	11·8205	12·7298	12·2866	12·5082	12·0805	13·9211	13·0008
Revolutions per minute	63·16	63·4	63·28	70·0	70·75	70·375	75·75	74·4	75·075	79·4	79·55	79·475
Slip per cent. apparent	—	—	—2·6	—	—	—1·6	—	—	—0·8	—	—	+1·03
Shaft horse-power	1,531	1,523	1,527	2,119	2,133	2,126	2,750	2,510	2,630	3,191	3,179	3,185
Pressure at boilers	175	175	175	160	160	160	155	155	155	200	200	200
Pressure and superheat, main steam.	171 175°	165 175°	168 175°	149 187°	154 187°	151·5 187°	154 194°	142 97°	148 195·5°	194 202°	187 202°	190·5 202°
Pressure and superheat, H.P. first expansion.	80 206°	80 206°	80 206°	110 203°	110 203°	110 203°	144 203°	132 201°	138 202°	165 209°	165 209°	165 209°
Pressure, I.P. first expansion.	17·6	17·6	17·6	28·0	28·0	28·0	38·5	34·8	36·65	45·7	45·8	45·75
Pressure, L.P. first expansion.	9·0″	9·0″	9·0″	4·0″	4·0″	4·0″	0·5 lb.	0·8″	0·05 lb.	2·0 lb.	2·1 lb.	2·05 lb.
Vacuum at L.P. exhaust	28·84″	28·76″	28·80″	28·53″	28·58″	28·55″	28·31″	28·39″	28·35″	28·09″	28·07″	28·08″
Barometer	—	—	30·04″	—	—	30·04″	—	—	30·04″	—	—	30·03″
Auxiliary exhaust pressure.	15·0 lb.	14·0 lb.	14·5 lb.	10·0 lb.	9·0 lb.	9·5 lb.	5·5 lb.	5·5 lb.	5·5 lb.	4·0 lb.	5·0 lb.	4·5 lb.
Auxiliary exhaust led to	Feed heater and excess condenser.		to winch	Feed heater and excess condenser.		to winch	Feed heater and excess condenser.		to winch	Feed heater and excess condenser.		to winch

To test the efficiency of the plant during a voyage, it was arranged to run a short water-consumption trial (say of 2 hours) once every week, and to fix the following conditions:—

Initial pressure at the h.p. turbine to be maintained constant throughout this period; no make-up feed to be used, and ash-hoist engine shut off; the temperature of steam at the h.p. turbine to be maintained as constant as practicable.

During these tests the consumption of the main and auxiliary machinery was measured separately. A specimen of the data obtained under these conditions is given in Table 6, and illustrates the value of carrying out systematic tests.

Taking the results obtained on the 5th November, 1921, and comparing them with those obtained on the contractors' trials, it will be observed that at the same pressure and superheat there is an increase of steam consumption of the main condensate of 1,530 lb. per hour—equivalent to $6\frac{1}{4}$ per cent. of the main condensate. To locate this increase, a trial was run between the Tyne and Leith on the 17th November, which confirmed the chief engineer's figures of an increase of over 6 per cent.

It was found that the valve on the drain connexion from the high-pressure turbine first expansion belt to main condenser was leaking. On opening up it was found that the valve was badly eroded. This valve and seat were renewed, and on a subsequent test the consumption for the main condensate over and above that obtained on the contractors' trials was only 390 lb. per hour, or $1\frac{1}{2}$ per cent.

Another source by which the steam consumption is increased is in the case where there is air leakage to the main condenser resulting in a drop in vacuum, which necessitates driving the air, and particularly the circulating pumps at such a speed that the increase in the consumption of the auxiliary machinery is out of all proportion. As is probably well known, the circulating pump has the largest duty of all the auxiliary pumps, and in most cases is fitted with a single-cylinder engine exhausting against a back pressure of 10 lb., which can hardly be termed an economical engine.

In the case of "C" ship, a test was made on the effect of increasing the revolutions of the circulating pump by about 50 per cent., but it was found that whilst a higher vacuum was obtained, the increased consumption of the circulating engine practically neutralized the gain in turbine efficiency due to the increase in vacuum. This points to the desirability of a more economical circulating engine, such as a compound engine, to gain the benefits of a higher vacuum.

In this ship it will be noted from the data shown in the diagram that the steam consumption of the turbines only was at the beginning of the vessel's life 9·3 lb. per shaft horsepower at 2,700 horsepower, the steam pressure at turbines 139 lb. gauge, 204° of superheat, the vacuum 28·21 inches, and that the steam for the auxiliaries was 1·55 lb. per s.h.p.

TABLE 4.—BOILER EFFICIENCY TRIALS ON VOYAGES.

Voyage No.	2	3	5	6	7	8
Duration, hours	187·783	176	261·25	235·4	197·5	212·2
Boiler pressure, lb. per square inch	188	176	190	190	190	190
Temperature, superheated steam, Main	533°	566°	553°	556°	549°	551°
,, ,, ,, Auxiliary	450°	440°	425°	442°	468°	435°
Feed temperature	219°	220°	218°	219°	205°	200°
Water, lb. per hour—						
Main condensate (including make-up feed)	26,580	23,830	25,530	25,310	25,610	26,930
Auxiliary condensate	5,170	5,870	4,150	5,310	3,580	3,500
Boiler feed	31,750	29,700	29,680	30,620	29,190	30,430
Coal, lb. per hour	3,645	4,010	3,475	3,275	3,262	3,330
Kind of coal	Scotch.	Scotch.	Scotch.	Scotch.	Scotch.	Scotch.
Calorific value, B.T.U.'s (gross)	14,338	14,263	14,200	14,120	14,038	13,587
Evaporation at feed temperature	8·71	7·41	8·54	9·35	8·95	9·14
,, from and at 212°	9·93	8·4	9·66	10·58	10·28	10·52
Boiler efficiency, per cent.	67·2	57·1	66·0	72·7	71·0	75·1
Ashes, percentage of coal	15·2	18·1	16·4	12·8	12·9	13·5

After the vessel had been in service for over twelve months a water-measurement trial was carried out, and the water consumption of the turbines only was 9·35 lb. per s.h.p. with steam pressure at turbines 138 lb. gauge, 200° superheat, and 28·47 inches vacuum, being practically the same as taken in the trials twelve months previously; the consumption of the auxiliaries averaged 1·23 lb. per s.h.p., being 20 per cent. less than actually recorded in the original trial, due to the care exercised by the engineers of the vessel in regulating the speed of the auxiliaries and maintaining them in a reasonable state of efficiency. The results of these trials and also of the original contractors' trials for comparison are shown in Plate XII.

Reverting to the results of coal consumption of voyages, given in Table 5, as some engineers attach considerable importance to the value of the coal coefficient in making comparisons with other ships, the value of the coal coefficient has been added. It will be interesting to note that whilst the coal per shaft horsepower is practically identical at both light and load conditions, there is a considerable difference in the actual coal coefficient between the light and the full load condition when the machinery is developing practically the same shaft horsepower. This would appear to serve as an illustration that considerable care requires to be exercised in making comparisons on the basis of the coal coefficient. The condition must, of course, be correspondingly similar.

It may be of interest at this stage to refer to the question generally of auxiliary machinery in merchant ships, for whilst economy in steam consumption has been increasingly effected in the main turbines, the same can hardly be said of the auxiliary engines which form an important part of the propelling machinery in merchant ships. Reliability and economy in working are undoubtedly the two most important factors of auxiliary engines; piston engines are favoured by many engineers. Some auxiliary engines of this type are expensive in steam consumption, and although manufacturers have done much to improve them, there appears room for still further improvement in this respect.

TABLE 6.—TRIALS SHOWING EFFECT OF LEAKY DRAIN VALVE.

Date of Trial	5/11/1921	17/11/1921
H.P. 1st Expansion Gauge Pressure ...	135	140
H.P. 1st Expansion Superheat F°	199°	190°
L.P. Exhaust Vacuum at 30″ Barometer ...	28·6″	28·28″
WATER — LB. PER HOUR.		
,, Main Condensate	25,980	26,830
,, do. obtained on Contractors' Trials	24,450	25,155
Difference between Voyage and Trials ...	1,530	1,675
Percentage Increase due to Leaky Drain Valve	6·26	6·66

A closer attention might also be given by the designer to a more suitable arrangement in the ship and to suitable pipe leads. In many

TABLE 5.—VESSEL "C." RESULTS OF EIGHT OUTWARD AND HOME VOYAGES.

Voyage No.	Outward 1	Homeward 1	Outward 2	Homeward 2	Outward 3	Homeward 3	Outward 4	Homeward 4	Outward 5	Homeward 5	Outward 6	Homeward 6	Outward 7	Homeward 7	Outward 8	Homeward 8
Mean draft	14′ 10¼″	24′ 5¾″	15′ 9″	24′ 6¼″	15′ 9½″	20′ 6¾″	15′ 11½″	24′ 5″	16′ 3½″	25′ 0¼″	16′ 3¼″	23 6″ ′	16′ 6″	25′ 1″	16′ 3¾″	25′ 0¾″
Mean displacement, tons	7,065	12,268	7,537	12,290	7,567	10,140	7,648	12,240	7,836	12,570	7,828	11,730	7,945	12,600	7,853	12,585
Distance, miles....	3,196	3,220	2,901	2,908	2,903	3,107	3,162	3,230	3,201	3,126	3,151	3,124	2,757	2,907	2,861	2,589
Steaming time, days	11·25	11·66	11·106	10·873	11·518	11·173	13·187	12·672	12·363	11·129	11·85	11·39	9·79	10·546	10·292	9·64
Revolutions per minute	74·0	73·35	73·9	73·25	74·7	73·16	71·17	70·4	74·63	73·94	71·94	71·6	73·5	72·63	74·6	74·26
Shaft horse power	2,490	2,550	2,641	2,668	2,351	2,512	2,623	2,626	2,709	2,825	2,739	2,764	2,701	2,715	2,763	2,834
Speed in knots	11·83	11·5	10·88	11·15	10·5	11·58	10·0	10·57	10·79	11·7	11·08	11·43	11·72	11·49	11·58	11·19
Slip % (apparent)	3·2	5·1	10·9	7·9	14·9	4·2	14·9	9·0	12·5	4·2	6·8	3·4	3·4	4·4	6·0	8·8
Coal per day, tons.	38·0	37·0	39·6	39·7	40·4	39·7	39·9	40·8	37·1	37·7	34·7	35·7	35·0	36·3	36·5	35·8
Coal, kind used....	Durham	Canadian and Durham	Scotch.	Scotch.	Scotch. Unscreened, very dirty.	Scotch. Unscreened, poor and fair.	Scotch. Poor and dirty.	Scotch. Bad.	Scotch. Good steaming.	Scotch. Good to fair.	Scotch. Fair and clean.	Durham Fair steaming, small.	Scotch. Good.	Scotch. Good steaming.	Scotch. Good steaming.	Scotch. Good steaming.
Coal, per s.h.p. hour .... lb.	1·425	1·353	1·399	1·389	1·602	1·475	1·42	1·45	1·278	1·245	1·182	1·205	1·209	1·248	1·231	1·179
Percentage of ash	—	—	12	9·2	18	16	23	21	15	16·4	12·8	14·6	12·9	12·25	13·5	13·0
$\frac{D\ 2/3 \times V^3}{S.H.P.}$	245	317·5	187·5	277	189·5	290	148	239	183	306·5	196	279	237·5	302·5	222	267
$\frac{D\ 2/3 \times V^3}{\text{Coal per day}}$	16,050	21,900	12,500	18,610	11,050	18,350	9,720	15,400	13,350	22,960	15,450	21,600	18,340	22,620	16,820	21,150

cases it would appear that further economies might be effected by the sea-going engineer in a more economical running of the auxiliary plant. It is true that the exhaust from auxiliary engines can be utilized to heat the feed water to an economical temperature, but in many cases a much greater quantity of exhaust steam is available than is necessary to heat the feed water to, say, 220°, the surplus being led to the main or winch condenser. The alternative is to lead the surplus to the l.p. turbines. It is common practice in naval work to utilize the auxiliary exhaust in the l.p. turbines when about 50 per cent. of the available energy is recovered.

This arrangement has been adopted in many merchant ships and in some instances has been found to prove disadvantageous where oil has been carried over with the steam from internal lubrication of the auxiliaries, which tends to leave a deposit on the blades of the turbine and the condenser tubes.

The ideal condition for merchant ships appears to be that where the consumption of auxiliaries is reduced to such an amount that all the exhaust can be utilized in feed heating.

In the vessel "C" just previously described, the whole of the auxiliaries are now utilized in feed heating.

Referring to the boiler efficiency trials given in Table 4, the main condensate includes make-up feed which was taken from the reserve tanks and led to the condenser so as to maintain a constant boiler level. This question of lost water has an important effect on the general economy, especially if the evaporator has to be used to restore it.

As illustrating the effect on the coal bill of two vessels, viz.: "S" and "T", which came under the Author's notice, it may be mentioned that in the case of "S" running at *two-thirds* power, the boiler feed as measured was 44,100 lb. per hour; the steam for auxiliary engines was 11,150 lb. per hour, leaving 32,950 lb. per hour for main engines plus all drains from cabin heating, galley, etc., which were led to the filter tank. The percentage of auxiliary consumption to main engines. etc. was 33 per cent.

In column "B" of Table 7 it will be observed that when running under the same condition as "A" there was sufficient auxiliary exhaust steam led to the feed heater to give 214° feed temperature, leaving a surplus of 5,000 lb. per hour still passing to auxiliary condenser. After this voyage the auxiliary engines were overhauled, and modifications were made so that the oil coolers were circulated by the main circulating pump instead of a separate pump as formerly used. On the next voyage made by this vessel when steaming at full power, the auxiliary consumption was reduced by nearly 1,000 lb. as compared with the total quantity of auxiliaries when running at only two-thirds power. The percentage of auxiliary to main engines plus drains was about 20 per cent. at full power as against 33 per cent. at two-thirds power. There was still, however, more than sufficient to heat the feed water as before to 214° F.

	A.	B.	C.
Pressure at boilers .... lb.	180	181	180
,, and superheat at H.P. nozzle box	102 lb. 166°	102 lb. 166°	150 lb. 102°
,, H.P. reaction first expansion lb.	60	60	102
Vacuum at 30″ barometer	28·1″	28·1″	27·3″
Revolutions per minute	70·76	70·96	80·75
Shaft horse power	2,960	2,960	4,491
Auxiliary exhaust pressure .... lb.	6·5	6·0	2·0
,, ,, steam led	All to auxiliary condenser, auxiliary drains, galley and steam heating drains to filter tank.	To feed heater and surplus to auxiliary condenser, auxiliary drains, feed heater drain, galley and steam heating drains to filter tank.	All to auxiliary condenser, auxiliary drains, galley and steam heating drains to filter tank.
Air pump, D.S. per minute	26·9	27·5	32·0
Circulating pump, revolutions per minute	105·6	104	165
Forced lubrication pumps (2), D.S. per min.	17	22	14
Fan engines (2), revolutions per minute	242	230	280
Dynamo engine	One.	One.	One.
Main feed pump, D.S. per minute	6·15	6·26	9·04
Ballast pump, ,, ,, ,, ...	30·37 circulating aux. condenser.	31·0 circulating aux. condenser.	32·0 circulating aux. condenser.
General service pump, ,, ,, ,, ...	20·6	22·4	Not in use.
Oil cooler pump, ,, ,, ,, ...	172·4	187·5	Not in use.
Bilge pump, ,, ,, ,, ...	—	—	12
Sanitary pump, ,, ,, ,, ...	105	98·3	48
Steering engine	As required.	As required.	As required.
Temperatures—			
Circulating inlet and discharge	51·5° 68°	51·5° 68°	74° 91°
Feed discharge to boilers	84°	214°	106°
Make-up feed	Nil.	Nil.	Nil.
Water, lb. per hour—			
Main engines plus alldrains to filter tank	32,950 }	39,300	51,039
Auxiliaries to filter tank	— }		—
Auxiliaries to auxiliary condenser	11,150	5,000	10,211
Boiler feed	44,100	44,300	61,250
Water, lb. per S.H.P. per hour—			
Main engines, plus all drains to filter tank	11·13 }	13·277	11·36
Auxiliaries to filter tank	— }		—
Auxiliaries to auxiliary condenser	3·77	1·69	2·28
Boiler feed	14·90	14·967	13·64
Percentage of auxiliaries to "main engines plus all drains to filter tank"	33·8	—	20

TABLE 8.—STEAMER "T".

Duration of trial	8 hours.
Pressure at Boilers	190
,, and superheat at H.P. nozzle box	163 118°
,, H.P. reaction, 1st expansion	87
Vacuum at 30″ barometer	28·9″
Revolutions per minute	86·9
Shaft horse-power	5,750
Auxiliary exhaust pressure	3·5
Auxiliary exhaust steam led to	Feed heater, excepting that circulating engine exhausts into main condenser.
Air pumps, D.S. per minute	22
Circulating pump, revolutions per minute	154
Forced lubrication pumps (2), D.S. per minute	21
Fan engines (2), revolutions per minute	340
Dynamo engine	One.
Main feed pump, D.S. per minute	7·55
Ballast pump, ,, ,, ...	Not in use.
General service pump, ,, ,, ...	Not in use.
Oil cooler pump, ,, ,, ...	Not in use.*
Bilge pump, ,, ,, ...	Not in use.
Sanitary pump, ,, ,, ...	22
Ash ejector pump, ,, ,, ...	As required.
Steering engine, ,, ,, ...	As required.
Temperature circulating inlet	48°
,, ,, discharge	68°
,, feed discharge to boilers	194°
Make up feed from tank	Water level in boilers brought to same at finish as at start of trial.
Water, lb. per hour—	
Turbines plus auxiliaries	63,000
Make up feed from tank	850
Boiler feed	63,850
Water, lb. per S.H.P. per hour—	
Turbines plus auxiliaries	10·95
Make up feed from tank	0·15
Boiler feed	11·10

* Oil cooler circulated from main circulating pump.

Note.—The following data shows the effects on auxiliary exhaust pressure and feed water temperature due to ash ejector being in use.

Ash ejector	Off.	On.
H.P. first expansion pressure	87	87
Auxiliary exhaust pressure	3	11
Temperature:—Feed heater inlet	93°	99°
,, ,, ,, outlet	202°	216°

To show the effect of fitting different types of auxiliary engines, the vessel "T" is given, being a sister ship to "S" but of increased

power. The circulating pump engine was of the compound type exhausting direct into the main condenser, also supplying circulating water to the air coolers. The forced-draught fan engines were also of the compound type exhausting into the auxiliary range. During one of the voyages a trial of 8 hours' duration was carried out, and the results are given in Table 8. It is interesting to observe that in this instance the whole of the steam from the auxiliary exhaust range was utilized in the feed heater, the temperature of the feed to the boilers being only 194°, the water consumption for turbines and auxiliaries being 10·95 lb. per s.h.p.

The effect on the feed temperature and auxiliary exhaust pressure when the ash ejector was in use is also worth noting, the feed temperature rising from 202° to 215°, and the pressure rising from 3 lb. to 11 lb.

During the last few years it has been the Author's privilege to have the opportunity of investigating the performances of a number of geared-turbine steamers, and in some cases where high consumptions of fuel have been recorded, it has been regrettable to note the poor efforts put forward to locate the reasons for them. Quality of fuel, poor class of fireman, and other similar reasons have been advanced, and, whilst to a certain extent these have been contributory causes, there are many cases where more careful investigation—such as water-measuring the main engines and auxiliaries separately—would have led to a correct location of the main causes of the apparent high consumptions.

It is appreciated that the installation of measuring tanks may involve expense which some shipowners may not be prepared to face, but the fitting of meters for measuring the water can be carried out at a nominal cost, and the advantages of such measurements are of great value to the shipowner. It is true that water meters are looked upon with a certain amount of suspicion as regards their reliability, but when properly looked after and precautions are taken at intervals against errors, by calibrating the meters, they are found to be very reliable. Where the measurements were carried out in vessel "C", referred to in the paper, the engineers took a particular interest in this work, and it enabled them to place their fingers on any defects in the system which they were unable to locate by casual observation.

The Author takes the opportunity of thanking those shipowners and superintendent engineers who have from time to time granted facilities for the purpose of these investigations.

[DISCUSSION.

DISCUSSION.

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E. : I think I am right in saying that Mr. Potts's paper varies in importance indirectly with its length, and it is a paper which must be of great importance not only to all marine engineers but even more so to chief engineers and superintendents and shipowners; and again—if one might emphasize it—of still more importance to all makers of auxiliary machinery. There are several points in the Paper of extraordinary interest, apart from the tables, which speak for themselves, and which I think would be of advantage if hung up in every engine room to show what could be done by care and maintenance and the minimum use of auxiliaries to give the best results from the main engines.

As the Author points out, the naval practice of using the exhaust steam in the main engines was one which gave us results, when the early days of turbines came into vogue, which were surprising. I recollect on some of the earliest trials, that when the exhaust steam from the auxiliaries was led into the low-pressure turbines we were astonished at the power we used to get. The most startling effect I heard of was brought to my notice by a well-known naval engineer who took the first Dreadnought to sea and drove the vessel at quite a respectable speed without opening the main steam valve, driving the engines by the exhaust from the auxiliaries.

In later trials we developed feed heating by direct contact and other systems of exhaust, and we found that by heating the temperature of the feed to the greatest possible extent, short of the feed pumps giving trouble, we got better boiler results because we gave the boiler less up-and-down work to do : but we could never reach the high temperatures attained in the merchant service, due to lack of head.

I would like to ask if there is any great benefit derived from the reduced steam in auxiliary service advocated by many superintendent engineers. It would also be useful to know the extent to which the consumption is affected by radiation. One appreciates the precautions taken in some power stations in the way of cleading or covering as I feel that there is still a great deal of saving that can be effected by eliminating radiation.

I could say much more, but I do not wish to detain you. The Paper has been of great interest to me and, I am sure, to the whole Institution.

Dr. Morrow

DR. JOHN MORROW, *Member* : Towards the end of this valuable paper the Author draws attention to the fact that water meters are looked upon with a certain amount of suspicion, and there can be no doubt that, in general, this suspicion is justifiable. The method of measuring the condensate by means of measuring tanks is, in all cases, greatly to be preferred, and I am glad to see that this method also was adopted for the trials described in the Paper. Mr. Potts and his colleagues are, however, so thoroughly conversant with the difficulties

arising in the measurement of water that we may rest assured that the figures put forward by them are perfectly reliable. Dr. Morrow

There is, however, a third method which has been employed occasionally and which combines convenience with reliability. It consists essentially of the passage of the condensate through calibrated orifices or nozzles and measuring the head of water over the orifices. Means have to be adopted to damp out the oscillations due to the periodic action of the air pumps. Some such apparatus might well form part of the equipment regularly fitted in every ship.

A consideration of the performance of a propelling installation as dealt with in the Paper naturally divides itself into three parts, namely, the main engines, the auxiliaries and the boilers. In a paper by Mr. R. J. Walker, read before the Institution in 1919, figures are given for the expected performance of double-reduction-geared turbines (*vide Transactions* Vol. XXXVI., Part 3, page 82, table) and whilst I should like to say that the results of the full-power trials given for vessel "C" in Table 2 are as good as, or better than, any similar performances of which I am aware, it must be admitted that they fall short of the standard laid down by Mr. Walker.

Until a few years ago land turbines enjoyed a distinct advantage over the lower-speed marine turbine, but, owing to the introduction of double-reduction gearing, the conditions are now reversed, and it is the marine turbine which is able to run at the speed most suitable for economy, whilst the speed of the direct-coupled turbo-alternator is limited to that required by the alternator. Bearing this in mind, and reviewing the results obtained in land practice, it does not seem that Mr. Walker's standard is unduly optimistic for marine installations in which superheating is adopted.

During the short time I have had Mr. Potts's paper in hand I have tried to see in what respects the results contained in it fall short of the above standard.

The efficiency ratios, based on steam to turbines only and on the steam conditions at the h.p. first expansion, will be found to vary from about 0·62 for the lowest power trial of Table 2, to 0·75 at full power; and whilst these results are very good compared with those hitherto published, it would appear that still further economy is possible.

In this connexion I should like to refer to the presence of the h.p. astern turbine usually incorporated in the i.p. ahead cylinder in installations of this type. The two parts are separated by a diaphragm fitted with an internal gland. Under ordinary running the astern portion is connected with the condenser so that its rotor is running *in vacuo*. The horizontal joint in the diaphragm is presumably a metal-to-metal joint and, as in the case of the somewhat similar diaphragms of impulse turbines, it is not bolted. It appears to me that, no matter how carefully such joints are made when the turbine is cold, they are liable to open up under steam. Leakage will then

Dr. Morrow

occur from the i.p. turbine direct to the condenser, through the joint as well as through the gland, and an appreciable loss may occur. This has no counterpart in the land turbine, I should like to ask the Author whether he has formed any opinion as to its magnitude in connexion with the trials of Table 2.

A second source of loss appearing in these trials is due to the throttling of the steam between the boilers and the turbines. In the full-power trial, Sunderland to Antwerp, *i.e.*, the fourth column in Table 2, the pressure of 200 lb. per square inch at the boilers is reduced to 150 at the h.p. first expansion. Although this throttling action does not necessarily involve a reduction in the heat contents of the steam, it decreases the proportion of that heat that is available for transformation into mechanical work, and at the same time it increases that portion which must necessarily be discharged to the condenser. It can be shown that, for an initial pressure of 200 lb. with 200° superheat on the Fahrenheit scale, throttling to 150 lb. pressure is accompanied by a loss of nearly 4 per cent. of the available energy of the steam. It appears from the progressive trials of Table 3 that the ship can maintain a speed of 13 knots with a pressure of only 165 lb. per square inch at the first expansion under trial-trip conditions, the revolutions being then 79½ per minute. It would seem, therefore, that under normal service conditions the turbines are not really operating at full power and consequently they were not displaying their best efficiency during the trials in Table 2.

The Paper gives no particulars of the dimensions and blading of the turbines, and we are therefore unable to judge whether the designs are completely up to date, or whether they have been influenced by war conditions in such a way as to result in some sacrifice of efficiency.

With regard to the auxiliaries, the appreciable economies referred to are noteworthy, and to some extent may be attributed to the fact that the auxiliary steam was measured. Such measurements provide the engineers of the ship with the information necessary for the realization of these economies; and if designers and owners provide the necessary apparatus I think we may rely upon the engineers to make the best use of it. It is in the measurement of the auxiliary steam that the greatest hope for improved design and operation lies.

Referring now to the boiler trials, given in Table 4 of the Paper, it is interesting to compare the results for Voyage No. 3 with those of Voyage No. 8. The boiler efficiencies are respectively 57·1 and 75·1 and appear to be very discordant, the load on the boilers being about the same in the two cases. The chief loss in a boiler is, of course, that due to the discharge of heat to the uptake; and the amount of this loss is to a considerable degree within the control of the operating engineers. It depends upon two factors, namely, the temperature and the amount of the products of combustion, and fortunately it is possible to reduce

these quantities simultaneously. There is, I find, some misconception on this point; but it would take too long to go into the question now, and I must be content with saying that the relative magnitude of the loss at different times may be made evident by measurement of either the temperature or the amount of CO_2 in the gases at the base of the funnel, or preferably by both. Dr. Morrow

The funnel temperatures were apparently not taken, or at least they are not given in the Paper; but the fact that the highest superheat occurs in Voyage 2 suggests that the low boiler efficiency in this case was due to a high uptake temperature.

Just as the provision of a meter leads to economy in the use of steam, so the fitting of an instrument for the measurement of temperatures at the base of the funnel would lead to increased economy in the generation of steam in the boilers.

Mr. JOHN NEILL, *Associate Member*: Referring to Plate IX., the difference between the curves is said to be due to the state of the ship's hull. Whilst this will doubtless be the principal reason for the difference, might I suggest that the effect of the wind would also have some influence. Trials 1 and 2 were carried out, one in each direction only, whereas on trial 3 the spots represent the means of double runs. Mr. Neill

The water consumption figures given on Plate XII., are very interesting. For turbines only at full power the consumption is practically 9 lb. per s.h.p. hour; this means an efficiency ratio on the Rankine cycle of about 74 per cent. Since this must include windage losses of astern turbines, frictional losses in double-reduction gearing, and in thrust block, the overall result appears very satisfactory.

It would be interesting to know how the efficiency ratio compares with similar figures obtained from land turbines and an analysis of the losses might give some indication of the mechanical efficiency of double-reduction gearing under actual service conditions at sea.

The boiler efficiency figures given in Table 4 indicate clearly the importance of using a good steaming coal. During the last three voyages, when good coal, having ash about 13 per cent., has been used, efficiencies as high as 75 per cent. have been obtained. These efficiencies are based on the gross calorific value of the coal, but if an average amount of moisture be allowed for and the efficiency based on the calorific value of the coal as fired, the overall efficiency of boilers and superheaters must have approached 80 per cent. We have often had figures of this sort under similar conditions with our experimental plant ashore, but that these figures are obtained under service conditions at sea indicates a very satisfactory state of affairs.

Incidentally, Table 4 shows that calorific value in itself is very little use in determining the steaming qualities of a coal. Coals with a large proportion of ash cannot be burnt so as to utilize fully the heat units

Mr. Neill

of the coal, and so the boiler efficiency drops. From the shipowner's point of view it shows clearly the benefit to be derived by supplying good bunker coal. The price may be a few shillings a ton more, but the saving due to increased efficiency of the boilers far outweighs the extra cost, and is the only true economy.

Reference is made on page 548 to the use of coal coefficients for comparing the performances of ships. Whilst this method must, of course, be used with care, if ships are reasonably similar the method is of great value, particularly when the information as to power developed is not complete. An interesting case came before my notice a short time ago. Three sister ships each completed round voyages of about 80 days, steaming at various displacements and speeds. Voyages were run simultaneously, and vessels coaled at same ports. When averaged out the coal coefficients were within 2 per cent. from the highest to the lowest. Had the performance been compared on the coal consumption per h.p. the ships would have appeared to vary about 18 per cent., but this was obviously due to incorrect returns of the h.p. The use of the coal coefficient in this case allowed of a correct comparison being made.

The general results obtained during voyages 6, 7 and 8 of Vessel C are extremely good, and indicate what may be expected from geared-turbine machinery with careful operation. The coal consumption per s.h.p. hour is slightly over 1·2 lb., and this figure also represents a good result in coal per i.h.p. hour for quadruple-expansion reciprocating engines using superheated steam. Therefore, under good conditions, the geared-turbine installation would appear to be about 12 per cent. more economical than the steam reciprocating one. It must, however, be kept in mind that in a turbine installation a very much greater proportion of steam is required for auxiliary purposes; the proportion is probably twice that required for the auxiliaries with reciprocating machinery. This explains why in a turbine ship it is of such great importance to pay special attention to the auxiliaries. The figures for auxiliary consumption given for Vessel C at the beginning of the vessel's life and after twelve months' service bring out this point most clearly, as also do the comparisons beween Vessel S and Vessel T.

In conclusion, I wish to thank Mr. Potts for the very interesting results he has produced; the very thorough manner in which this mass of data under trial and service conditions has been recorded is rather unique, and I trust will have the desired effect in causing an increased interest to be taken in the question of measurements of water consumption.

Mr. Walker

Mr. R. J. WALKER, C.B.E., *Fellow*: I had not intended making any remarks on this paper, preferring rather to leave the field to Mr. Potts; but I may be excused for offering a few comments in regard to Dr. Morrow's reference to some statements in a paper that I read before

the Institution in 1919. I am sorry I have not a copy of that paper with me so as to make a comparison, but I think generally that the questions Dr. Morrow raises are subsequently answered almost in his own words. Mr. Walker

As regards the figures of steam consumption given in Mr. Potts's Paper for the Steamer "C", it will be observed that the turbines for this vessel on service were running below their designed conditions, which would account for a partial falling off in the efficiency as compared with the designed full-power conditions.

The figures I gave relating to steam consumptions in the paper referred to by Dr. Morrow were given on the basis of the designed conditions being fulfilled as regards power, initial pressure, superheat and vacuum. These figures were based on actual results previously obtained with geared turbines, and I think are capable of achievement; and if the necessary corrections are made in the Steamer "C", it will no doubt be found that the results obtained in that vessel are more or less in agreement with the steam consumptions referred to in the 1919 paper.

Mr. A. J. BERRY, *Member*: In view of the Author's statement that when checking the condensate by means of a separate water-meter it had been established that the auxiliary machinery was responsible for a large proportion of the total steam consumption, would he state whether they were working on superheated steam, as I understand that this vessel was fitted with smoke-tube superheaters. Also, if this was the case, could Mr. Potts give any information with regard to the effect the superheat had on the running of these auxiliary engines? Mr. Berry

VOTE OF THANKS.

Mr. HAROLD THOMSON, *Vice-President*: I have very great pleasure in moving a hearty vote of thanks to Mr. Potts for this very interesting paper. Personally I am sorry that we have not had more discussion because I think the paper is well worthy of discussion. It is a very valuable paper, partly because the figures given in it are absolutely reliable—at least I believe them to be so—which is not always the case with data obtained from trial trips or sea voyages. I had the pleasure some years ago of being associated with Mr. Potts on many occasions when we ran trials at sea and I can testify to the great care with which all his results were obtained, and I think the great value of this paper lies in the fact that we can be sure of the accuracy of his figures. The Paper shows the very great importance of little things in engineering matters. We talk about the great gain to be effected by altering methods of propulsion and other things, and Mr. Potts comes along and shows that we can gain six or seven per cent. simply by a little attention to valves. There is a great deal of useful information in the Paper, and I hope you will accord Mr. Potts a hearty vote of thanks for the trouble he has taken in writing it. Mr. Thomson

CORRESPONDENCE.

Mr. Butler

MR. R. J. BUTLER (of Messrs. William Beardmore & Co., Ltd.): The paucity of figures available for actual fuel and water consumption of ships in service makes this paper an extremely valuable one, and the thanks of the profession are due to the Author for publishing so freely the results obtained. There must be an enormous mass of data available to superintendent engineers on the subject and it is much to be regretted that little of this ever reaches the light of day. Of course, the expenses of fitting measuring tanks to determine the exact water consumption—the only reliable means of ascertaining this figure—are such as to render this a very uncommon proceeding in mercantile ships, even for contractors' trials, but the cost of integrating counters on the main feed pumps is very small and much useful information could be acquired, even if only of a relative and not an absolute nature, by these instruments. The Author is fortunate in his experience of water-meters, as most engineers have found these fittings to be unreliable on shipboard: perhaps he would state the type of meter used?

With regard to Table 4, the lb. of coal per hour are given for 6 voyages and comparing these with the tons per day quoted in Table 5, some small discrepancies are to be noted. Perhaps these are due to the length of the day on the voyages not being exactly 24 hours.

The difficulty of accurate design is borne out by comparing Voyages Nos. 5 and 8 of Ship "C." In voyage 8 a larger quantity of water was evaporated for a slightly smaller expenditure of coal of inferior calorific value, with the result that the boiler efficiency was no less than 9 per cent. higher with approximately the same steam conditions, and rate of firing only differing by a fraction of a lb. per sq. ft. of grate area. Turning to Voyage 3 a still further drop of 9 per cent. is shown with the same quality of coal and the same total evaporation per hour. Although these points concern more closely the boiler performance, the large discrepancies would show that scarcely as much reliance is to be placed on the accuracy of the water meters, as the Author appears to have, and his remarks on this subject would be much appreciated.

In Table 5, are shown large differences in the average speed under almost exactly similar conditions of draft and displacement obtaining in Voyages 1, 2, 4—a drop of $\frac{1}{2}$ knot—and this is presumably due to the bottom fouling; hence also the progressive falling off in the Admiralty constant, and coal coefficient. Such information tabulated against days out of dock, would be of great value, and perhaps Mr. Potts could see his way to give this. The Author's figures as to the gluttonous capacity for steam of auxiliary engines, can be confirmed. Although many makers will quote these consumptions when tendering, such great discrepancies occur in the estimates of different manufacturers for the same size and duty of auxiliary engine, that there is pressing need for systematic investigation of this question, with a view to reliable information being available. Further, if Mr. Potts would add some information as to the leading particulars of the gearing fitted, he would increase the indebtedness to him already felt for the production of such a valuable paper.

Mr. T. A. CROWE, *Graduate*: Particular interest attaches to Table 6 of Mr. Potts's paper, which gives actual boiler efficiencies obtained at sea over a period of 10 days. Trials of marine return-tube boilers are very scarce and particularly over such an extended time. The efficiencies vary from 57·1 per cent. to 75·1 per cent., *i.e.*, 18 per cent.; or, in the first case the boilers are consuming 24 per cent. more coal than in the second case. The calorific value of the coal is actually 5 per cent. higher in the first case, than in the second. The boilers are working at practically the same output in each case, so that the loss in efficiency does not appear to be fully accounted for by the extra 4·6 per cent. of ash in the second case. It would appear either that a large amount of excess air was being supplied or that the funnel temperature was very high. Mr. Crowe

It would be interesting if Mr. Potts could give us the smoke-box temperature and the percentage of CO_2 in the funnel gases, in each of these cases if they are known. In some trial results given in a paper by Mr. Reid read before the Institution of Naval Architects he shows how by reducing the air supplied from 23·1 to 14·8 lb. of air per lb. of coal, the funnel loss was reduced by 30 per cent., and the boiler efficiency was increased nearly 12 per cent. It appears extraordinary that shipowners will go to great expense to increase their turbine efficiencies by one or two per cent. and yet they will not spend a few pounds to increase their boiler efficiency by 10-12 per cent.

Mr. FREDERICK T. EDGECOMBE (of Messrs. William Denny & Bros., Ltd.): This is a paper that all who have to do with ship design must be interested in, not only for the intrinsic value of the carefully garnered data, but also, and perhaps more importantly, for the method of analytical examination that has been used. Measurement of coal consumption and water consumption have been simultaneously made and give the evaporative efficiency of the boilers, water consumption and shaft horse-power indicate the turbine efficiency, shaft horse-power and effective horse-power from model experiments are a measure of propulsive efficiency, and if the model experiments have included model propeller tests to ascertain the value of wake and augmentation of resistance, we have a comparatively complete system of analysing the different components which go to make up the total efficiency. Mr Edgecombe

The Author points out in his opening remarks that "in the case of a poor performance, responsibility may lie with the main engines, with the auxiliary machinery, or with the boilers." If by "poor performance" is meant the total efficiency of the whole ship as a system, then we must also include as possible contributors to a poor result, the state of the paint of the immersed hull, wind and weather conditions, sea temperature, depth of water, and design of propeller. From the shipowner's point of view, the displacement carried, per ton of coal consumed in relation to speed, must always be the ultimate test and this includes all the elements mentioned.

The Paper points out the difference in revolutions for the same shaft

Mr. Edgecombe horse-power which were obtained when the ship was dirty and when she was clean painted, but it is necessary to note that there may be substantial variations in results with apparently clean painted conditions, if care be not always taken to ensure that the paint on the bottom has been thoroughly dried before undocking the ship.

The only real indication of complete performance is a measured-mile trial where all these features are taken account of; but for an analysis of mechanical efficiency of machinery only, very valuable results may be obtained when the ship is on her ordinary service, as the Paper so lucidly demonstrates. Faults either in construction or design of the boilers, turbines, gearing or auxiliaries may be located if it be found that the shaft horse-power delivered be not in satisfactory accordance with the coal consumed, and if it be at all common for auxiliaries to consume 33 per cent. of the main power, with no special constructional fault that can be detected, there is obviously great force in the Author's contention that more attention might usefully be paid to the design of the smaller engines.

The shaft horse-power delivered must be measured by torsion-meters, and these should be capable of averaging any variation of torque that may be present, so that true mean torsion is obtained.

Sectional analysis of this kind has all been done most systematically and the results separately presented, as they should be. The propulsive efficiency obtained with ship "C" on the measured mile as given in Plate XI. is only 51·7 per cent. on a basis of naked e.h.p. and shaft horse-power; if we allow 10 per cent. for losses in engine friction and gearing, the propulsive coefficient on a basis of naked e.h.p. and indicated h.p. is only 46·5 per cent., which suggests that there is room for enquiry as to the conditions at the trial. It is possible that in spite of the ship being heavily trimmed by the stern, the tips of propellers not being deeply immersed, there may have been air-drawing at the propeller; but the example seems to demonstrate the necessity for more and more investigation of the same kind.

Analysis by means of model experiments to obtain propulsive efficiency, and by means of torsion-meters to get mechanical efficiency, with further subdivisions as between hull and propeller in the first case, and between turbines, boilers and auxiliaries in the second case, is the only sure road to progress. The value of such data is cumulative and gains strength from confirmation by other and independent authorities. Its synthetical value in building up new designs, when once firmly established, is obvious.

Attention is drawn in the Paper to the care that must be exercised in making comparisons on the basis of the coal coefficient expressed by the formula $\frac{D^{\frac{2}{3}} \times V^3}{\text{Coal per day}}$. The difference in value of this coefficient for light and loaded draughts is very great and the coefficient is only applicable to exactly similar conditions. This limits the value of it considerably, but probably some of the discrepancy is due to the fact

that for such a ship and speed, the e.h.p. varies more nearly as $D^{\frac{1}{2}}$ than as $D^{\frac{2}{3}}$ which is used in the formula. If $D^{\frac{1}{2}}$ be substituted, the value of the coal coefficient may represent more truly the difference in propulsive efficiency due to decrease of draught, and consequent emersion of propeller tips, but the index value is only approximate and varies with the dimensions and speed of the vessel under consideration. Mr. Edgecombe

I am grateful for the opportunity of adding my appreciation of the Paper, which will probably stimulate others to contribute similar data to the transactions of the Institution.

Mr. ARCHIBALD GILCHRIST (of Messrs. Barclay Curle and Co., Ltd): I think that Mr. Potts's Paper is an exceedingly interesting and instructive one, and deserves the serious attention and consideration of shipowners as well as shipbuilders and marine engineers. It is the common practice of engineers to look at efficiency of propelling plant only up to the point of the consumption of coal per unit of power developed; but it is my opinion that a study of the efficiency of the ship as a whole would repay the engineer as well as the shipowner. In general, the efficiency of a geared turbine improves as the designed power is approached, the consumption of coal per power unit decreasing till that point is reached. Pursuing the matter further, the speed of the vessel is also increased with the increased power, and the time taken for a voyage is consequently reduced, and it will be found that instead of saving coal when running at a low power the reverse will be the case. This I may illustrate by Table 9, which shows the actual conditions from Mr. Gilchrist

TABLE 9.—DOUBLE REDUCTION GEARED TURBINE STEAMER.

Table prepared (from the actual results of the above Vessel's Voyage No. 3) to show that the total consumption of coal remains practically the same at all speeds. A gain in time taken with the same expense for coal is obtained when running at or near the designed power of 4,000 shaft.

Per cent. of designed power	60 %	70 %	80 %	90 %	100 %
Corresponding shaft horse-power	2,400	2,800	3,200	3,600	4,000
Coal per S.H.P. hour from actual logs	2·32	2·04	1·81	1·658	1·59
Coal per I.H.P. hour assuming $\text{I.H.P.} = \frac{\text{S.H.P.}}{0{\cdot}9}$	2·09	1·84	1·63	1·49	1·43
Coal in tons per day	59·6	61·3	62·0	63·7	68·3

Voyage Out.—On the voyage out the power averaged 2,964 shaft while the speed maintained was 12·7 knots. Assuming that the power varies as the Speed^3 then the speed for the various percentages of power would be as follows:—

Speed	11·82	12·44	13·0	13·55	14·0
Hours taken to do 6,200 miles	524	497	476	457	442
Total tons of coal for voyage	1,300	1,270	1,230	1,210	1,260

Voyage Home.—On the voyage home the power averaged 3,509 shaft while the speed maintained was 12·32 knots. Assuming that the power varies as the speed^3, then the speed for the various percentages of power would be as follows:—

Speed	10·87	11·42	11·96	12·44	12·88
Hours taken to do 6,200 miles	570	542	518	497	482
Total tons of coal per voyage	1,420	1,390	1,340	1,320	1,375

voyage results of a double-reduction-geared turbine refrigerated vessel, and which shows a fair gain in coal consumption when the vessel is

TABLE 10.—VOYAGE COMPARISONS OF GEARED TURBINE STEAMERS.

Ship.	Total distance steamed.	Average speed per hour.	Revs. per min.	Slip per cent.	Daily Consumption.	Ash per cent.	Knots per ton Coal.	Tons per 1,000 knots.	Mean Draft.	Displacement.	Total Fuel used.	Class of Fuel.	Weather conditions.	$D^2, \times S^3$ Cons.	Dimensions of Vessel.	Type of Machinery.
A	37,015	11·89	81·0	9·72	53·3	17·9	5·32	188	23′ 9″	13,300	6,948	N.W. & Ind.	Fa/Mo	18,100	470×58×34′ 10″ and 0·74 Coeff.	S.R. geared turbines.
B	41,493	11·9	77·9	6·28	53·78	13·0	5·28	189	23′ 9″	13,400	7,850	Dur. & Ind.	Fa/Mo	17,600	470×58×34′ 10″ and 0·74 Coeff.	S.R. geared turbines.
C	16,409	12·0	75·61	13·1	56·59	11·0	5·1	196	26′ 7″	18,500	3,228	S.W.	Fa/Mo	21,400	518×64×38′ 4″ and 0·755 Coeff.	D.R. geared turbines.
D	17,047	11·36	75·99	15·8	57·0	12·3	4·78	209	26′ 10″	18,700	3·562	S.W.	Fa/Un	18,100	518×64×38′ 4″ and 0·755 Coeff.	D.R. geared turbines.
Mean C & D	16,728	11·68	75·8	14·5	56·8	11·65	4·94	202	26′ 8″	18,600	3,395	S.W.	Fa/Mo	19,750		
E	16,806	11·86	76·6	12·4	60·8	12·4	4·64	215	26′ 5″	17,850	3,626	S.W.	Mo/Un	18,800	500×64×37′ 0″ and 0·74 Coeff.	D.R. geared turbines.
F	17,951	11·82	75·81	12·17	60·0	10·85	4·72	212	25′ 6″	17,000	3,800	S.W.	Fa/Un	18,200	500×64×37′ 0″ and 0·74 Coeff.	D.R. geared turbines.
Mean E & F	17,378	11·84	76·2	12·3	60·4	11·62	4·68	213	25′ 11″	17,425	3,713	S.W.	Fa/Mo	18,500		

In above vessels working pressure is 200 lb. per square inch, except in vessel "A," where working pressure is 180 lb. per square inch.
All boilers are worked natural draught and without superheater.

running at her full power. Mr. Potts graphically illustrates the same point in Table 5, Vessel "C" Voyage 3 outwards. With engines developing 2,351 s.h.p. the coal consumption works out at 40·4 tons per day, quality Scotch, unscreened, very dirty. The same vessel Voyage 8 homeward, 2,834 s.h.p. 35·8 tons Scotch, good steaming, *i.e.*, for 9·64 days the coal consumption was at the rate of 35·8 tons per day on the homeward voyage against 11·518 days at 40·4 tons, in addition the cargo carried on the homeward voyage being 66 per cent. greater. I have taken these as dealing with the highest and lowest powers given on the Table, and the other figures on the Table more or less confirm it. Mr. Gilchrist

Dealing with the efficiency constant $\frac{D^{\frac{2}{3}} \times V^3}{\text{Coal per day}}$ I am generally in agreement with the Author, and think that it would pay shipowners to run their vessels at varying speeds until they had sufficient data to plot a coal curve, in order to arrive at the most suitable power for maximum efficiency where the quantity of coal consumed is a minimum over the voyage. The coal constant formulæ given do not appear to be a true efficiency indicator.

The thanks of the engineering community are due to Mr. Potts for again bringing forward the important part which auxiliaries play in the economical working of geared-turbine machinery, and to emphasize still further this point, the writer has before him at the present time the case of a high-class passenger vessel where the auxiliary consumption, including steam heating, cooking, etc., only amounts to about $16\frac{3}{4}$ per cent. of the steam used by the main engines. The total consumption for all purposes in this vessel burning oil, works out in the region of 1 lb. per s.h.p. hour for all purposes. In another vessel where lack of economy was apparent, after the introduction of water-meters in order to localize the inefficiency, it was found that the auxiliaries were using 44 per cent. of the total steam used by the main engines. After these auxiliaries were thoroughly overhauled and put in good working order, the consumption dropped to about 15 per cent. of the steam used by the main engines.

I was very interested to note the increase in revolutions in a vessel, as shown in Plate IX. due to painting the hull, as I am familiar with a case of our own in which we got a $\frac{3}{4}$ knot increase in the speed with practically the same power, which was apparently due to the repainting of the hull. The condition before painting appeared to be quite good, although the bloom was slightly off the paint, the draft condition on both trials being identical.

Mr. ANDREW HAMILTON, C.B.E., *Member*: I think we are indebted to Mr. Potts for a valuable contribution to the literature on the performances of turbine installations. The systematic measurement of water, both in connexion with main and auxiliary machinery, should be more widely encouraged, as it is the only true means of ascertaining Mr. Hamilton

Mr. Hamilton the efficiency of any one or more machines. When such measurements are periodically and systematically recorded, it is easy to check any change in the performance of the main or auxiliary machinery; such measurements being done, as suggested by Mr. Potts, through meters or through measuring tanks in connexion with the auxiliary condenser.

I agree with Mr. Potts that too close attention cannot be given to the efficient upkeep of all auxiliaries in and out of the engine-room when, with careful attention, so large a percentage of the total steam used can be saved.

With reference to Mr. Potts's remarks on coal-coefficient comparisons, it is very necessary, as he states, when comparing results by such coefficients that fairly similar cases be chosen with regard to speed, ship's length and displacement, as what may prove to be a good coefficient for one vessel may be looked upon to be quite an unfavourable coefficient for a vessel of different dimensions, etc.

There is no doubt that Mr. Potts has given the closest attention to the performances of marine-turbine installations, and I may say that I have had the pleasure of some intimate experience with him in connexion with vessels we have had to deal with. Table 10 gives results of the running of several vessels now sailing between the United Kingdom and the East, which may be of interest compared with the results given in the Paper.

AUTHOR'S REPLY.

The President asked whether there was any great benefit to be derived from reduced steam in the auxiliary service advocated by many superintending engineers. When the vessel "C" went into service, the auxiliary steam pressure was 150 lb.; at the end of eleven months, the pressure had been gradually reduced to 130 lb., and during this period the auxiliary steam consumption was reduced 20 per cent., possibly owing to the reduced pressure, but largely due to regulating the speed of the auxiliary engines, and maintaining them in a state of efficiency. At the present time, this vessel is running with an auxiliary steam pressure of 100 lb. Primarily, the auxiliary steam pressure was reduced so that the various engines could be kept under better control when steaming at the service power. The auxiliary engines were generally of too large a capacity for the service conditions, particularly the circulating pump and fan engine, the steam valves being only about a quarter of a turn open. The Author

Referring to Dr. Morrow's remarks regarding the magnitude of the leakage through the internal gland and the horizontal joint in the diaphragm between the exhaust end of the i.p. ahead and the exhaust end of the h.p. astern turbine, it has been the Author's practice to test for leakage all the main, and particularly the astern, valves before proceeding to sea—generally during a mooring trial—by pumping the main condensate to measuring tanks, observing that all the auxiliary exhaust valves to the main condenser, main steam, ahead and astern valves, were shut hard down, and maintaining 28 inches of vacuum throughout the system. Under these conditions the quantity of main condensate represents the steam admitted to the glands of the turbine plus any leakage that may occur. This procedure was carried out in the case of vessel "C", and the main condensate amounted to 500 lb. per hour. A further test was carried out to approximate the amount of steam passing the internal gland between the i.p. ahead and h.p. astern turbines at a pressure corresponding to full-power conditions. The eduction pipe connecting the i.p. turbine exhaust to the l.p. ahead was blanked off, steam was admitted through the h.p. manœuvring valve until a pressure of 5 lb. was maintained in the i.p. exhaust belt, and vacuum of 28 inches throughout the remainder of the system. The quantity of main condensate measured was 880 lb. per hour. The steam to glands, as previously measured, was 500 lb. per hour, the difference between the two experiments being 380 lb., which represents the leakage through the internal gland and joint of diaphragm between the ahead and astern turbines, equivalent to about 1 per cent. of the turbine steam. Regarding the boiler efficiency obtained on voyages Nos. 3 and 8, where the figures were 57·1 and 75·1 respectively, the coal used on voyage No. 3 was a mixed lot, obtained from four different collieries, of the unscreened variety and very dirty, necessitating cleaning four fires per watch, and

The Author

giving 18 per cent. of ash. In the case of voyage No. 8, where good steam coal was used, three fires per watch were cleaned, with 13½ per cent. ash, and the difference in the boiler efficiency would, no doubt, be partly accounted for by the increased percentage of ash, and by the loss of heat when cleaning four fires per watch.

With reference to Mr. Neill's remarks with regard to the drop in revolutions due to the state of the ship's hull, it may be mentioned that the weather conditions on trial No. 2 were adverse with a head wind, and on trial No. 1 the wind was slight at the slow speeds but increased at the high speeds with swell on the starboard quarter.

On trial No. 3 there was a slight N.N.E. swell. Generally, the mean results of trials Nos. 1 and 2 would compare favourably, as to weather conditions, with the measured-mile results of trial No. 3. Further, the state of the ship's hull on trial No. 3 was not conducive to the best speed and power results being obtained, as it had only received one coat of composition which was not thoroughly dry when the vessel proceeded on her trial.

In reply to Mr. Berry, the steam to auxiliary engines in vessel "C" was superheated; in the cases of vessels "S" and "T" saturated steam was supplied. During twelve months' running of vessel "C", as far as I am aware, the use of superheated steam has had no ill effect on the working parts of the auxiliary engines.

Replying to Mr. Butler's remarks regarding the difference in coal consumptions in Tables 4 and 5, these coal consumptions were taken over different periods of time; in Table 5 the quantity of coal given is that consumed over the steaming time as recorded in the engineers' log for the whole voyage. As mentioned in the Paper, the boiler-efficiency trials were carried out by consuming a definite quantity of coal from the cross bunker, and the figures in Table 4 give the duration of the trials and the quantity of fuel burned over that period.

The Author may have been fortunate in his experience with the water meters used on these vessels, but, as mentioned in the Paper, the engineers in the vessels under review took a keen interest in their work, and appreciated the value of having on board their vessels means whereby they could determine the amount of steam being generated. The water meters on vessel "C" were calibrated from time to time, and the figures given in the Paper are in accordance with these calibrations; the largest difference obtained between the different calibrations was 1½ per cent.

In reference to Mr. Butler's question regarding the variation in the boiler efficiency during voyages Nos. 5 and 8 of the vesel "C", it should be noted that this vessel was in the North Atlantic trade, and the boiler trials were carried out when the vessel was running westward at a ballast draft. It will be observed from the voyage results that the weather conditions obtaining during voyages Nos. 5 and 8 were very

much in favour of voyage No. 8, resulting in a steadier firing platform, which, as will be appreciated, is an important matter on a vessel in this condition. The same remarks apply regarding the weather conditions on voyage No. 3, and, further, the boiler efficiency was affected to a greater extent on account of using unscreened and dirty coal, as mentioned in the Author's reply to Dr. Morrow. In the Author's opinion, the varying conditions mentioned had more effect on the boiler efficiencies than the slight discrepancies shown by the calibration of the water meters. It may be of interest to mention that, during the water-measuring trials, the water was measured over the same period of time by means of tanks, water meters and integrating counters fitted on the feed pumps, and the maximum and minimum difference as measured by any of the methods was $1\frac{1}{2}$ per cent. The large difference in the average speed shown in voyages Nos. 1, 2 and 4 was, no doubt, partly due to the condition of the hull's surface, but in the case of voyage No. 4 the adverse weather conditions obtaining were of more account than the bottom fouling. The second painting of the hull of this vessel was carried out immediately preceding voyage No. 3 in November, 1921, and the third docking and painting took place after voyage No. 9 in November, 1922. The Author

In connexion with Mr. Edgecombe's remarks that the propulsive coefficient was only 46·5 per cent., it may be mentioned that during the trials the s.h.p. was measured by means of a Hopkinson-Thring torsion-meter fitted directly abaft the thrust shaft; also by a torsion-meter of the Denny-Edgecombe type fitted immediately forward of the propeller shaft, where the torque of the shaft is recorded on a diagram in one revolution. The torque obtained by the two different instruments corresponded very closely at a point in the revolution where the ship's permanent torsion-meter flashed across the scale. The difference in power compared with the recording instrument never varied more than $1\frac{1}{2}$ per cent., and the variation on the torque diagram, as obtained from the Denny-Edgecombe instrument, never exceeded $2\frac{1}{2}$ per cent. The low propulsive coefficient obtained on the measured-mile trial was, no doubt, partly due to the condition of the surface of the hull, and also owing to the true slip of the propeller being over 35 per cent. at the ship's speed of 13 knots per hour.

The Author is obliged to Mr. Andrew Hamilton for the valuable particulars which he has contributed to the discussion, and to Mr. Archibald Gilchrist for his interesting remarks regarding the steam consumption of auxiliary engines. In the case of vessel "C", the Author would anticipate an oil-fuel consumption of 0·8 lb. per shaft horse-power per hour in service. At the present time, further experiments are being made on vessel "C" which will, doubtless, throw more light on the steam consumption of the auxiliary engines, arrangements having been made whereby several of the units can be measured separately over a long period.

The Author

Since reading this paper, certain evaporator trials have been made, where the vapour and the steam to coils were measured independently over an extended period; these showed that about 2 lb. of steam were required to make 1 lb. of fresh water, a large increase on the figure usually advanced by the auxiliary makers

The Author is indebted to the members of the Institution for the manner in which his paper was received, and thanks those gentlemen who have contributed to the discussion.

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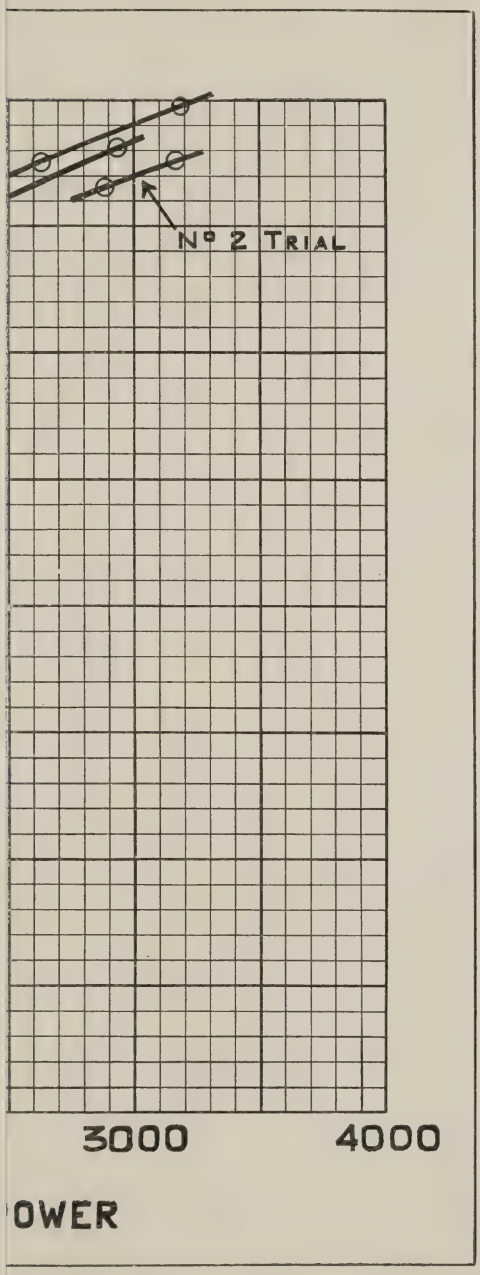

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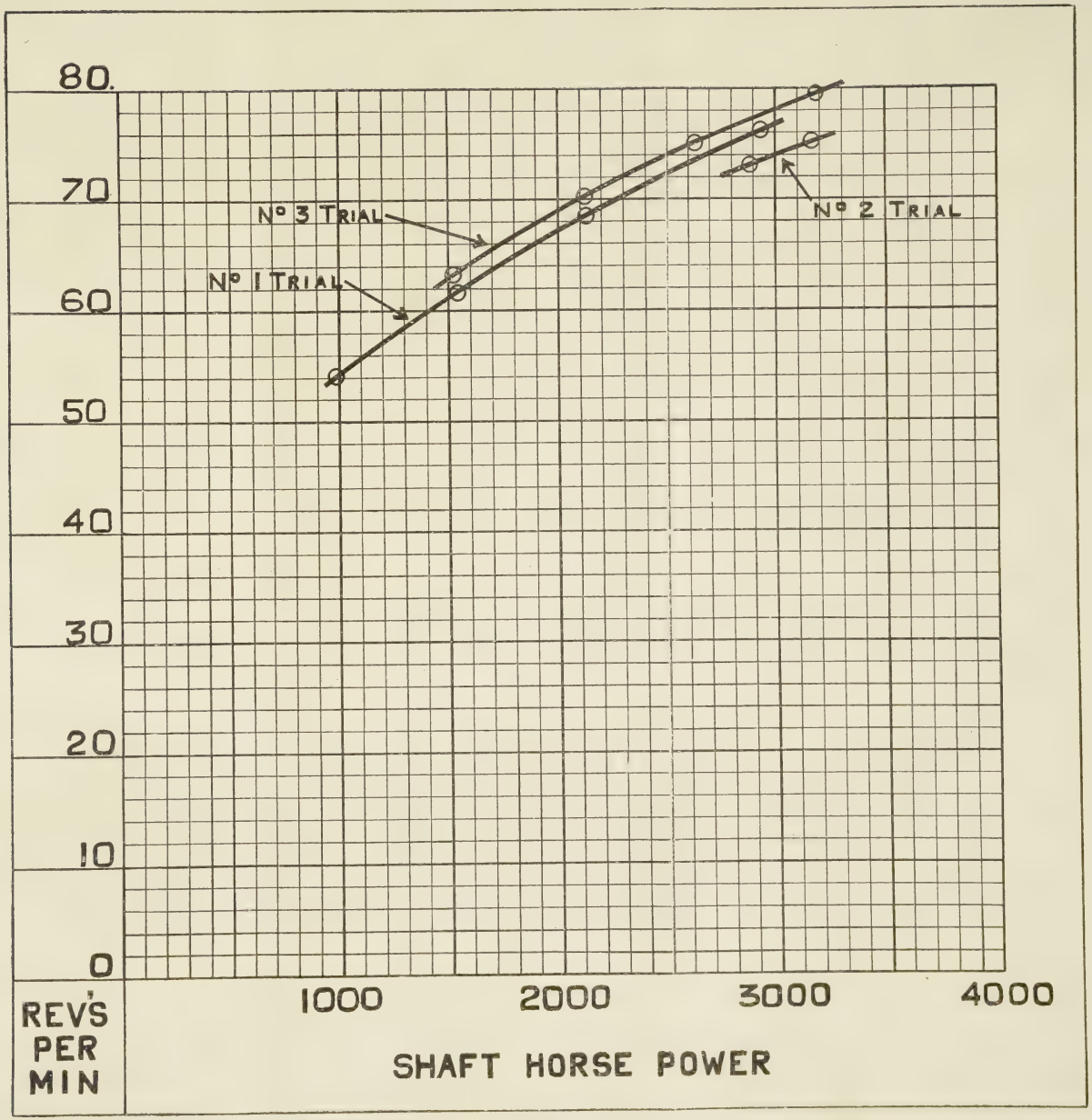
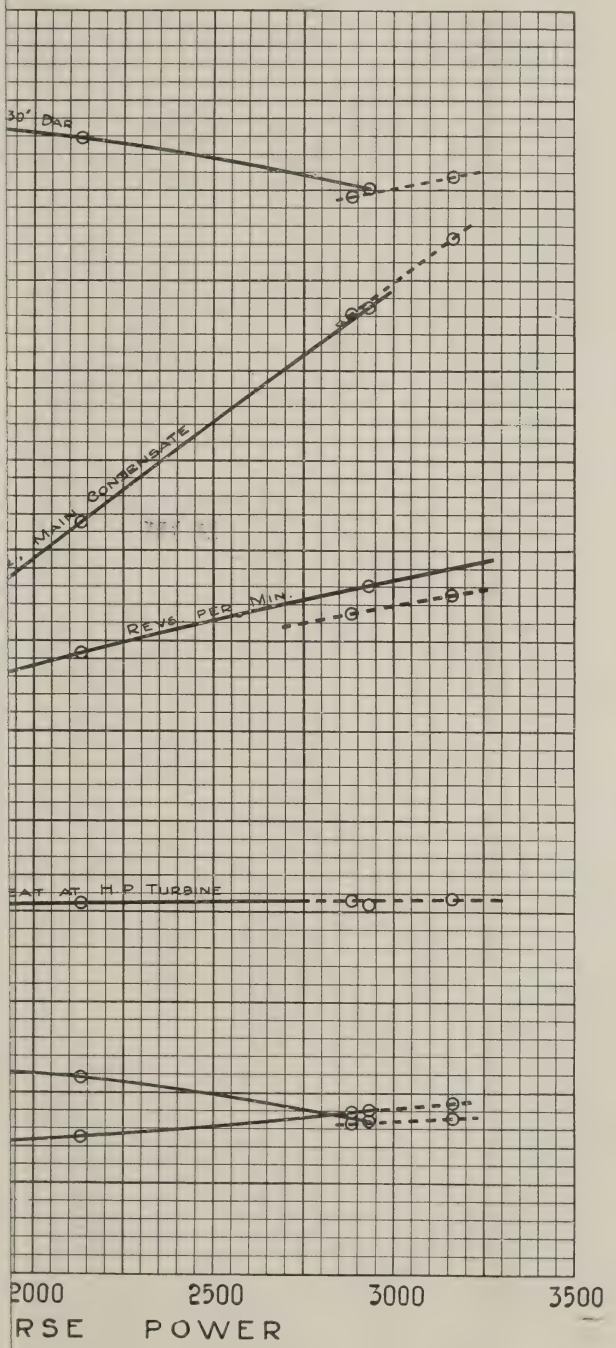
Illustrating Mr. T. G. Potts's Paper "Notes on the Economical Performance of Turbine Installations in Merchant Ships." (See page 543.)



SHOWING VARIATION IN REVOLUTIONS DUE TO STATE OF SHIP'S HULL.

Performance of Turbine Installations
543)

Nos. 1 & 2



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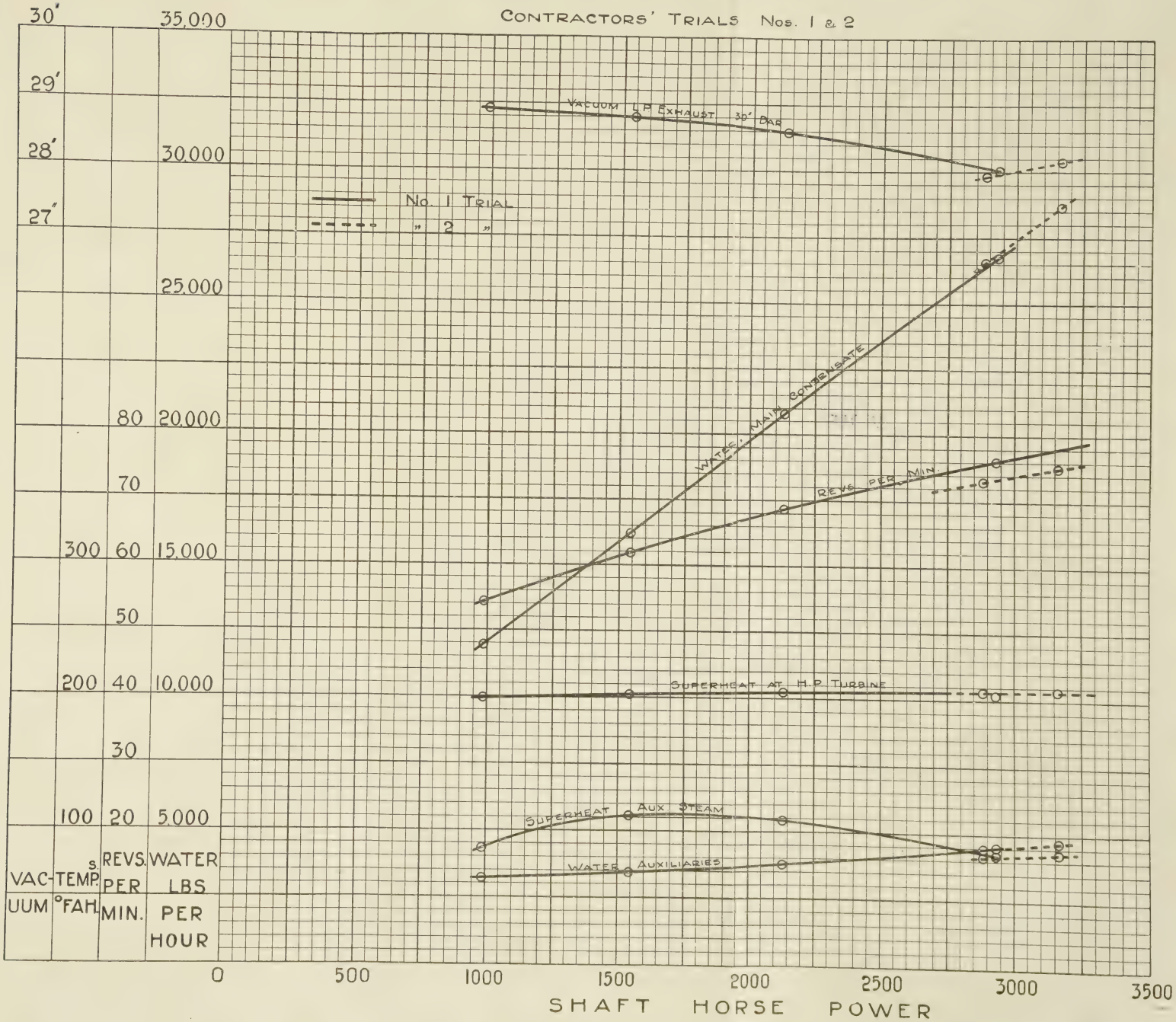

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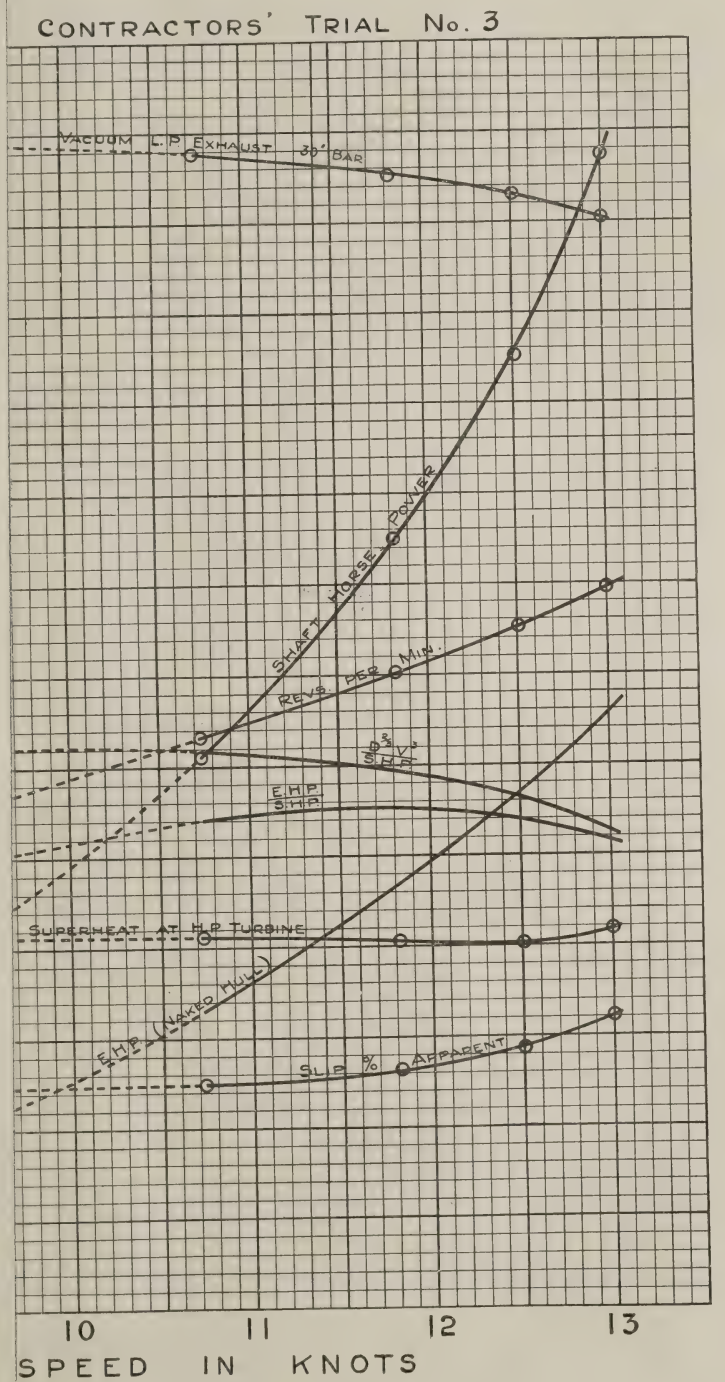

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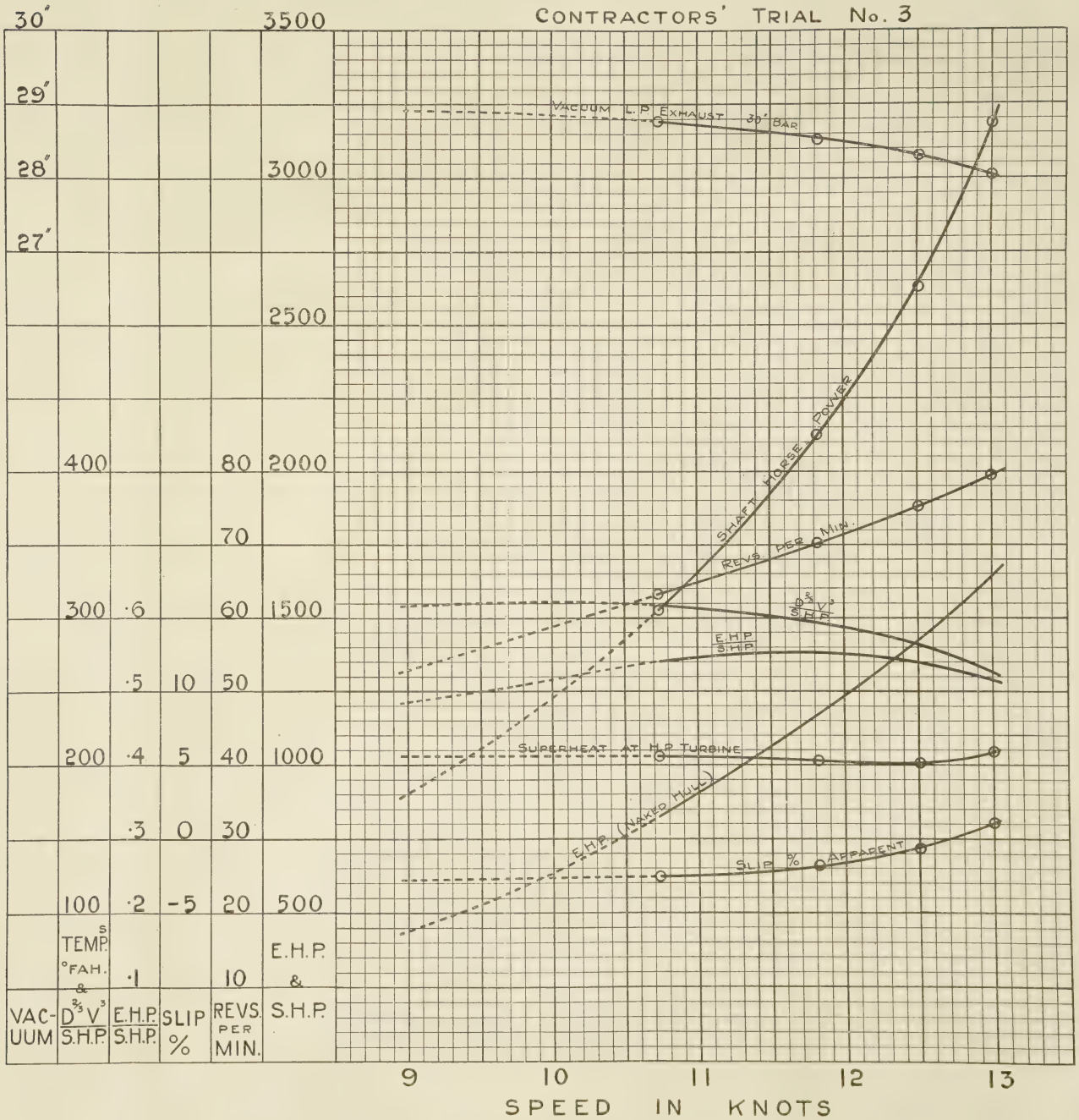

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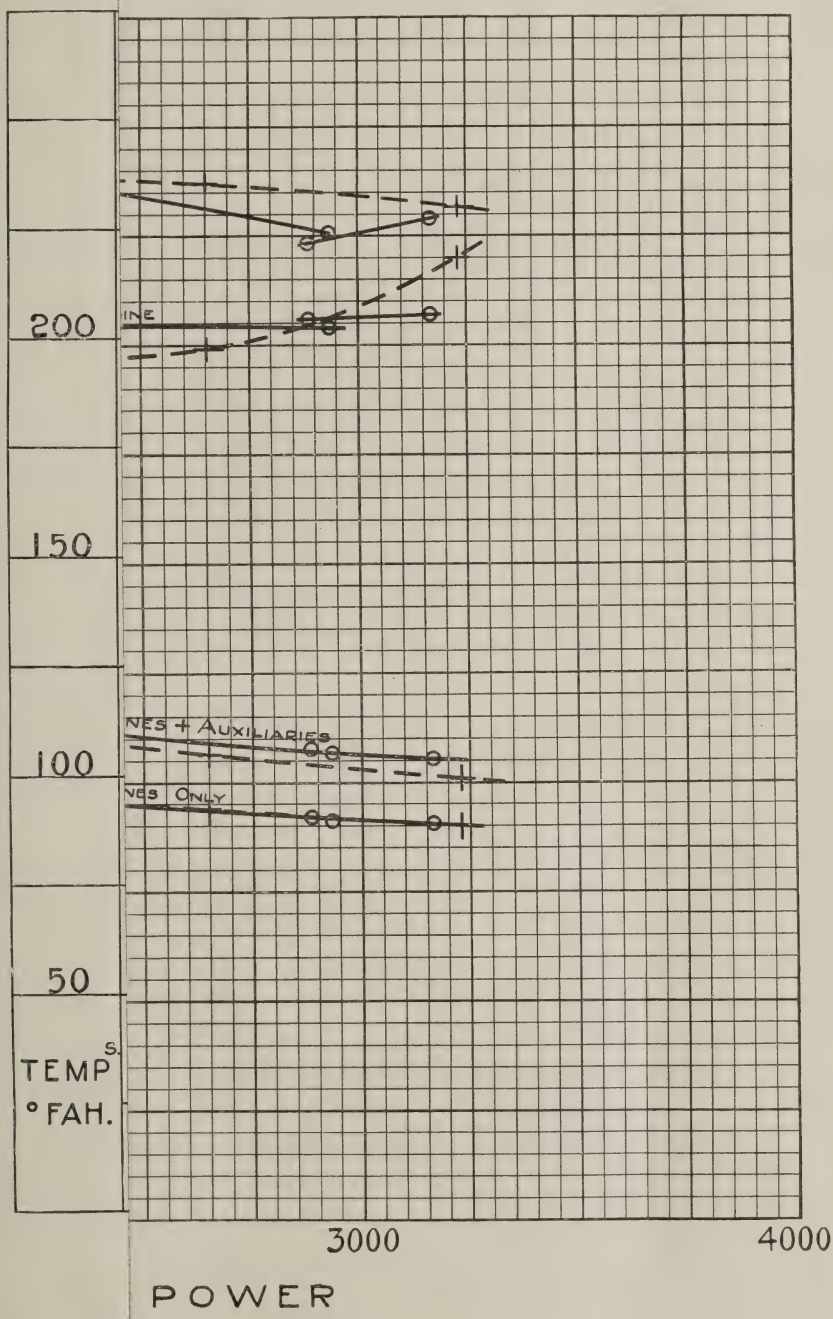

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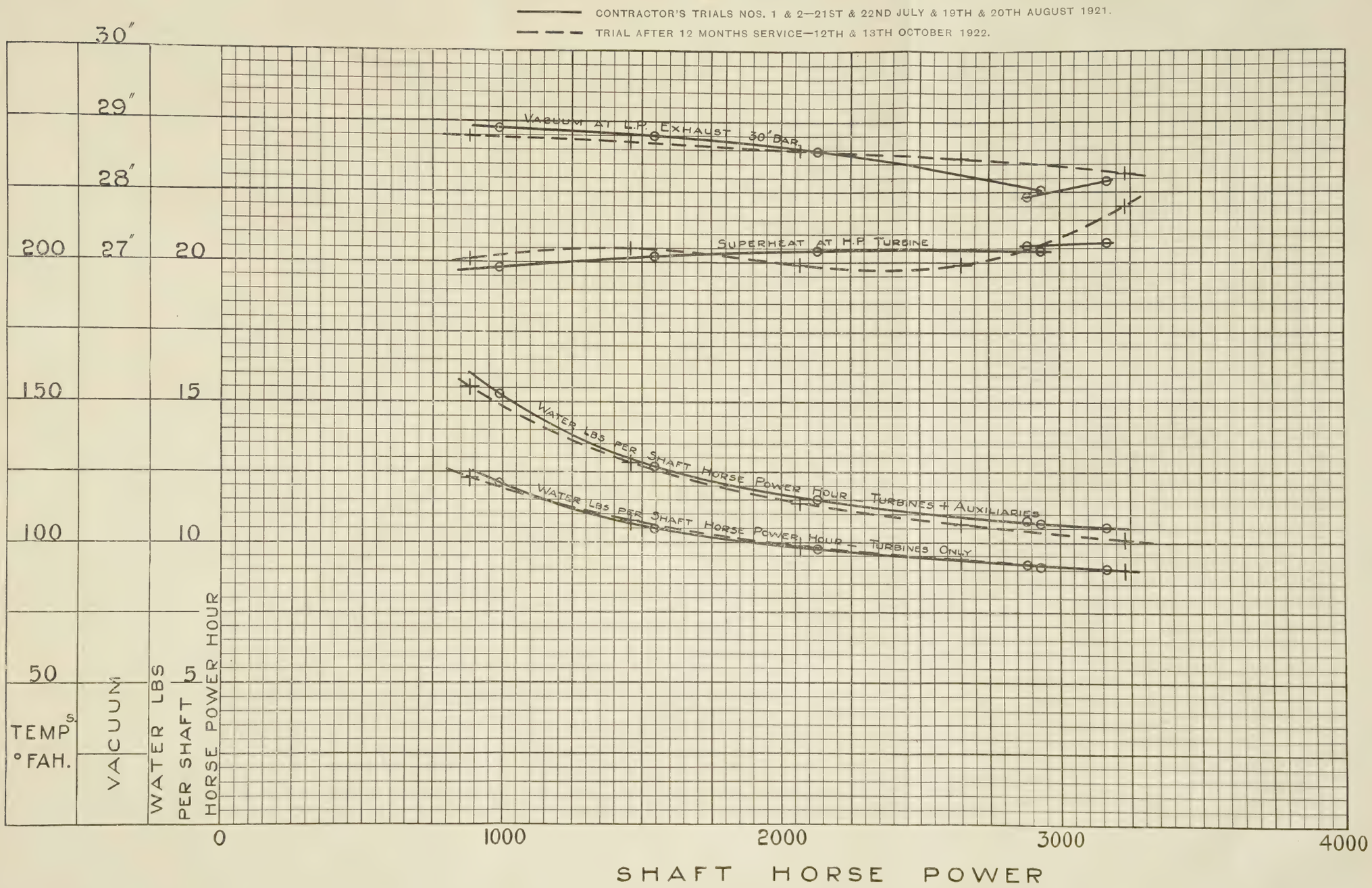


POWDERED COAL AS FUEL.

By J. T. DUNN, D.Sc., F.I.C.

[Read in Newcastle-upon-Tyne on the 6th April, 1923]

The mechanical power needed for industrial operations is, in this country at least, practically all derived from the energy of combustion of fuel, whether by raising steam pressure by burning the fuel under a boiler, or by using it in an internal-combustion engine. Fuel is also largely used for the production of heat to be used in other ways than in power generation; whether for the conduct of chemical operations which will only take place at a high temperature, such as the manufacture of glass or the reduction of metals from their ores, or for the modification of the physical properties of substances produced by high temperature, as exemplified in the operations of the foundry or the forging of heated steel.

All ordinary fuels contain carbon or hydrogen or both; the carbon burning to carbon dioxide, and the hydrogen burning to water, give out heat, definite in quantity for a definite amount of a given fuel. The technical problem in any given instance is so to construct the plant and manage the operation that that definite amount shall be, first obtained, next utilized as nearly completely and with as little loss as possible. The industrial problem is to work out the technical problem, or so to modify its working out, as to achieve the conduct of the operation as cheaply, or at as low a cost, as possible.

When we speak of economy of fuel, therefore, we may have in mind what may be called technical economy, that is to say, high efficiency—utilization of a high percentage of the total heat theoretically obtainable from the fuel; or we may have in mind what may be called industrial economy—the production of the substance or article with which we are concerned at as low a cost, for fuel and for the labour and appliances needed to burn it suitably, as possible. These two aspects of economy are not necessarily achieved in the same way or by the same means. One fuel may be so much lower in cost than another, for example, that though when we use it we burn it inefficiently and wastefully, it may yet effect its object at a lower cost than the other, though that can be used with a high efficiency. Again, to burn any fuel efficiently is clearly more economical in the technical sense than to burn it inefficiently and wastefully; but if, to use it efficiently, we have to make different arrangements

from those obtaining in its ordinary inefficient use, the industrial economy will depend on whether or not the saving of fuel due to the technical economy more than balances the extra cost of the new arrangements made to burn it. If we can make nine tons of fuel do the work formerly done by ten, we have effected an economy, and from the point of view of the conservation of our coal supplies the change is all to the good; but if in order to do so we have to incur a total cost of a hundred pounds where we formerly paid ninety, the change will not be industrially practicable.

The only natural fuel which we in this country possess is coal. We need not consider peat: though our stores are considerable in quantity, the difficulties in the way of its utilization have hitherto prevented its use on any great scale as fuel. Scottish shales are very restricted in quantity. Many parts of the world have stores of oil; but oil in this country is much more costly than coal, and as the probability is that the world's oil supplies will be exhausted much earlier than those of coal, the tendency is for the cost of oil relatively to coal continually to increase; so that only in certain directions do the greater conveniences and the higher efficiency of oil-burning appliances outweigh the higher cost of oil compared with that of coal.

In considering the combustion of coal, two points meet us at once as detracting from efficiency, or as requiring adequate arrangements to secure it. The first is that coal is not entirely a combustible substance, but always contains mineral matter, which on burning the coal is left in the form of ash. Now, not only is this ash useless from the fuel point of view—not only does it lessen the value of a given weight of coal in the proportion in which it exists in it, but it tends to prevent the burning of the combustible portion and renders it incomplete, so that the more ash a coal contains, the more unburnt combustible is the clinker likely to contain; and further, the removal and disposal of the clinker demands more labour and becomes a larger item of cost as the ash increases. For this reason many large coal deposits are useless—not that they would not yield heat by being burnt, but because the imperfection of the combustion and the troubles of dealing with the clinker make their use industrially unprofitable. The second point is that when coal is burnt in a furnace it does not burn away as a whole continuously, but gives off, as it heats up, large volumes of gaseous or vaporous combustible matter, and leaves behind a solid residue of carbon or coke; and the supply of air for combustion has to be so regulated that it can deal with the gaseous as well as with the solid products. If the supply of air be short, much of the combustible matter goes off as smoke, involving both loss and nuisance; and to insure that this shall not be the case, a considerable amount of surplus air has in practice to be supplied, involving waste through the heat which it carries away with it after its passage through the furnace. You are probably more familiar than I with the many mechanical stoking devices which have been introduced

to deal with this difficulty, and get rid of the irregularities of hand stoking.

It is claimed that both of these difficulties are very much lessened, if not got rid of, and that many other advantages result, if, instead of the coal being burnt on grates in the solid form, it is finely pulverized, and injected into the furnace in the form of a stream of dust, mixed with the air which is necessary for its combustion. The industrial use of powdered coal has been for many years now an accomplished fact in the cement industry, where its use is almost universal; and its success in this industry naturally led to attempts to use it in other directions. These for a long time were not very promising; but continued experiment and inquiry into the conditions requisite for success led to such satisfactory results being obtained that to-day large numbers of installations exist using solely powdered coal—chiefly in various branches of the metallurgical industry, but also in the production of power by steam raising. This is especially the case in the United States, and also in France. Much less progress seems to have been made in this country, but installations are in use here, and the rapid increase in the use of powdered fuel abroad seems a valid reason for drawing attention to it. In 1919 there was used in the United States about 11 million tons of coal in the form of powdered fuel, of which about 6 million was used in the cement industry, 2 million in the iron and steel industry, 1½ million in the copper industry, and about 200,000 for the generation of power. This last figure is probably greatly exceeded now.

A typical installation for the burning of powdered coal includes arrangements for (*a*) breaking, (*b*) drying, (*c*) pulverizing the coal, (*d*) transporting it to the furnace, (*e*) feeding it through the burner. The systems in use divide themselves sharply into two: (1) The unit system, in which the coal is pulverized at the furnace, so that there is a separate pulverizer for each furnace; (2) The multiple system, in which the coal is pulverized at a centre and transported as a powder to any number of furnaces that may be included in the installation.

Considering first a unit installation, typified by the turbo-pulverizer of the Powdered Fuel Plant Co., the coal, if not already small enough in size, is broken down by some form of crusher to about three-quarters of an inch cube, and is delivered either by some mechanical arrangement or by hand to the hopper of the pulverizer. From the hopper it falls into the distributor, which feeds it to the pulverizer continuously. The amount of coal which the distributor allows to pass to the pulverizer determines the output of the machine and the amount of coal burnt at the furnace. The pulverizer consists of a series of discs fitted with loaded blades, which rotate on a common shaft in a casing divided into as many compartments as there are discs. The coal travels in succession through all the compartments, subjected in each to the breaking action of the blades, and passes through a fan on the same shaft as the discs into the pipe leading to the burner. The fan draws a continuous current of

air through the pulverizer, which, of course, mixes with the coal dust drawn through. There are two air inlets, separately adjustable, one at the distributor end, the other between the pulverizer and the fan. The pulverizer is erected close to the furnace, so that there is only a short length of pipe to traverse to the burner.

The unit system is very compact, and is very useful where the installation is small and would not justify the capital outlay for a separate pulverizing installation and transport system; but the cost of course increases almost proportionately with the number of furnaces to be fed. Another advantage is that unless the raw coal be very wet it can be used in the pulverizer without preliminary drying, for there are no great lengths of piping, no bins nor feed-screws, and hence there is little likelihood of clogging of wet coal occurring. The turbo-pulverizer does not pulverize the coal so finely as some of the pulverizers in other systems (in one instance 70 per cent. passed a $\frac{1}{200}$-inch sieve and 88 per cent. a $\frac{1}{100}$ inch) but the fineness is sufficient to ensure practically complete combustion of any ordinary coal. Nearly all the air needed for the combustion of the coal is drawn in by the fan and mixed with the coal before it reaches the burner; and it is possible to secure smokeless combustion with a great variety of coals.

In the multiple systems the first step, as in the unit system, is the crushing of the coal. Before pulverization, however, the coal is in all these systems dried to such an extent as to leave not more than 0·5 to 1·0 per cent. of moisture in it. Not only is finer pulverization secured by using dry coal, but the risk of stoppages, caused by the clogging of the powder and its sticking or hanging in the pipes, conveyors, valves, or storage bins, is very greatly lessened. In nearly all the systems the drier consists of a long inclined cylinder, which rotates slowly on its axis, and down which the coal slowly travels. The cylinder is heated on the outside by a small furnace near one end, and the flue gases, after travelling along the outside of the cylinder and becoming cooled to a temperature at which it is safe to bring them into contact with the coal, then travel through the inside, and carry with them the water vapour which the coal gives off.

From the drier the coal goes to the pulverizer. Much stress has been laid on the need for extremely fine pulverization. Theoretically, of course, the further comminution proceeds, the more intimate can the mixture be made of the coal and the air which is needed to burn it. An inch cube of coal needs for complete combustion $6\frac{3}{4}$ cubic feet of air, or a cube of about $22\frac{1}{2}$ inches in the side; and as combustion can occur only where the coal and the air are in contact, all this air must come into contact with the surface of the cube, which begins by being 6 square inches and gradually diminishes, before the coal can be completely burnt. But if the coal be divided into smaller cubes of $\frac{1}{100}$-inch in the side, there will be a million of these, their aggregate surface will be 600 square inches instead of 6, and if they be uniformly distributed through the $22\frac{1}{2}$-inch

cube of air, each will be at the centre of a little cube of its own, less than a quarter of an inch in the side: clearly the conditions here are more favourable to rapid and complete combustion than in the former case, even were the coal entirely combustible, still more so when it contains a percentage of ash. It is usually considered that the coal should be so finely powdered that 95 per cent. of it will pass a sieve with 100 meshes to the inch and 80 to 85 per cent. will pass one with 200; this represents considerably finer material than we have just been considering. Generally, the finer the grinding the more rapid can the combustion be made; but also, of course, the more power will there be needed for the grinding. In some cases rapid combustion and a short, hot flame may be desired; but in others a slower and longer flame may be preferable, and in such cases probably rather less fine grinding may be sufficient.

For the transport of the powdered coal from the pulverizer to the furnace burner various methods are adopted. The earliest was the use of screw conveyors, which for short distances are quite efficient, and are not liable to get out of order. The coal from the pulverizer is raised by a current of air or by an elevator into a collecting bin at a suitable height, from the bottom of which it is discharged into the screw conveyor leading to the bin at the furnace.

More recently pneumatic transport has been developed, in which the coal is made to flow through pipes by air pressure. The finely pulverized coal particles entrain, or "adsorb" on their surfaces, a considerable amount of air (a given volume of pulverized coal contains only 46 per cent. of actual coal, and 54 per cent. of air by volume), and have almost the same freedom of flow as a liquid.

In the Fuller-Kinyon system, which is a combination of mechanical and pneumatic transport, the fuel from the storage bin falls into the "Fuller-Kinyon" pump, a rapidly rotating helix which drives it forward into piping. As the fuel leaves the pump and enters the piping it meets a small jet of compressed air, which, mixing with it, renders it still more nearly fluid and facilitates its passage through the piping. Propulsion to distances as great as 1,200 feet has been effected by this method, and a considerable vertical rise is possible. Clearly a suitable arrangement of pipes and valves would make it possible to feed the furnace bins of any number of furnaces by means of a single pump of sufficient capacity.

Perhaps the simplest of the pneumatic transport methods is that of the Quigley Co., in which the coal is fed from the storage bin into cylindrical "blowing tanks," from which it is driven by air pressure on its surface through pipes of 4-inches diameter to the bins or hoppers at the furnaces. An operator at the blowing tanks can send any desired quantity of fuel to any furnace bin in the system. More than a ton of coal per minute has been transported 600 feet through a 4-inch pipe by this system, with a pressure of 15 lb. per square inch.

The Holbeck system differs entirely from the others, in that the

furnaces are supplied, not from separate bins, but by branch pipes from a central system of piping in which a mixture of fuel and air circulates continuously. Fuel from the storage bin is delivered by a feed screw driven by a variable speed motor into the intake of a high pressure blower, and passes, along with the air taken in by the blower, into the circulating main, which carries it ultimately into a cyclone separator, from which the air passes away and the fuel is delivered to the storage bin again. Where necessary, branch pipes lead from the circulating main to the burners; and when the valve of one of these is opened, and the issue of fuel causes a drop of pressure in the main, it operates mechanism which increases the speed of the feed screw and the supply of air to the blower, so that the quantity of fuel and air in the main is kept constant.

Each system has, of course, advantages and disadvantages. The Fuller-Kinyon system, for example, is said to need less power than the others for the transport of a given quantity of fuel over a given distance. The Holbeck system does away with the cost and the inconvenience of the bulky storage bins at the furnaces; but on the other hand it has, like the unit installations, no reserves, and any stoppage or failure in the main circulating system at once shuts down the whole of the furnaces dependent on it. In this as in other things, no one method is always the best; and the most suitable for any given set of circumstances will be determined by study of those circumstances themselves.

Where storage bins exist at the furnaces, the coal is usually taken from them by a screw conveyor, which drops it into the tube of the burner. Here it is mixed with the "primary air," usually injected into it from behind, which carries it along into the furnace. This mixture does not usually contain the full amount of air needed for complete combustion, and "secondary air" is introduced, sometimes by suction on the injector principle, sometimes from a separate blower. Usually this is introduced concentrically with the air and fuel mixture: in some burners the mixture comes through the central pipe, the secondary air through the surrounding annulus, in others the situation is reversed; and a great variety of devices exists to secure proper mixture of fuel and air, easy regulation of proportions, and appropriate velocity. In the early installations troubles and failures frequently occurred through too high speed of the entering mixture; and the aim now is to secure as low an entering velocity as is consistent with the quantity of fuel to be burnt, and as will prevent any flame from carrying back into the supply tube.

In the Holbeck installations the fuel arrives at the burner already mixed with a large proportion of air, and with a speed determined by the pressure in the circulating main and the length of the branch pipe; and it goes direct into the burner tube, a sufficient amount of secondary air being added from a separate blowing system. In unit installations of the aero-pulverizer type, too, the mixture is blown direct from the pulverizer into the furnace; and the regulation of the amount of air

drawn into the fan allows of practically the whole of the air needed for combustion being mixed with the fuel as primary air, if that be found desirable.

Advantages: (1) Better combustion: for reasons already given the complete combustion of powdered fuel can be secured, with very little more than the theoretically needed quantity of air, so that the highest possible temperature of combustion may be realized, and the least possible loss of heat from the installation, due to the escape of the products of combustion at a temperature above that of the air, will occur. The combustion is almost the same as that of a gaseous fuel. Further, the combustion is more nearly complete than that of coal burnt in the ordinary way, so that not nearly so much of the fuel remains unburnt in the ashes. The ashes from a powdered-fuel installation are almost free from unburnt carbon, whilst the clinker from an ordinary coal fed furnace contains even under the most favourable conditions a very sensible amount.

(2) Easier regulation: the speed of entry of the fuel, and the proportions of fuel and primary or secondary air, can be varied readily so as to obtain the best conditions of combustion, and utilize to the best advantage the heat evolved, according as a long and gentle, or a short and intensely hot flame is desired. The same ease of regulation makes it possible too, whilst not introducing excess air to any extent, to secure rapidly completeness of combustion and freedom from smoke, even if the coal should vary very considerably in quality.

(3) Rapidity of starting and stopping: the full flame is at once available on starting, and the heating up of a furnace starting from cold is much more rapid than with solid fuel. Combustion is stopped the moment the supply of fuel is shut off, and no waste of further fuel takes place. There are no losses through the banking of fires, and variation of the rate of combustion to meet varying loads can be readily effected.

(4) Use of inferior fuels: the lessened interference of the ash with the process of combustion, when the fuel is finely powdered, allows of complete combustion even when the percentage of ash is very high; and many fuels can be completely and economically burnt in pulverized form, which it would be impossible to utilize in the ordinary way. In this possibility of using inferior fuels, powdered coal holds out promise of great economy in the future.

(5) Saving in labour: in large installations especially there is a marked saving in labour effected by the use of powdered fuel.

Disadvantages: (1) Ash: in solid-fuel furnaces, the ash remains on the hearth; but with powdered fuel it is first formed as a fine suspended dust, which will tend, of course, to settle out, but from its fineness will largely be carried on with the products of combustion and be liable to deposit on boiler tubes, on the objects to be heated in the furnace, or in regenerator chambers if there be any such. Further, formed as they

are in the hot flame, these particles of ash, if the ash be readily fusible, may be melted, and will then coalesce and fall on the furnace hearth as a melted and more or less viscous slag. In early installations, much trouble arose from this cause; but through modifications of the form and size of the combustion chambers, and through lessening of the velocity of the entering fuel and air, the trouble has now been very greatly reduced. In most installations, it is found that a large proportion of the ash is carried off through the stack; and systematic collection and removal of the remainder is found not to be a difficult matter. The melting point of the ash of the coal used is in this connexion an important datum; and a coal is usually sought for, the ash of which has either a very high melting point, so that it may all be either carried off or collected in the form of dust, or a very low melting point, so that it may form a free running and easily removable slag. The problem of ash is an individual one, depending on the nature of the furnace in which it is proposed to use powdered fuel.

(2) Refractory materials: here again, early experience showed that the high temperature of the flame was very destructive of furnace linings, the bricks melting down under the combined influence of the high temperature and the mechanically abrasive and chemically destructive action of the ash particles which scoured along them; but the same improvements, and the device of introducing a little excess air along the walls of the combustion chamber, have greatly mitigated the evil. The production of refractory linings of higher melting point and greater chemical resistance would, however, be of great assistance in powdered-fuel-fired, as in oil-fired installations.

(3) Explosion: much has been made of the fact that a mixture of coal dust and air in certain proportions is explosive. Explosions have occurred in powdered-fuel plants, but investigation has shown that these explosions have in all cases arisen from causes which were preventible, and not inherent in the nature of the operations carried on in the plant; and the possibility of explosion in a well-designed plant which is properly inspected and kept in order is very remote. The mixture of coal and air which is forced through the pipes in any of the pneumatic transport systems is too rich in coal (or too poor in air) to be explosive, and can only be rendered so by additional air entering the system, as for example, by the secondary air driving back into the system through some stoppage of the primary blower and obstruction at the burner mouth; and even then explosion can only occur if some spark or other means of ignition can be carried in along with the secondary air. With perfect tightness of pipes and containers, thorough ventilation and cleanliness, the avoidance of naked lights, and care in operating the drier, the risk of explosion is reduced to an almost negligible quantity.

Costs: It is very difficult to obtain trustworthy figures of the cost of powdered-fuel equipment which will hold good in this country; nearly all of those which are pubished are of American origin, and not many of

those afford a basis for comparison with the cost of working similar furnaces on solid fuel. Herington, in his book on Powdered Fuel, gives an estimate of costs of powdered fuel as compared with actual costs of side-feed stokers in a Canadian installation of 8 boilers of a total horse-power of 4,900. The daily requirements were put as follows :—

	Solid Fuel. Dollars.	Powdered Fuel. Dollars.
Coal	1440·00	1356·00
Labour...	402·72	171·12
Power	0·98	25·31
Repairs and supplies ...	3·30	7·57
	1847·00	1560·00

and showed a daily saving of 287 dollars, equivalent to 88,970 dollars per annum.

The complete cost of the necessary powdered-coal equipment, including building, motors, changes in boiler furnaces, and erection, was put at 138,000 dollars; and assuming fixed charges as being 15 per cent. of this total cost yearly, or 20,700 dollars, the net annual saving would be 88,970 less 20,700, or 68,270 dollars, nearly 50 per cent. on the capital outlay.

The following figures, kindly supplied by Messrs. Edgar Allen & Co., Ltd., of Sheffield, afford a comparison between two reheating furnaces, one worked with powdered coal, the other with solid fuel, over a period of four months of intermittent work, and over a week :—

	Solid Fuel.	Powdered Fuel.
Turns worked	65¾	65¾
Steel heated	104 tons	163 tons
Slack or coal used	71 ,,	64 ,,
,, per ton of steel	13·75 cwt.	7·85 cwt.
Cost of fuel	£126 2s. 0d.	£55 15s. 7d.
Cost of motor	...	£32 17s. 6d.
Cost of wages	...	£19 4s. 0d.
Total cost per ton of steel ...	£1 15s. 6d.	13s. 3d.
Turns worked	6	6
Steel heated	9 tons, 9 cwt.	16 tons, 10 cwt., 3 qrs.
Fuel used...	7 tons	5 tons, 12 cwt., 2 qrs.
,, per ton of steel	14·8 cwt.	6·8 cwt.
Cost of fuel	£13 0s 0d.	£5 4s. 0d.
Cost of motor	...	£2 12s. 0d.
Cost of wages	...	£1 6s. 2d.
Total cost per ton of steel ..	£1 7s. 0d.	11s. 1d.

In a district where marine work is so important, reference, even though it must be brief, should be made to so-called "Colloidal Fuel." Pulverized coal is very bulky, and the installation of pulverizing and transporting machinery in the confined space available on shipboard is impracticable. During the war, a band of American experimentalists under the leadership of Mr. Lindon W. Bates found that when finely powdered

coal dust is mixed with fuel oil, the addition to the mixture of small proportions of certain substances called "fixateurs" (the most usual being a lime-rosin soap or grease) greatly retarded the rate—already very slow, in consequence of the fineness of the particles—at which the coal settled out from the mixture; so much so that even after six weeks, only about 5 per cent. of the solid particles had settled out. It was found practicable to mix as much as 30 per cent. of coal with 70 per cent. of oil, and still to produce a fluid which could be handled, in the matter not only of storage, but of passage through pipes and valves, in exactly the same way and with the same facility as fuel oil. The advantages of this fuel consist, first in its lower cost as compared with fuel oil, and second in its higher calorific value per given volume, for whilst, weight for weight, the coal in the mixture is of lower calorific value than the fuel oil, yet its higher specific gravity raises the specific gravity of the mixture so considerably that whilst in a certain sample, for example, the calorific value of the fuel oil alone was 148,000 B.Th.U. per gallon, that of the mixture containing 30 per cent. of coal was 162,000 B.Th.U. per gallon.

The U.S. Admiralty placed at the disposal of the experimenters a small vessel, which was worked with colloidal fuel from April to July, 1918, with entirely satisfactory results. Very little work seems to have been done in this country on colloidal fuel; but a proposal which indicates the possibility of running oil-burning installations on a fuel containing only 70 per cent. or less of oil, and of conserving the other 30 per cent. certainly seems worthy of attention.

DISCUSSION.

Sir Charles A. Parsons

THE CHAIRMAN, The Hon. Sir Charles A. Parsons, K.C.B., F.R.S., *Past President* and *Hon. Fellow* (introducing Dr. Dunn): It is a great pleasure to me to call upon Dr. Dunn to read his Paper on what I think may be a very important subject to us engineers, and possibly to marine engineers in this district, that is, the use of powdered coal as fuel. Instead of burning the coal on the grate, it is projected like atomized oil into the furnace. It is a subject which has been very largely developed in America, as Dr. Dunn will tell us, and upon which Government reports have been published by the Research Council in London.

After Dr. Dunn had read his paper, THE CHAIRMAN said: I am sure we are all very much indebted to Dr. Dunn for laying this subject before us so very lucidly as he has done this evening; it is one of which, as he says, we have perhaps taken too little notice in this country. I am sorry that Dr. Dunn thinks this method of burning coal is scarcely applicable to marine purposes at the present time, and I can only hope that he is not right in that conclusion. I hope some way may be found by which coal may be successfully pulverized on board ship at the boiler front (no storage of fine coal being involved) and then immediately injected into the furnace: I cannot see why there should

not be enough space provided on board ship to do this, in view of the fact that it would mean that fewer hands would be wanted, thus allowing of greater space in the boiler room. The reduced number of hands to look after the powdered coal and the general conditions would then be very much like what they are with an oil-burning installation on board ship. The advantages are very obvious, of course, especially in view of the saving in the B.T.U.'s obtained per ton by burning coal in this manner. Sir Charles A. Parsons

I should like to ask Dr. Dunn whether there is any limit to the fineness which must not be exceeded, because I remember many years ago that when charcoal was put into a barrel with some iron balls and rotated for a long time, the whole thing took fire upon the barrel being opened. There was apparently spontaneous combustion due to the very exceptional fineness of the charcoal. Now, would powdered coal be likely to behave in a similar way?

It is very interesting to realize how this very fine coal can be transported very much like a fluid. Taking the figures given in the middle of the Paper, where it is stated that the powdered coal was transported for 600 feet with 15 lb. pressure per square inch, and assuming the mixture of coal and air is in mechanical contact with the pipe and is one-half the specific gravity of water, then the co-efficient of friction must be less than one-tenth, otherwise it would not travel long. Perhaps the mixture will be rather less than one-half the weight of water?

Dr. DUNN: No, I should think rather more.

The CHAIRMAN: Some of us have seen the "Brownian" movements of very fine particles in a fluid: they never go to the bottom. Perhaps some of these particles of coal act in the same way and never settle and so never touch the tube; it is rather an interesting question.

Prof. G. GERALD STONEY, F.R.S., *Fellow*: In ordinary boilers at a power station an efficiency is obtained of 84 or 85 per cent. with ordinary mechanical stokers; therefore, with powdered fuel there is very little margin, and any gain would be by the use of cheaper coal than is used at the present time. In that direction I do not think there is much prospect for powdered fuel. There is, however, a great prospect for powdered fuel at sea. Nobody has made a success of mechanical stokers, but I believe the powdered-fuel stoker might be made a success of at sea, but of course there will still be the difficulty of trimming. Prof. G. Gerald Stoney

I have great hopes that somebody will experiment in the use of it in internal-combustion engines. I myself believe it is possible. People will probably say that the dust problem would grind away the piston rings and cylinders and so forth; but look at the very hard gritty dust which a motor-car takes in! I suppose probably there is somewhere between 20 and 30 lb. of air used in a motor-car engine for every lb. of petrol used, and the quantity of dust carried in on a dusty road by that air must be very large indeed. If you burn out the carbon deposit in the cylinder of a motor-car you will find that there is a lot of deposit

Prof. G. Gerald Stoney

that is dust, and if a motor-car engine can work under such conditions, why should not an internal-combustion engine be able to work with powdered fuel?

Dr. H. Peile

DR. H. PEILE: Powdered coal as fuel is a subject of very great interest. Dr. Dunn referred to the use of inferior coals in a powdered state. This is an important point as there is no doubt that if such coal can be used in the way suggested it will be a great benefit to this district where a large amount of coal is left in the pits owing to its inferior quality. The great difficulty to be overcome in the use of powdered coal is the ash nuisance. In one case of which I know, where small coal was burned with forced draught, the nuisance from the ash was intolerable. It found its way into the houses, filled up the spouts and proved such a general nuisance that the use of this type of furnace was discontinued. The question of the ash nuisance is one that requires great consideration as it is necessary to find some way out of the difficulty.

During the war I had experience of working a Bettington boiler, the fuel used being very finely ground coal and coke ballast. This raised steam very satisfactorily but with several mechanical difficulties. Very great trouble was found in regulating the amount of air necessary to burn the fuel, and we were again confronted with the ash difficulty. The fine coal and ashes were blown over into the enonomizers, causing a great deal of trouble and nuisance. The ash from the boilers was very free from carbon, the combustion was very good the gases from the back of the boiler showed $12\frac{1}{2}$ per cent. of CO_2. The main difficulty of this type of boiler was to get a lining that would stand the high temperature.

Can Dr. Dunn tell us—

(1) How much excess of air is used in burning powdered fuel as compared with hand firing practice?

(2) What is the quantity of carbon he expects to have left in the ash?

(3) How much fuel is required in drying the coal for use?

One of the purposes for which powdered coal might be used with advantage is in power stations where a sudden demand for power takes place and steam must be generated very rapidly. I believe this has already been tried in this district.

Eng.-Commdr. C. J. Hawkes

ENG.-COMMDR. C. J. HAWKES, R.N. (ret.), *Member of Council*: Having in view the fact that the only natural fuel which we possess is coal it is perhaps remarkable that so little progress has been made in this country with powdered coal for steam raising. There is evidence, however, that more attention is being paid to the question at the present time and the Author's interesting paper is most opportune. The use of powdered coal has several distinct advantages when applied to land boilers but there would appear to be many difficulties to be overcome before it can be applied to marine boilers. A self-contained powdered

coal installation includes crushers, pulverizers, etc., and from the figures quoted from Herington's book on "Powdered Fuel" it seems doubtful whether, in the case of small plant, there is likely to be any "industrial" economy derived from its adoption, as compared with ordinary mechanical stokers, in spite of a possible slight increase in "technical" economy. Eng.-Commdr. Hawkes

The rapidity of combustion depends on the size of the particles of powdered fuel and on the points at which the secondary air is introduced. Also, it is necessary to provide a furnace of sufficient dimensions to ensure that the temperature of the ash shall fall below its fusion point before it comes into contact with the heating surfaces or furnace brickwork of the boiler. The Author states on page 576 that the present aim is to secure as low an entering velocity of the mixture of powdered fuel and air as is consistent with the quantity of fuel to be burned and as will prevent the flame from carrying back into the supply tube. Under these conditions, and with finely pulverized coal, what is the minimum length or depth of furnace that should be provided and is it preferable to introduce the fuel horizontally or vertically? The need for providing a certain length or depth of furnace would appear to make it difficult to use powdered fuel in an existing boiler designed for hand firing without a complete re-design of the furnace. Perhaps the Author could also give some information as to the capacity of the largest burner at present in use, the points limiting the capacity of an individual burner, the maximum quantity of fuel that has been consumed smokelessly per hour per cubic foot of combustion chamber volume and the minimum quantity of excess air required.

The risk of explosion is perhaps remote with a carefully designed system. In the unit system, described on page 573, it is stated that nearly all the air needed for combustion is mixed with the coal before it reaches the burner. The chances of a back flash would be greater in this system but there is only a short length of pipe between the pulverizer and the burner. It is understood that, with the air-flotation system of distribution it has been found that the mixture of powdered coal and air is not uniform throughout and it is necessary to introduce a supply of secondary air at low pressure. From the point of view of risk of explosion it would also seem preferable to limit the quantity of primary air to that sufficient for the introduction of the powdered fuel and to introduce the additional air required to complete the combustion as secondary air.

Colloidal fuel possesses the advantages of oil fuel but it is presumed that it would have some of the disadvantages attached to the use of powdered coal, although to a smaller extent. The preparation of colloidal fuel involves the use of crushers, pulverizers, &c., and arrangements for mixing the powdered coal with the oil fuel. The cost of pulverizing and mixing will depend on the output of the plant but at first sight it does not appear that there is likely to be any appreciable saving, over the use of oil fuel only, with a mixture containing 30 per

Eng. Commdr. Hawkes

cent. of coal and 70 per cent. of oil—and if the proportion of coal is materially increased there will probably be difficulty in handling the mixture. In the American experiments it is stated that the addition to the mixture of small proportions of certain substances, retarded the rate at which the coal settled out from the mixture. Some years ago an experiment was carried out with carborundum in lubricating oil. It was found that coarse grains of carborundum readily settled to the bottom of the test tube whereas fine grains remained in suspension. A swinging movement given to a test tube containing fine particles in suspension caused particles to adhere to the sides of the tube. If a mixture of powdered coal and oil fuel were carried in a ship's bunker would it be expected that the rate of separation given on page 580 would be increased due to the motion of the ship? Could the Author state the type of boiler fitted in the U.S Admiralty vessel which was worked with colloidal fuel, whether any modifications were made to the furnaces and, if so, the nature and extent of the modifications.

During the last few years it has been frequently suggested that colloidal fuel should be used in internal-combustion engines but there does not appear to be any outstanding advantage in doing this, and the disadvantages, viz., the accumulation of ash in the cylinders and its possible abrasive action, &c., have been occasionally overlooked.

Mr Crowe

Mr. T. A. CROWE, *Graduate*: The subject of pulverized coal is, or should be, one of great interest to engineers in this country, as it deals with the most efficient method of burning our one great natural fuel supply, namely, coal. In spite of this it seems a pity to burn any coal in its raw state, as long as there are valuable markets for the bye-products obtained by the distillation of coal. In this connexion I should like to ask Dr. Dunn whether any of the pulverizers with which he is acquainted can deal efficiently with coke, or the so-called semi-coke or coalite obtained by the low-temperature distillation of coal, or if the power required or the wear and tear are excessive.

Degree of Pulverization.—As Dr. Dunn says in dealing with the degree of pulverization, a high degree of pulverization means a short hot flame. I think this is undoubtedly the best flame for boiler firing, as it means that combustion will finish soon after leaving the nozzle, and there will be less danger of the flame striking the brickwork at the back of a water-tube boiler, and melting it and depositing slag upon it. It would also appear to be possible to reduce the furnace volume when using a higher degree of pulverization. I should like to know if Dr. Dunn could give us any comparative figures of the furnace volume required per lb. of coal burnt per hour in the unit system and in the multiple system. The makers of the apparatus generally say that 1 cubic foot per lb. of coal per hour is sufficient, but in practice about double this volume is found to be more satisfactory. When this is compared with the 8-10 lb. of oil burnt per cubic foot of furnace volume, it will be seen how enormous the combustion chamber must be for

pulverized fuel. I understand that some experiments have recently been carried out with burning pulverized coal in Lancashire boilers, and Mr. Crowe

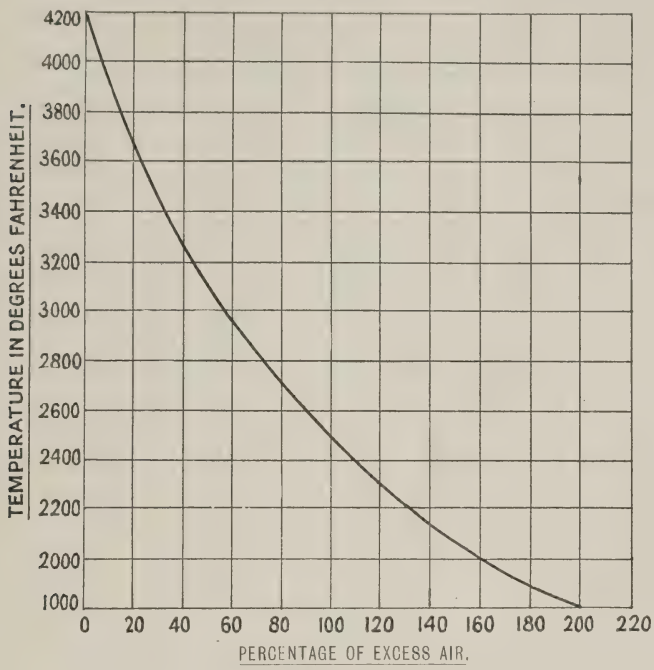


FIG. 1.—CURVE SHOWING TEMPERATURES OBTAINED FROM COMBUSTION OF COAL IN AIR.

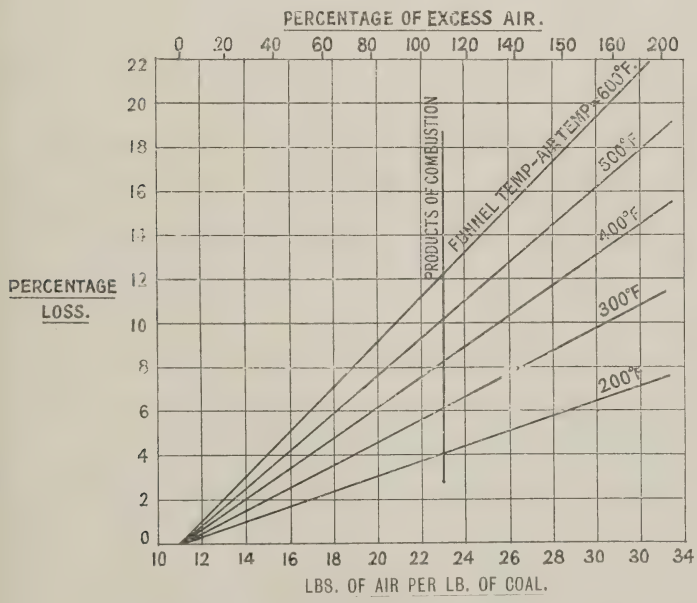


FIG. 2.—HEAT LOST BY EXCESS AIR AND PRODUCTS OF COMBUSTION.

would like to know whether Dr. Dunn can give us any of the results obtained. In one pulverized-coal installation that I have seen, I

Mr Crowe

noticed a lot of slag sticking to the lower rows of tubes; would Diamond blowers, or compressed-air blowers be sufficient to remove this deposit?

I should like to know approximately how long the loaded blades last in a turbo-pulverizer, when in continuous service.

Dr. Dunn mentions the question of the speed of injection of the mixture. In the early installations, when burning anthracite or a coal containing less than 20 per cent. of volatile matter, it was found necessary to inject the mixture vertically downward, so that the rising flame would heat and ignite the down-coming mixture. I should like to know if this is still necessary or if the speed of the mixture can be kept below the rate of propagation of the flame.

I am rather sorry that Dr. Dunn has not given us any examples of the efficiencies obtained with pulverized coal and colloidal fuel. I thought the following example might be of interest to members of the Institution :—

TEST OF EDGEMOOR WATER TUBE BOILER ON PULVERIZED COAL AT THE LAKESIDE POWER PLANT, MILWAUKEE.

Duration of test	47 hrs. 50 mins.
Boiler heating surface (sq. ft.)	13057
Combustion chamber volume (cub. ft.)	9057
Steam pressure (lb./sq. in. G.)	266·7
Temperature of superheated steam (° Fahr.)	596
Temperature of feed entering boiler (° Fahr.)	213·5
Temperature of feed entering economizer (° Fahr.)	130·5
Gas temperature leaving economizer (° Fahr.)	268
Percentage of CO_2 by volume	13·6
Coal burnt per hour (lb.)	9292
Water evaporated / hr. (lb.)	79124
Heat transmission / sq. ft. heating surface / hr. (B.Th.U.) ...	6840
Higher calorific value of coal (B.Th.U.)	11580
Efficiency of boiler and superheater	82·9 %
,, ,, ,, ,, ,, and economizer	89·0 %
,, based on lower calorific value of coal	90·5 %

Costs.—I should like to ask Dr. Dunn how it is that there is no cost for labour in the case of the hand-fired reheating furnace. It would appear that the labour cost would be higher with the hand-fired, than with the pulverized-coal-fired furnace.

Figures 1 and 2, I think, show the reason why pulverized coal gives such high boiler efficiencies. Fig. 1 shows the flame temperatures obtained by the combustion of coal with various amounts of excess air, and shows how the flame temperatures vary from about 2,600° Fahr. with hand firing, to about 3,600° Fahr. with pulverized coal. When it is remembered that the heat transmission varies approximately as the fourth power of the absolute temperature, it will be seen how rapid the heat transmission must be in the case of pulverized coal.

Figure 2 shows the heat carried away by the excess air and by the products of combustion. If the difference between the air inlet temperature and the funnel temperature is 400° Fahr. it will be seen that the

percentage of heat carried away by the excess air falls from 8 per cent. in the case of hand firing to about 2 per cent. in the case of pulverized coal. Mr Crowe

Mr. E. ANGUS LEYBOURNE (of the Powdered Fuel Plant Company): I do feel that the unit system is the one that will make the biggest headway in the future. It has the advantage of low initial cost, no storage of coal in powdered form is necessary, and existing plants can be adapted to this method of firing at a minimum cost. Mr. Leybourne

With the turbo-pulverizer the correct quantity of air is added to each particle for the purpose of combustion, and, owing to the fact that air is added during the process of pulverization, a very intimate mixture is formed. This machine was developed first in France where economical reasons demanded the burning of low-grade fuels.

I was to-day at a colliery where a Lancashire boiler has been started up on powdered coal but I am unable to say yet what results have been obtained. However, it started quite satisfactorily and ran well for a short period, and I have every reason to believe that this will be a great success. I certainly think there is a new era ahead for powdered coal as applied to the Lancashire boiler.

The question of the application of powdered coal to marine work has been raised and while I think that this will ultimately take place, there are so many applications for land type boilers and furnaces, that I consider that it will be applied to these first and after engineers have found the advantages which accrue, its adoption on ships will naturally follow.

The ash question is not so serious as has been suggested, and generally speaking the ash can be got out in clinker form, or as slag, according to how one constructs the combustion chamber. I have not found that any undue wear takes place in the brickwork; in fact I have seen ordinary fire-bricks stand by for lengthy periods when working with powdered coal. In some cases it is desirable to slag the ash, and this system has been adopted by The Lambton & Hetton Collieries, Ltd., with a view to making the furnace self-operating and the slag is run out into a water conveyor. By this means a coating of slag is maintained on the walls of the furnace which acts as an insulator and protects the brickwork.

A question has been asked with regard to the length of the life of the blades. This, of course, is a variable quantity depending on the class of fuel pulverized, and with average coal the blades might have to be replaced three times a year.

VOTE OF THANKS.

Upon the motion of the Chairman, The Hon. Sir Charles A. Parsons, a vote of thanks was unanimously accorded to Dr. Dunn for his paper.

CORRESPONDENCE.

Mr. Hunt

MR. N. H. HUNT, *Member*: I should like to congratulate the Author on the very clear way in which he has placed the problem of powdered fuel before the Institution. I was present at some tests made on a vertical boiler of the Bettington type in a power station at Johannesburg, South Africa in 1912. There were some features which I noticed during those tests which seemed to be rather crude and I imagine from the Author's paper, that rapid strides must have been made since that time.

The points to which I refer particularly are:—

(1) The method of starting up the flame. On the tests, a piece of oily waste was ignited and held on the nozzle until the powdered fuel had caught fire. I presume other methods now obtain.

(2) The flame which came from the vertical nozzle, about 5 inches in diameter, seemed to impinge on some tubes more than others and consequently some trouble was experienced due to unequal expansion and pitting of tubes also occurred. I should like to know how that flame is evenly distributed over the tubes in modern equipments and whether the shape of the nozzles is always circular.

(3) With a unit system it is possible to regulate the speed of the motor that drives the fan and pulverizer and consequently to secure a range of steam evaporation. I should like to know what variation can be obtained in present-day practice and more particularly the maximum and minimum velocities of the mixture through the nozzle. Presumably the minimum velocity is just greater than that at which a firing back into the nozzle would occur.

(4) The particular purpose for which boilers using powdered fuel seem to be installed, particularly in power stations, is to deal with the peak loads for which they are eminently suited. It would no doubt be possible to fire a Lancashire boiler with powdered fuel, and in this case I should like to know what arrangements would be made for the "banking" period, as presumably it would not be advisable to raise steam in less than four hours with this type of burner which would be necessary if the boiler could not be "banked." If the pulverizer and fan were shut down, is there much difficulty in starting up again due to powder being trapped in the pipes on shutting down, or are arrangements made so that the large coal supply is shut off before the pulverizer is stopped?

(5) When the ash escapes from the flue as dust it is certain to be a nuisance in a congested area, or even to workshops near the boiler house. What arrangements are made to deal with ash which escapes from the flues as dust? Could the Cotterel electric process be used effectively for this purpose?

(6) Lastly, I should like to know what is the lowest calorific value of a coal that could be used effectively in a powdered-fuel-fired boiler, as it seems to be along this line that powdered fuel will make most progress.

Mr. JOHN W. JACKSON (of The Lambton & Hetton Coal Co., Ltd.): There is no doubt whatever, that powdered fuel has not been given the amount of consideration in this district that it demands. There are, as Dr. Dunn states, many positions where it can be used very economically indeed, it being much easier and cleaner to handle than many other forms of fuel. As for the matter of capital cost, this system of combustion is extremely attractive and compares very favourably with any existing system. It has proved much more flexible than the ordinary furnace arrangements in being able to deal economically with lower grades of fuel, while it deals with higher-grade coal as satisfactorily as most other systems. Mr. Jackson

In reliability, it has proved to be at least equal to any of the existing systems and very superior to most, and in maintenance charges it again compares most favourably with existing systems.

Mr. A. W. REICHWALD, *Associate*: I should like to join in the thanks which no doubt have been extended to Dr. Dunn for giving in a clear, concise and well balanced form a mass of most interesting information on the subject of pulverized coal as fuel. Signs are abundant that this method of burning coal is rapidly developing in this country and I should like to see us overtake American and Continental users of this system in its technical development. There is, for instance, the all-important problem of burning COKE dust in the same way as powdered coal. Enormous quantities of this material are available in a very even size of half an inch and downwards, containing about 12 per cent. ash and of 10 to 12,000 calories, the principal difficulty being to find machinery which will stand up to the work of pulverizing coke dust without excessive wear and tear. Mr. Reichwald

As regards the paper itself, Dr. Dunn shows under the heading of "Disadvantages" that although inferior coals may be burned by this system, the presence of ash is a decided disadvantage, this being especially so where dust-firing is applied to metallurgical processes, such as the refining of copper. In this connexion one is led to suggest that many of the difficulties due to the presence of ash that are mentioned in the Paper, may be overcome by the application of Froth Flotation processes to the fuel prior to its use. By means of the flotation method all free shale is removed and only inherent ash left. Unfortunately very few data are available regarding the burning of flotation products in dust-firing installations, chiefly because at this stage of the application of dust firing in England the governing factor has been cheap fuel rather than special efficiency; but it is probable that attention will be directed towards flotation in the future and for the following reasons:—

There is obviously a great practical difference between free ash in the form of shale and structural, *i.e.*, fixed, ash in coal. The latter is to a great extent composed of the remnants of the cell walls of the original vegetable matter from which the coal was derived; it is practically non-fusible and by virtue of its impalpability settles with extreme slowness,

Mr. Reichwald

being thus very readily carried away in any upcast or draught of air. Shale particles, on the other hand, settle comparatively easily and are often fusible.

In other words it is probable that the difficulties due to ash in dust firing may be attributed to free ash or shale rather than to inherent ash.

It is clear that, when a product containing only inherent ash is burned, a very large proportion of the impalpable residual ash will be carried through the stack, thus reducing to a minimum difficulties caused by slagging of walls and deposits on boiler tubes; whereas in the case of a product containing comparatively large quantities of free ash, these difficulties tend towards a maximum.

AUTHOR'S REPLY.

The Author

With Sir Charles Parsons, I should hope that powdered coal may be found adaptable to marine work. Looking at the question as a chemist, I am not able to make very fruitful suggestions on points of engineering; but it would seem as though some form of unit system might be devised for marine work, in which the coal would be shipped and stored after passing through the breaker, ready to go into the hopper of the pulverizer. As to the question of spontaneous combustion, I do not think that the fineness of grinding would sensibly affect that. I fancy that the charcoal spoken of by Sir Charles must have been at a pretty high temperature when it was opened to the air. The friction of the mixture passing through the pipes is probably not that of coal at all, as Sir Charles himself suggests, but that of the coating of adsorbed air which is very much less than that of coal.

The difficulty of ash in internal-combustion engines would probably be much greater than that of dust in motor-car engines, as, unless the coal were very carefully picked, there would be very much more of it; further, at the high temperature of the explosion, the likelihood of fusion would be great, and much more of it would remain in the cylinder, instead of leaving as dust in the exhaust gases.

In good powdered coal practice 30 per cent. of excess air appears to be easily attainable, and in some cases 20 per cent. has been reached, still with complete combustion. The ash, where it leaves as dust, is practically all mineral matter, with no unburnt carbon at all. I have no figures for the actual carbon in ash recovered from the boilers, but from some of the published trials I should calculate it at between one and two per cent. The heat needed for drying the coal obviously depends on the amount of moisture to be driven off, and varies between three quarters of a pound and, say, two and a half pounds per hundred pounds of coal dried.

I am not able to answer from personal experience some of Commander Hawkes's questions; but the boilers at the Lakeside Station of the Milwaukee Electric Railway Co. have the fuel chamber 22 feet deep below the arch, for a vertical introduction of the fuel and air, and the flame descends to within 3 feet of the bottom. At present, it seems necessary

for complete combustion of powdered coal to have half a cubic foot of combustion space per lb. of coal burnt per hour. I do not know what are the dimensions of the largest single burner made, but they certainly are made to deliver at least 2,000 lb. of coal per hour. The Author

As to colloidal fuel, in the U.S. trials the mixing was done on shore and the fuel charged into the ship in the same way as oil; the saving in bulk as compared with oil was thought to be a considerable advantage. The rate of separation given was that actually found in use on shipboard. The boilers were of the Normand type, and the feed and burner system the usual navy standard. No changes were made in the burners. An additional two-inch pipe was run from the bottom of the fuel storage tank to the oil-pump suction, and a reversing flame combustion arch installed at the rear of the boiler furnace. These were all the alterations to the ordinary oil-burning arrangements.

The question of the advantage or otherwise of carbonizing all coal to obtain the products of distillation, and burning none in its raw state, referred to by Mr. Crowe, is after all largely one of markets and demand; and in the case of ashy coals especially the possibility or otherwise of using the highly ashy coke produced is the most important factor in making a decision. I should doubt whether any of the pulverizers on the market could deal with hard coke save at great expense for wear and tear; but Messrs. Edgar Allen and Co. have, I know, used coalite in the powdered condition in their furnace, without finding excessive wear in the pulverizers.

The combustion chamber is arranged, whether with vertical or inclined introduction of the fuel, so that combustion may be complete before the gases impinge on the boiler tubes; in this way the uneven heating mentioned by Mr. Hunt is avoided. The speed of the motor in the unit system can be varied so as to vary the rate of delivery of the fuel. There are very conflicting accounts as to the inconvenience caused by ash escaping from the chimney shaft. If the shaft be high, the ash is spread over such a wide area before it falls that no inconvenience is felt; but there are reports of trouble arising from the deposition of the ash. In any case the ash is clean and not greasy, and is easily removed. No doubt electrical precipitation could be applied to get over any such difficulty; it would be a question of cost, very largely. It would be difficult to say what would be the lowest calorific value available in coal for use in powdered form, but coals containing over 30 per cent. of ash have been successfully used.

The removal of ash, whether by washing or by froth flotation, referred to by Mr. Reichwald, is of course a very desirable thing, and coal with low ash presents just as great advantages over coal with more, if it is to be used in powdered form, as it does in the ordinary method of use. Many of the coals high in ash, however, have their ash very finely disseminated through them and difficult of removal; and it is in regard to these poor coals in particular that pulverization is of such great value, making them usable, while they are not usable as fuel in bulk.

POSSIBILITIES OF INCREASED EFFICIENCY IN RAILWAY LOCOMOTIVES.

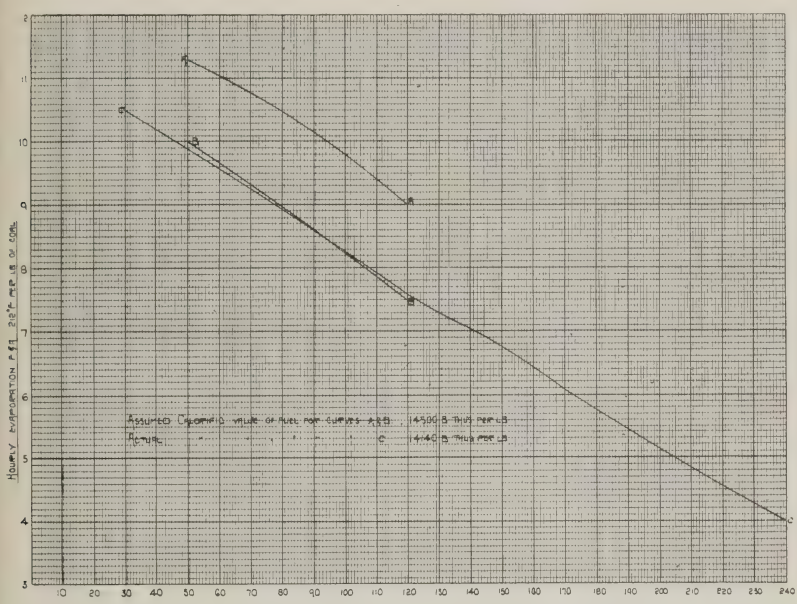
By T. GRIME, *Assoc. Member.*

[Read in Newcastle-upon-Tyne on the 20th April, 1923.]

The regime of the steam locomotive as the almost universal prime mover for railway operation is well over a century old. In this period there have been many developments in engineering, notable among which may be cited the introduction of the electric motor, the steam turbine, and the internal-combustion engine. The steam locomotive has up to the present maintained its position against its rivals, in spite of the fact that its efficiency is admittedly low when considered as a heat engine. The object of this paper is to consider the possibilities of increasing the steam locomotive's efficiency, and to compare the results it gives with those which might reasonably be expected from other systems of rail traction. There is a tendency in certain quarters to overrate the question of thermal efficiency. Were this the only consideration there is no question that the internal-combustion engine would hold the field exclusively. The real basis is commercial efficiency which takes into consideration first cost, maintenance and depreciation charges, reliability, and adaptability to the continually varying operating conditions met with in railway service. The steam locomotive admirably fulfils its purpose, and in the opinion of the Author there is, in the majority of cases, practically nothing to be gained either in the direction of increased efficiency or improved operation, by any change in the type of prime mover employed.

The Steam Locomotive.—The standard machine for railway operation for ordinary conditions in modern practice may be said to be the simple steam locomotive. The modern saturated-steam engine is in its essentials practically an enlarged edition of the locomotive of 1850, with the exception of improvements in detail, and it is very doubtful if any great improvement in its thermal efficiency has taken place since that date. Neglecting compounding which will be considered later, the most important step towards improved thermal efficiency has been the introduction of superheating, which took place about 1909, since which date it has made headway to such an extent that practically every locomotive now built to cover any distance is equipped with superheating apparatus. The value of the superheater in increasing both thermal and operating efficiency in long distance runs, is indisputable. For

shorter journeys or where stops are very frequent the value of superheating is by no means so great. Every stop made by a superheated engine involves a fall of temperature of the header and elements, especially when the engine is standing for any length of time. This reduction of temperature is not regained until after the engine has been running for some distance, so that the effect of frequent stops is to cause a lower mean temperature of superheat, with corresponding reduction in the economy obtainable. In the experience of various English railway companies, it would appear that in the majority of



Rate of firing in lb. per. sq. ft. grate area per hour.

Curve A. Hourly evaporation per lb. coal from and at 212° F. for various rates of firing. High Limit Rodgers.

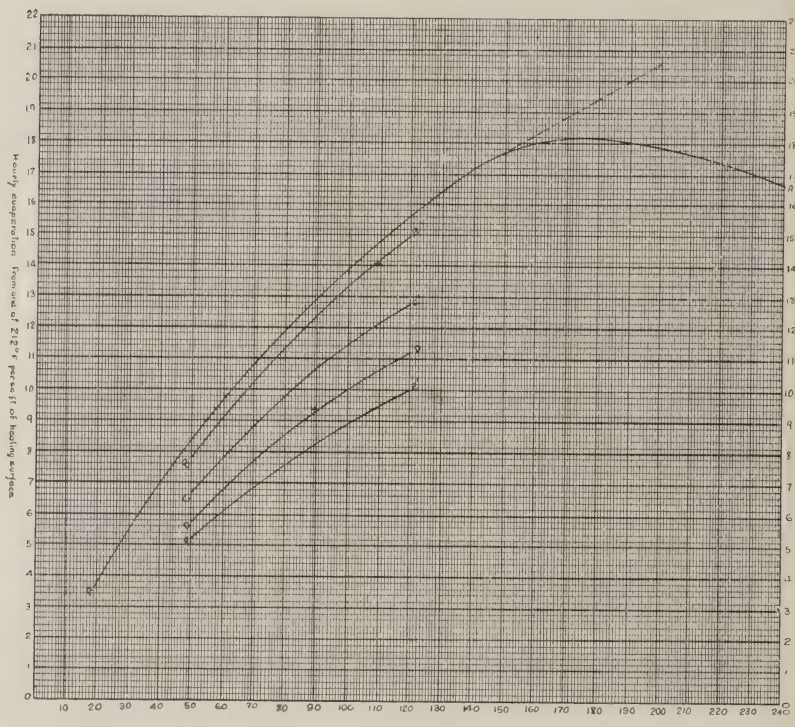
Curve B. Hourly evaporation per lb. coal from and at 212° F. for various rates of firing. Low Limit Rodgers.

Curve C. Hourly evaporation per lb. coal from and at 212° F. for various rates of firing. Pennsylvania R.R. actual tests.

FIG. 1.—BOILER EVAPORATION CHART.

local services superheating is a decided advantage, but that in some cases, where stops are made at very short intervals (the London suburban area of one of our important companies is referred to), the fuel economy falls as low as 3 per cent., which is insufficient to justify the use of a superheater. For shunting duties the general experience is that superheating is of no advantage (some valuable data as to the advantage of a superheater in local service will be found in Mr. J. A. Hookham's paper on "Superheated and Non-Superheated Tank Engines," read before the Institution of Locomotive Engineers, in London, on the 4th May, 1922).

The principal factor governing the efficiency of the steam locomotive is the boiler. The efficiency of the boiler depends mainly upon the rate of firing, experiments having proved conclusively that it is the efficiency of combustion rather than that of heat absorption that determines the efficiency of the boiler as a steam generator. In order to be able to calculate approximately the efficiency of the steam locomotive, Fig. 1 has been drawn up giving water evaporated per lb. of coal for rates of firing usually met with in actual practice. To obtain the best compromise in a steam locomotive the boiler should be so proportioned



Rate of firing in lb. per sq. ft. of grate area per hour.

Curve AA′ Pennsylvania R.R. actual tests. Heating surface/grate area = 57·5
,, BB′ Rodgers (Locomotive designer and design). Heating surface/grate area = 60
,, CC′ ,, ,, ,, ,, ,, ,, ,, ,, = 70
,, DD′ ,, ,, ,, ,, ,, ,, ,, ,, = 80
,, EE′ ,, ,, ,, ,, ,, ,, ,, ,, = 90

Calorific value of fuel (actual) 14,140 B Th. U's. per lb. Curve AA′.
,, ,, ,, ,, (assumed) 14,500 ,, ,, ,, ,, ,, for remaining curves.

FIG. 2.—BOILER EVAPORATION CHART.

as to avoid exceeding a firing rate of 120 lb. per square foot of grate area per hour under the heaviest normal demand, the average throughout a run should not much exceed 80 lb. per square foot of grate area per hour.

The steam consumption of a modern superheater simple steam locomotive should lie between 16·5 lb. per i.h.p. hour under most favourable conditions as when running at speed with an early cut-off, and 21 lb. per i.h.p./hour when working hard under a late cut-off; the corresponding figures for saturated steam will range from 22/28 lb. per i.h.p./hour. Using these figures in conjunction with Fig. 1, we are enabled to calculate approximately the thermal efficiency that may be expected from a steam locomotive under any given conditions.

Formula for thermal efficiency.

Let E = evaporation of water per lb. of coal per hour from feed temperature to boiler pressure.

S = steam used by engine per i.h.p./hour.

C = coal per i.h.p./hour = S/E.

K = calorific value of fuel in B.Th.U.'s per lb.

Then thermal efficiency per cent. on i.h.p. basis =

$$\frac{60 \times 33{,}000 \times 100}{778 \times C \times K} = \frac{254{,}500}{C \times K}$$

E can be determined by reference to Fig. 1.

In this chart—

Line *a-a* represents best results obtained in British practice (higher values).

Line *b-b* represents best results obtained in British practice (lower values).

Lines *a* and *b* are taken from Rodger's paper "The Locomotive Designer and Design," read before the Institution of Locomotive Engineers, in April, 1917.

Line C-C represents results obtained on the Altoona test plant of the Pennsylvania R.R., U.S.A. (See *Engineering*, Feb. 4th, 1921).

TABLE I.

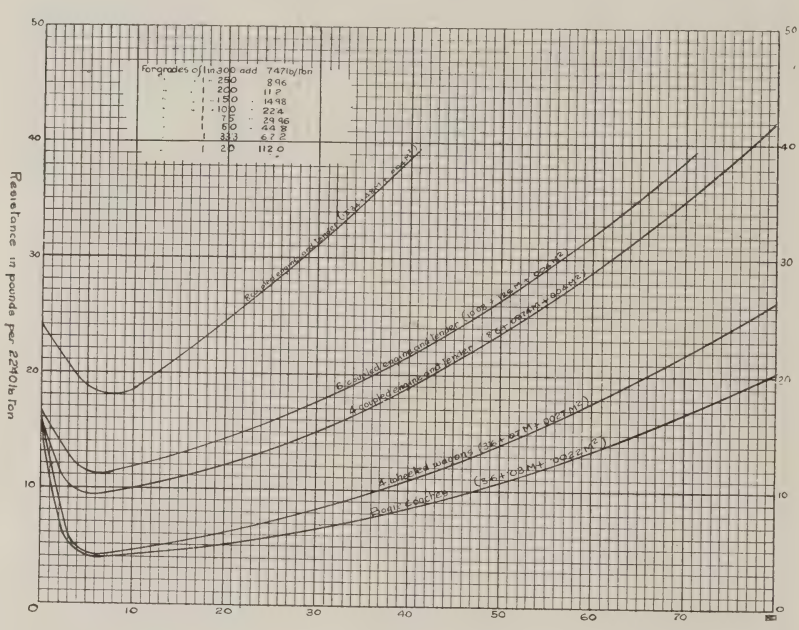
THERMAL EFFICIENCY BASED ON INDICATED HORSE-POWER.

Rate of firing in lb. of fuel per sq. ft. grate per hour.	Thermal efficiency on I.H.P. basis assuming 1 lb. Steam per I.H.P. hr. with superheater and 28 lb./I.H.P. hr. with saturated steam.			Theoretical efficiency on I.H.P. basis assuming 16·5 lb. steam per I.H.P. hr. with superheated and 22 lb. I.H.P. hr. with saturated steam.		
	Max Per cent.	Min. Per cent.	Mean. Per cent.	Max. Per cent.	Min. Per cent.	Mean. Per cent.
80 (Superheated)	7·3	6·36	6·83	9·27	8·08	8·675
80 (Saturated)	5·48	4·78	5·13	6·95	6·07	6·51
120 (Superheated)	6·27	5·4	5·835	8·0	6·87	7·435
120 (Saturated)	4·58	4·05	4·315	5·96	5·17	5·565

Explanation of table of calculated thermal efficiencies from a theoretical basis.—In this table (Table I.) the maximum values are

calculated on the assumption that the boiler evaporation follows th line *a-a* (Rodger's figures), and the efficiency is based on a calorific valu of 14,500 B.Th.U.'s per lb. of coal. The minimum values are calculate on a basis of line C-C (Pennsylvania results), being representative o boiler performance. The efficiency is calculated assuming a calorifi value of 14,140 B.Th.U.'s per lb. of coal.

Table II. gives the overall thermal efficiencies at the drawbar. Fo goods traffic the drawbar efficiency is taken as 80 per cent., which corresponds to a six-coupled engine hauling 9·3 times its own weight o four-wheeled goods stock on the level at 20 miles per hour. For passenge



Curves showing resistance of locomotives and rolling stock at various speeds (L. H. Fry's formulæ).

Fig. 3.

service the drawbar efficiency is taken as 60 per cent. which corresponds to a four-coupled engine hauling 3·25 times its own weight on the level at 56 miles per hour, bogie rolling stock being assumed.

In order that these figures may be verified, a chart (Fig. 3) is given, showing resistance of locomotives and rolling stock at various speeds, based on the formula of Mr. L. H. Fry, which is generally accepted for calculations of train resistance. It will be noted that the drawbar efficiency is taken on the level. When a grade is being ascended this factor rises due to the gravitational forces per ton being the same throughout engine and train. The basic thermal efficiencies taken are the means of the figures given in the third and sixth columns in Table I.

TABLE II.

Conditions and firing rate.	Mean thermal efficiency on based or I.H.P.	Overall thermal efficiency in heavy service (80 per cent.) drawbar efficiency.	Overall thermal efficiency in passenger (or other light) service (60 per cent. drawbar efficiency).
	Per cent.	Per cent.	Per cent.
80 lb. per sq. ft. grate/hr.			
Superheated	7·75	6·2016	4·651
Saturated	5·82	4·656	3·492
120 lb. per sq. ft. of grate area/hr.			
Superheated	6·635	5·308	3·981
Saturated	4 94	3·952	2·964

As a comparison, the Author has investigated several results which have actually been published as regards passenger service, some of which are taken from trials and others are the results of observations over long periods (see Appendix).

The actual results quoted are as follows :—

	Highest. Per cent.	Lowest. Per cent.	Mean. Per cent.
Superheated engines.	4·93	4·26	4·57
Saturated ,,	3·89	3·26	3·575

From this it will be seen that the theoretical values given compare well with the values estimated from the results of actual tests in British practice.

Having investigated the thermal efficiency of the ordinary types of steam locomotives in use it will be well to consider how the present state of affairs may be remedied. Neglecting minor matters such as feed-water heating, etc., which, though helpful in this respect, can hardly be expected to produce any great increase in overall thermal efficiency, especially when increased maintenance and first cost of pumps, etc., as compared with injectors, are considered, the first thing that strikes the outsider is the desirability of compounding with the object of economizing in steam consumption.

The whole position with regard to compounding is bound up with the question of fuel cost and first and maintenance charges. The conditions under which a locomotive operates are about as unfavourable to any great economy being effected in this direction as they possibly can be. The use of a condenser is not at present a practicable success for everyday service, and loads and conditions are so variable that it is only in occasional favourable conditions that compounding without condensing is any great advantage. To illustrate this, particulars are given in the Appendix of results of tests carried out with some four-

cylinder superheated compounds in France. These indicated that at normal loads the gain in efficiency due to compounding without a condenser is very small, and that it is only under conditions where a corresponding simple engine would have to be "thrashed" (or, technically speaking, operated at a considerably later cut off than normally and the boiler correspondingly forced) that any great advantage is obtained. It is, therefore, hardly surprising to find that in this country where fuel is relatively cheap compounding is little adopted, and even on the Continent there appears to be a decided tendency to return to the simple superheated engine. The most successful application of the compound system would appear to be in the huge articulated locomotives operating in the United States of America and elsewhere. These engines are usually applied to exceptionally heavy hauls on the level or continuous gradients, and this class of service, involving as it does long continuous operation at high constant power output, offers probably the most favourable field for a compound locomotive. Under average service conditions prevalent in this country and also in the majority of countries abroad, it may be safely said that at the present price of coal the additional complication of the compound engine, with consequent greater first cost and maintenance, outweighs the saving in fuel cost which is usually effected. It must always be remembered, however, that increase in fuel cost above a definite point will turn the scale in favour of the compound engine, and this accounts for the fact that in many countries abroad the compound engine is extensively employed.

The figures obtained for overall thermal efficiency of the steam locomotive will probably seem very low to those unacquainted with the peculiar conditions obtaining in railway traction. Nevertheless the Author hopes to show that they will bear comparison with the results that may be expected from other systems of traction when all the difficulties involved are considered. In spite of this, however, the locomotive engineer cannot afford to fall into a state of complacency, but must investigate the possibilities of every known way of increasing the efficiency of the locomotive. The lines upon which developments may be expected can be summarized as follows:—

(1) The construction of condensing steam locomotives.

(2) Electrification of railways.

(3) The development of the internal-combustion engine.

Condensing Locomotives.—The condensing locomotive is by no means a new proposal. The great variation in quantity of steam to be dealt with, coupled with the complication involved in the necessary arrangements for circulating and cooling the cooling water, together with the necessity of hauling about additional weight which is unavailable for adhesion, renders the successful application of a condenser to locomotive purposes a matter of some difficulty, and it is not surprising to find that all such schemes have, up to the present, fallen into disuse. A few

locomotives have recently been constructed in which a turbine is used in conjunction with a condenser, the turbine in some cases driving the wheels through gearing and in other cases by the medium of electric or other system of transmission.

The Author was rather surprised to find a turbine used without the interposition of some form of flexible transmission since it would appear to him that the low starting torque of the turbine, when compared with that of a reciprocating engine of corresponding capacity, would place the turbine locomotive at a considerable disadvantage at starting when compared with an engine of the more orthodox pattern. Assuming, however, that the starting difficulty can be overcome, it should be possible on these lines to develop a locomotive which will give quite a good overall efficiency at the drawbar.

Allowing for an average efficiency of 12 per cent. being obtained at the turbine and assuming a loss of 5 per cent. in transmission between turbine shaft and rail (a low estimate) we get, thermal efficiency at rail—$0{\cdot}95 \times 12 = 11{\cdot}4$ per cent.

Assuming that drawbar pull = 60 per cent. of tractive effort, then overall thermal efficiency $= 11{\cdot}4 \times 0{\cdot}6 = 6{\cdot}84$ per cent., which represents a fuel saving of 33·1 per cent. when compared with the average results given for superheater tests, and 9·55 per cent. as compared with the best results obtained from the French compounds.

The approximate results obtainable with a turbine locomotive with electrical transmission may be summarized as follows:—

Efficiency of turbine and condenser, say, 12 per cent.

Efficiency of electrical transmission (generator and motor), 75 per cent.

Efficiency of gearing between motor and wheel, 95 per cent.

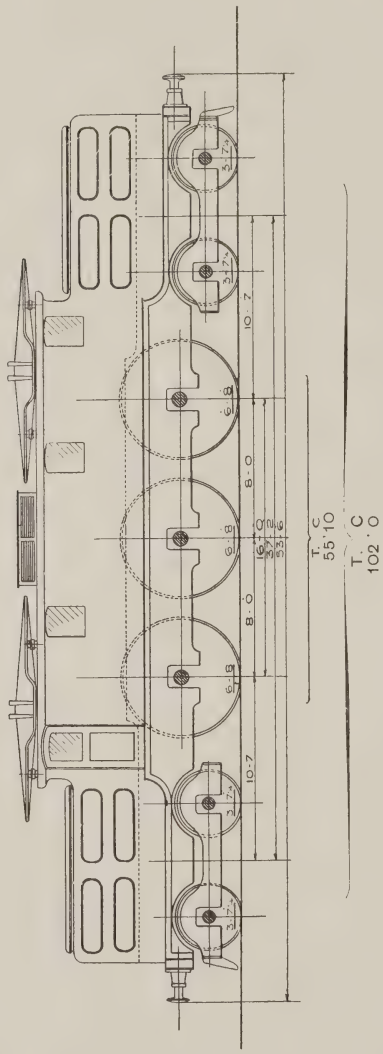
Overall thermal efficiency at rail $= 12 \times 75 \times 95 = 8{\cdot}56$ per cent.

Overall thermal efficiency at drawbar $= 8{\cdot}56 \times 0{\cdot}6 = 5{\cdot}136$ per cent.

This indicates a fuel saving of 10·75 per cent., taking average superheater-engine results, and is considerably inferior to the best French compound results given. When the much greater first cost of the turbine locomotive is considered coupled with a probably much increased maintenance cost due to the condenser and other appliances appertaining to its operation, it is very doubtful whether the locomotive is going to be a successful proposition from a commercial standpoint, especially when electrical transmission is employed. Thermally, doubtless, the use of a condenser is abundantly justified, but it is well known that unless a financial saving can be proved the possibility of any engineering appliance being adopted is very slender. It may be argued that higher thermal efficiency can be obtained at the turbine. It must be remembered, however, that the load is far, very far, from the comparatively constant load obtaining in stationary practice and in the case of the pure turbine machine the speed also varies through a wide range. The difficulties in the way of maintaining a high vacuum are

much greater than in stationary practice, so that, on the whole, the writer considers the figure taken to be fairly representative.

Railway Electrification.—The question of railway electrification is now receiving a great deal of attention in many quarters, and in view of the grossly exaggerated reports frequently made in its favour, generally by electrical engineers without actual experience of railway



Motors (6) Total H.P. One hour rating 1,800.
Continuous rating 1,260.
Rated Tractive effort at 65 miles per hour 8,400.
Horse power " " " " " 1,454.
Horse power per ton weight of engine 14·25.

FIG. 4.

operating conditions, it will be well to afford the subject some consideration in order that we may be able to make a few comparisons.

The advantages claimed for railway electrification may be briefly summarized as follows:—

(1) Greater economy of fuel consumption.

(2) Lower weight of electric locomotive as compared with a steam machine of equal capacity.

(3) Possibility of better acceleration and higher average speed.

(4) Cleanliness.

Considering first the question of fuel economy, Mr. Roger T. Smith, in a paper read before the Institution of Electrical Engineers in 1914, gives the following figures for power-house economy and transmission losses at various points :—

Average thermal efficiency of power station taken over a period of one year, 11 per cent.

Efficiency of transmission between power and sub-stations, 95 per cent.

Efficiency of sub-station, 79 per cent.

Efficiency of transmission between sub-station and train, 93 per cent.

These figures give a thermal efficiency of $11 \times 0{\cdot}95 \times 0{\cdot}79 \times 0{\cdot}93 =$ 7·68 per cent. at collectors.

Before an estimate can be formed as to the efficiency at the drawbar, it is necessary to know the approximate weight of an electric locomotive to perform a specified duty. Fortunately this is not very difficult. Sir Vincent Raven states that his new electric locomotive of the 4-6-4 type is capable of maintaining a tractive force of 8,400 lb. at 65 miles per hour and weighs 102 tons. If we take the resistance to traction per ton of weight to be the same as that of a six-coupled steam locomotive and tender (a fair assumption when electrical losses and mechanical losses in gearing are allowed for) we get, on reference to Fig. 3—

Resistance of engine on level $= 35{\cdot}18$ lb./ton, total $35{\cdot}18 \times 102 =$ 3,590 lb.

Deducting this from the total of 8,400 lb. leaves 4,810 lb. available at the drawbar. The drawbar efficiency therefore $= \frac{4{,}810}{8{,}400} = 57{\cdot}3$ per cent. The overall thermal efficiency at drawbar, therefore, is $7{\cdot}68 \times 57{\cdot}3 = 4{\cdot}4$ per cent., which is approximately equivalent to the results that would be expected from a superheated-steam locomotive under similar conditions. Some comparative data as to the relative efficiency of steam and electric locomotives were given by Mr. J. E. Muhlfeld in a paper read before the American Society of Mechanical Engineers and other bodies on the 22nd October, 1920. Full details will be found in the Appendix; summarized, the results are as follows :—

RELATIVE THERMAL EFFICIENCES OF STEAM AND ELECTRIC LOCOMOTIVES.

Kind of locomotive.	100 per cent. load rating. Per cent.	75 per cent. load rating. Per cent.	50 per cent. load rating. Per cent.
Electric	5·79	5·95	4·54
Steam (with superheater)	3·85	4·83	5·88
Steam (without superheater)	2·7	3·86	4·47

Mr. Muhlfeld remarks that, as 100 per cent. load rating conditions occur in practice for very short periods and as the majority of the drawbar load represents from 30 to 60 per cent. of the maximum capacity, comparisons should properly be made only with the net thermal

efficiencies at 50 per cent. load ratings.

Sufficient has, I think, been said on the subject of thermal efficiency to show that on that score alone there is practically no justification for the electrification of steam railways, so we will leave this point and consider the remaining claims.

There is a considerable saving of locomotive weight by the use of an electric locomotive for the simple reason that it is not a self-contained unit. The necessity of any self-contained unit having to carry its own fuel supply must inevitably result in a heavier unit than is the case for a machine not so handicapped. As an example, it will be interesting to compare the weight of the electric locomotive previously referred to with a steam locomotive of similar capacity. The maintenance of a 8,400-lb. tractive effort at 65 miles per hour involves a total horse-power of $\frac{8{,}400 \times 65}{375} = 1{,}454$ h.p.

Since the weight of the locomotive is 102 tons, we find that $\frac{1{,}454}{102} = 14{\cdot}25$ h.p. is exerted per ton of weight of engine under these conditions.

For the steam locomotive we can take the new Great Northern 4-6-2 locomotive as being representative of really good design. This locomotive has a boiler heating surface of 2,930 square feet, excluding superheater. If we assume an evaporation of 12 lb. steam per square foot of heating surface per hour and 21 lb. of steam to be required for one h.p. hour, we get h.p. $= 2{,}930 \times \frac{12}{21} = 1{,}675$, which can easily be maintained.

The weight of the engine alone is 92·45 tons, and that of the tender 56·3, making a total of 148·75 tons.

Horse-power per ton weight of engine alone is therefore—

$$\frac{1{,}675}{92{\cdot}45} = 18{\cdot}15 \text{ h.p.}$$

Horse-power per ton weight of engine and tender combined—

$$\frac{1{,}675}{148{\cdot}75} = 11{\cdot}26$$

These figures show that, neglecting the tender, the steam locomotive is, weight for weight, a considerably more powerful machine than its electrical competitor. When the tender is taken into consideration, however, there is a saving of weight of practically 30 per cent. in favour of the electric locomotive. In any case, however, since the thermal efficiency of the drawbar is practically the same in each case, the additional weight of the steam locomotive is of no great consequence.

Whether better operation can be obtained by electrification or not depends entirely upon circumstances. The electric motor has a large reserve of power to draw upon and is capable of taking a very large overload for short periods. This can be taken advantage of at starting, with the result that a considerably better acceleration is possible with

an electric locomotive on suburban and similar service than with a steam locomotive. Especially is this the case when the multiple-unit system is employed, thus dispensing with the dead weight of a locomotive to a large extent. The steam locomotive is operating at its lowest efficiency when frequent starts, with consequent late cut-offs and also stand-by losses when coasting and standing are the rule. It is safe to say as a rule in suburban services with an average of perhaps three-quarters of a mile between the stations, that electrification will result in better operation and higher thermal efficiency. From a financial and operating standpoint, it is quite likely that equal results could be obtained from a steam engine of about twice the power usually employed, although if this is done we get drawbar efficiencies of the order of 30/40 per cent. with consequent low overall thermal efficiency, even though the efficiency on an indicated-horse-power basis may be quite good. Under average railway conditions the steam locomotive is ideally suited for its duty, and practically no improvement would result from a change to electrical operation. It must be remembered always that the electric locomotive is not a self-contained unit, and it is possible for a whole section to be stopped by a breakdown in the transmission system. A breakdown on a steam locomotive is seldom sufficient to cause a total stoppage, and even if it does, another engine can usually be commissioned to help matters. In the case of a breakdown in electrical transmission the whole section is stopped until the trouble is repaired, and in the meantime everything is at a dead standstill.

The last item is that of cleanliness. It is, of course, evident that a certain amount of smoke is inevitable with steam operation. After all, however, the smoke from the railway is a very small part of the total smoke in a large city, and unless there are other advantages to be obtained, railways are not going to waste capital on that score alone. Should city corporations wish to enforce electrification on such grounds, then railway companies would be quite justified in charging extra fares to pay for the increased capital charges. If this is done it is most unlikely that the aforesaid corporations would persist in their attitude.

It may be said finally that the whole question of railway electrification is one of finance. The amount of capital involved is enormous. The cost of the locomotives alone amounts to two to three times the cost of modern steam locomotives of corresponding capacity, and in addition there are many expensive items, such as power and sub-stations, third rails or overhead gear, etc., to be paid for and maintained in working order. The scrapping of existing plant is also an enormous item. In actual maintenance the electric locomotive, due to the elimination of the boiler, is possibly cheaper than the steam, but to obtain a fair basis the maintenance of generating and transmission equipment must be added to the locomotive maintenance costs. In short, it may be said that only when very heavy traffic is the rule can any fair return be expected from electrification. Some interesting comments will be found in a contribution to the discussion on a paper on Railway Electrification

delivered before the American Institute of Electrical Engineers in 1913, by Mr. A. H. Babcock. He says, with reference to reports made on electrification of mountain railways of Western America :—

"For not a single one of these reports, which have been made in considerable numbers, could it be said that the operating economies which have been affected by superseding with electric locomotives the steam locomotives on such railways, are enormous, or indeed of such amounts as to defray in a few years the initial outlays for substations, for feeders and contact conductors, and for electric locomotives. Quite the converse is the fact, and in every case on which a report has been made the conclusions have been adverse to electrification on precisely the reverse of these claims." (*Proceedings* American Institute of Electrical Engineers, November, 1913, page 2132).

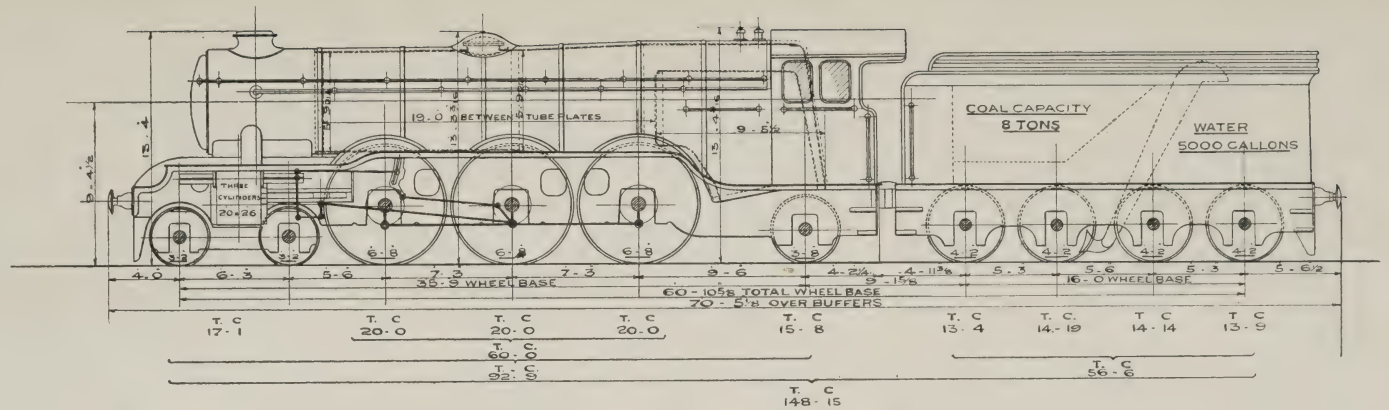
The Author, having expressed his opinion on the subject of electrification, leaves it for others to add any further comments they may wish, and will proceed to discuss the possibilities of the internal-combustion-engined locomotive.

Internal-Combustion-Engined Locomotive.—For shunting and light-railway service where trains are few, the small internal-combustion locomotive, as at present employed in considerable numbers, has everything to recommend it. It is always ready to start immediately when required, has no stand-by losses and can be handled by one man, who need not have any special skill, and is free for other duties when the locomotive is not in operation. These advantages are of great value where the service is not sufficient to require a locomotive in fairly continuous operation, and the saving effected by the elimination of stand-by losses is frequently sufficient to give a fair return on the increased cost of the internal-combustion locomotive as compared with a steam locomotive of equal power.

Such engines generally consist of a petrol engine similar to the usual motor-car or motor-boat type, driving the wheels through an ordinary three- or four-speed gear box. These features involve nothing novel or untried, and locomotives of this type should require very little more attention in the way of maintenance than does the ubiquitous commercial motor lorry. For services requiring continuous operation, however, the cost of petrol as compared with coal renders any such prime mover out of the question, even though thermally it may be more efficient than the steam engine.

For the high powers required to replace the ordinary steam locomotive in railway operation, it is necessary to have something very different from the petrol-engine type, and experiments are now being conducted in various quarters with a view to developing an internal-combustion locomotive using heavy oil which can take the place of a steam locomotive under favourable conditions.

The great obstacle in the way of the adaptation of an internal-combustion engine for locomotive purposes is its lack of flexibility. By



Heating	Surface	Large Tubes	835	sq. ft.
,,	,,	Small ,,	1880	,,
,,	,,	Firebox	215	,,
,	,,	Total	2930	,,
,,		Superheater	525	,,
		Total	3455	,,
		Grate area	41·25	,,

Working Pressure 180 lb. per sq. in.

Approximate Boiler Horse Power 1675.
Weight of Engine without Tender 92·45 Tons.
Horse Power Per Ton of Weight Neglecting Tender 18·15
Weight of Engine and Tender in full working order 148·75 Tons.
Horse Power Per Ton of Weight of Engine and Tender 11·26.

Fig. 5.

careful attention to detail it seems possible to get an engine sufficiently flexible as regards speed, but this alone is not sufficient. The internal-combustion engine as at present used is largely a constant-torque proposition or at any rate the variation of torque possible with steam is entirely out of the question. The consequence is that a directly driven internal-combustion engine is likely to be a very bad starter, although once away there is no reason why a fairly successful performance should not be obtained. Attempts have been made and systems proposed for starting an internal-combustion engine by air, a typical example being the design put before this Institution by Mr. Burn last session. As pointed out by the Author of the present paper in the discussion that followed Mr. Burn's paper, such an arrangement is a very poor compromise, inasmuch as at a slow speed (2 to 4 miles per hour as a rule in these proposals), the air supply falls away and the tractive force is reduced to little more than one half its starting value. The possibilities of such a locomotive getting away with any heavy load on a gradient are therefore very small, and as a formidable rival to the steam locomotive any such internal-combustion locomotive can safely be ignored.

There is, however, a direct-driving arrangement that has decided possibilities; this is the combination of steam and internal-combustion engines, and particularly the Still engine. Such an engine could be built capable of handling the traffic once started with the maximum economy, and a steam boiler of the ordinary locomotive type could be provided to give the necessary high tractive force at the start. In such an engine the boiler would be oil-fired, so that the consumption of fuel in the firebox could be rapidly arrested when the need for it was not present. The exhaust gases should prove sufficient to keep a good pressure in the boiler when steam was not being drawn from it. To many people the carrying of a boiler would seem an expensive proposition, but it must be remembered that it is necessary to obtain adhesive weight, and a boiler is as useful for this purpose as anything else with the additional advantage that it provides a ready means of increasing the power when required, and also (let us whisper) for working the engine home under steam if anything should go wrong with the internal-combustion side of the piston.

We have so far confined our attention to the direct-driven type of engine. A great deal of attention is now being given to the indirectly-driven type of engine with electrical or other transmission medium. Two examples of this type will shortly be under test at the works of the firm with whom the Author has the honour to be connected (Messrs. R. & W. Hawthorn, Leslie & Co., Ltd.). For obvious reasons it is not advisable to discuss the various devices adopted to give the internal-combustion engine the required economy and flexibility as yet but the schemes look most attractive on paper and very high hopes are entertained as to the ultimate success of the schemes which are to be put to the test.

The thermal efficiency of the internal-combustion locomotive is most attractive and ample reward awaits the designers of such an engine that can combine this thermal efficiency with the flexibility and reliability of the steam locomotive. The following figures which the Author previously put forward during the discussion on Mr. Burn's paper will bear repetition as indicating the possibilities of an internal-combustion-engined locomotive.

1. Directly-driven engine.

Fuel consumption (oil) per i.h.p. hour = 0·35 lb.

Allowing a mechanical efficiency of 80 per cent. we get

$$\text{Fuel per rail h.p. hour} = \frac{\cdot 35}{\cdot 8} = \cdot 437 \text{ lb.}$$

With a drawbar efficiency of 65 per cent. we get

$$\text{Fuel per drawbar h.p. hour} = \frac{\cdot 437}{\cdot 65} = \cdot 673 \text{ lb.}$$

2. Internal-combustion locomotive with intermediate transmission. (Electric, steam, air, or hydraulic).

Fuel per i.h.p. hour = 0·35 lb. as before.

Allowing mechanical efficiency of 80 per cent. and a transmission efficiency of 75 per cent. gives us

$$\text{Fuel per rail h.p. hour} = \frac{\cdot 8 \times \cdot 75}{\cdot 35} = \cdot 583 \text{ lb.}$$

$$\text{And per drawbar h.p. hour} = \frac{\cdot 583}{\cdot 65} = \cdot 9 \text{ lb.}$$

3. A fair average fuel consumption for a modern superheated steam locomotive will be about 4·2 lb. per drawbar h.p. hour.

If we take the cost of oil fuel at £4 per ton and that of coal at 28s. per ton we get:

Cost per drawbar h.p. hour with directly-driven internal-combustion locomotive = 0·289d.

Cost per drawbar h.p. hour with indirectly-driven internal-combustion locomotive = 0·385d.

Cost per drawbar h.p. hour with modern superheated steam locomotive = 0·63d.

The economy possible by the use of an internal-combustion engine is, it will be seen, most marked. Unfortunately the internal-combustion locomotive at its present stage has many serious disadvantages, namely—

1. Lack of flexibility when compared with steam locomotive, necessitating electrical or other transmission medium which cannot but add greatly to first and maintenance costs.

2. Increased complication with consequent greater risk of breakdown.

3. Enormously increased first cost as compared with steam engine of similar capacity.

It must be remembered that although the fuel bill is an important item it is only one of the many featuring on the expenditure side of a railway company's account, and until the above handicaps are greatly reduced and an internal-combustion locomotive is produced which compares favourably with the steam locomotive as regards simplicity and reliability, there seems little hope of this type of locomotive being introduced on any large scale.

The Author sometimes thinks that investigation as regards the internal-combustion engine is proceeding along the wrong lines. Special transmission systems, etc., all tend in the direction of adapting the locomotive to the internal-combustion engine. What is really required is an internal-combustion engine adapted to the locomotive. If an internal-combustion engine could be developed with which an indicator diagram similar to the steam engine could be obtained at starting, in conjunction with the higher efficiency of the internal-combustion engine when running at speed, a decided step would have been made. Such an engine if of simple construction would possibly be much lower in efficiency taken all round, but at the same time more preferable for locomotive purposes than the present complicated types. What is urgently needed, in fact, is a simple flexible and possibly uneconomical internal-combustion engine, bearing the same relation to the complicated stationary article as does the simple high-pressure-steam locomotive to the compound and condensing stationary steam engine. Given such a prime-mover it would only be a matter of time before we saw it adopted in large numbers for every class of railway service. We have not, so far as the Author knows, any internal-combustion engine that will answer to the above requirements at present, but at the same time this certainly seems possible and affords a promising line for investigation.

In the meantime there are many parts of the world where the scarcity of coal and the abominable quality of the water available are most unfavourable to the steam locomotive and in these regions there is a good field for the employment of even the present imperfect type of internal-combustion locomotive.

Speculations as to future railway development and the conditions likely to obtain in the future, are difficult, as the opinions of loud talking politicians and a badly informed public, appear generally to be the deciding factor rather than the carefully considered opinion of the engineer trained in these matters. A really efficient transportation scheme would employ all three systems each in its best sphere. Thus

if conditions such as the bottle-neck effect seen at some of our terminal stations apply and electrification offers relief, it should be resorted to. The general run of work can be well looked after by the steam locomotive, whilst isolated branch lines with infrequent and light services offer excellent fields for the development of the internal-combustion locomotive which could undertake more and more important duties as it increased in flexibility and reliability, which it is almost certain to do. It is to be hoped that the future will see all systems considered on their merits and adopted accordingly, as only by this means can we obtain increased efficiency in railway operation.

In conclusion the Author can only say that if this paper affords the members of the Institution a fraction of the interest which it has given him during its preparation he will consider the time taken to have been well spent.

APPENDIX.

Much of the matter contained in the Paper—notably the portion dealing with the thermal efficiency of the steam locomotive, is based on certain fundamental theoretical assumptions. In order to demonstrate the accuracy of the conclusions given the following records have been drawn up of results attained in actual practice on various railways.

Taking saturated steam first, some excellent results were obtained in the trials of a London and South Western Railway 4-4-0 type locomotive as far back as 1891.

Particulars of the locomotive were as follows:—

	T.	C.	Q		
Cylinders, 19″ dia. × 26″ stroke.				Boiler inside diameter (min.), 4′ 3″.	
Coupled wheels, 7′ 1″ diameter.				Length between tube-plates	11′ 4″
Weight of engine in working order....	48	12	2	Working pressure, 175 lb. per sq. inch.	
Weight of tender in working order....	33	8	0	Heating surface, 240 tubes, $1\frac{3}{4}$ outside diameter	1,245.60 sq. ft.
				Firebox	122·16 sq. ft.
Total engine and tender	82	0	2		1,367·76
Tender tank capacity, 2,908 gallons.				Grate area	18 sq. ft.
Coal	2 tons.				

Tractive force taking 85 per cent. working pressure 16,400 lb.

Ratios:—

$$\text{Adhesive factor} = \frac{29{\cdot}75 \times 2{,}240}{16{,}400} = 4{\cdot}065.$$

$$\frac{\text{Heating surface}}{\text{Grate area}} = \frac{1367{\cdot}76}{18} = {\cdot}76$$

$$\frac{\text{Length of tube between tube-plates}}{\text{Internal diameter}} = \frac{136}{1{\cdot}542^*} = 87.$$

* Tubes $1\frac{3}{4}$ dia. 11-13 Wg. taken as 12 Wg. mean thickness.

SUMMARY OF RESULTS.

		1	2	3	4	5	Mean.
Load hauled in tons	Maximum	240	138	168·5	244½	137½	
	Mean	180	136	168·5	199	137½	164·2
Maximum i.h.p.		684·1	610·1	803·6	804·3	626·1	
Mean i.h.p. (from cards)		490·6	485·1	558·1	582	536·7	530·5
Mean boiler pressure		167·5	167·2	171·7	169·4	170·8	169·32
Maximum speed, miles per hour		68·5	67	78	81	75	
Mean speed, miles per hour		37	45·2	46·1	44·45	46·7	43·89
Water consumed per i.h.p. hour		22·4	23·02	20·7	19·94	22·9	21·792
Coal consumed per i.h.p. hour (including standing losses)		2·31	2·61	2·34	2·52	2·45	2·446
Equivalent evaporation lb. coal from and at 212° F.		11·35	10·3	10·4	9·28	10·96	10·458
Coal per sq. ft. grate per hour (running)		62·54	70	72·18	80·89	62·7	69·662
Calorific value of fuel (B.Th.U's. per lb.)		13,903	13,903	13,583	12,840	13,477	
Boiler heat efficiency per cent.		77·5	70	71·1	66·6	75·5	
Engine heat efficiency per cent.		9·8	9·55	10·7	11·02	9·61	
Overall on i.h.p. basis		7·7	6·68	7·6	7·34	7·25	7·314

Results of trials:—

Trial No. 1 was made between London and Bournemouth, 111 miles, reduction of load being due to detachment of coaches at various points.

Trial No. 2 was between Bournemouth and London.

Trial No. 3 was between London and Exeter, 171½ miles.

Trial No. 4 was between Exeter and Woking, 147 miles, variation of load, being due to attachment of coaches at various points.

Trial No. 5 was between London and Salisbury, 83⅝ miles.

As no profile is available the determination of the drawbar pull is rather a complicated matter but an approximation can be arrived at as follows:—

The mean h.p. for the trips is 530·5 and the average speed 43·89 m.p.h.

$$\text{Transposing the formula horsepower} = \frac{\text{Tractive effort and speed in m.p.h.}}{375}$$

we get: $$\text{Mean tractive effort} = \frac{375 \times \text{h.p.}}{S} = \frac{375 \times 530{\cdot}5}{43{\cdot}89} = 4{,}530 \text{ lb.}$$

The frictional resistance of the engine on level from Fig. 3, is 20·47 lb. per ton; the total resistance therefore is $20{\cdot}47 \times 82 = 1{,}680$ lb. The resistance of the train, assuming bogie stock, will be 9·17 lb. per ton.

The total therefore being $9{\cdot}17 \times 164{\cdot}2 = 1{,}505$ lb.

Making a total of 3,185 lb.

Deducting this from the total of 4,530 lb. leaves us with 1,345 lb. resistance due to the average grade acceleration and other considerations, and this must be divided between engine and tender in proportion to their total weights, thus:

Total weight of engine and train=246·2 tons.

$$\text{Proportion of extra resistance for engine}=\frac{82}{246\cdot2}\times1{,}345=448 \text{ lb.}$$

Additional resistance of train	=897 lb.
Total resistance of engine ∴	=2,128 lb.
Total resistance of train	=2,402 lb.
Total	4,530 lb.

$$\text{Drawbar efficiency } \therefore=\frac{2{,}402}{4{,}530}\times100=53\cdot2 \text{ per cent.}$$

Overall efficiency on drawbar=7·314×·532=3·89 per cent.

$$\text{Coal per d.h.p. hour}=\frac{2\cdot446}{0\cdot532}=4\cdot6 \text{ lb.}$$

The above figures are rather better than the average for saturated steam engines. Tests on the Midland Railway indicated a fuel consumption of 2·9/3·1 lb. of coal and 29 lb. of water per i.h.p. hour with saturated steam. Purdue University results carried out on a small and somewhat out-of-date locomotive showed a steam consumption of 26·6/30 lb. of water per i.h.p.

Modern figures as to fuel and water consumptions are somewhat scarce, but the following results calculated from such figures as are available, may safely be taken as representing average good practice.

Kempe's *Handbook*, 1919, states on the authority of the late Mr. C. S. B. Cooke, chief mechanical engineer of the London and North Western Railway, that the average fuel consumption of the George the Fifth class superheater locomotives on that line, was from 34½/38 lb. per mile, the corresponding loads being 350 tons at average speeds of 50 m.p.h. The proportions of the engine are as under:—

Cylinders, 20½×26.
Coupled wheels, 6′ 9″ diameter.

Tender tank capacity	3,000 gal.		
Tender coal capacity	6 tons.		
Weight of engine in working order....	59	17	0
Weight of tender in working order....	39	5	0
Total	99	2	0
Tractive force taking 85 per cent. w.p.	20,000 lb.		

Boiler (inside diameter min.), 4′ 11½″.
Length between tube-plates, 12′ 2¼″.
Pressure, 175 lb.

Heating surface, 168—1⅞″ tubes	1,004·96
24—5″ tubes	380·44
Firebox	161·75
	1,547·15
Superheater	302·5
	1,849·65

Grate area, 22·4 sq. ft.

Ratios.—

$$\frac{\text{Adhesive weight}}{\text{Tractive force}}=4\cdot28$$

$$\frac{\text{Superheater surface}}{\text{Evaporative surface}}=\cdot195 \text{ or } 19\cdot5 \text{ per cent.}$$

$$\frac{\text{Evaporative surface}}{\text{Grate area}}=69.$$

$$\frac{\text{Tube diameter (in.)}}{\text{Length between tube-plates}}=\frac{146\cdot25}{1\cdot667}=88.$$

Analysing the above figures we get

Tractive force to move engine and tender at 50 m.p.h.= 23·45 lb. per ton × 99·1 .... =	2,320 lb.
Tractive force to move train at 50 m.p.h.=10·64 lb./ton= 10·64 × 350 .... =	3,720 lb.
Total	6,040 lb.

From the nature of this railway and the length of non-stop runs (the trials were between London and Crewe and *vice versa*, a distance of 158 miles in either direction), no serious error will be introduced by assuming the journey as over level track.

$$\text{Average i.h.p. } \therefore=\frac{6{,}040\times50}{375}=805 \text{ h.p.}$$

$$\text{Coal per hour at 38 lb./mile}=38\times50=1{,}900 \text{ lb.}$$

$$\text{Coal per sq. ft. grate/hour}=\frac{1{,}900}{22\cdot4}=84\cdot8 \text{ lb.}$$

$$\text{Coal per i.h.p. hour}=\frac{1{,}900}{805}=2\cdot36 \text{ lb.}$$

$$\text{Drawbar efficiency}=\frac{3{,}720}{6{,}030}\times100=61\cdot7 \text{ per cent.}$$

$$\text{Coal per d.h.p. hour}=\frac{2\cdot36}{61\cdot6}=3\cdot82 \text{ lb.}$$

Assuming a figure of 13,500 B.Th.U's. per lb. as the calorific value of the fuel (a fair average) we get:

$$\text{Thermal efficiency on i.h.p. basis}=\frac{254{,}500}{2\cdot36\times13{,}500}=8 \text{ per cent.}$$

$$\text{Overall thermal efficiency at drawbar}=8\times0\cdot617=4\cdot93 \text{ per cent.}$$

For the corresponding saturated engines the *Locomotive Engineers' Pocket Book* gives the fuel consumption as 57·53 lb. per mile for the same data.

The grate area is the same in each case so that we get

$$\text{Coal per hour with saturated steam}=57\cdot53\times50=2{,}880 \text{ lb.}$$

$$\text{Coal per sq. ft. of grate per hour}=\frac{2{,}880}{22\cdot4}=128\cdot5 \text{ lb.}$$

The weight of the non-superheater engine is 96·75 tons or $2\frac{1}{4}$ tons less than the superheater engine. This difference, however, is so small as to be negligible so that i.h.p. can be taken as 0·805 as before.

$$\text{Then coal per i.h.p. hour}=\frac{2{,}880}{805}=3\cdot58 \text{ lb.}$$

$$\text{Then coal per d.h.p. hour}=\frac{3\cdot58}{61\cdot7}=5\cdot8 \text{ lb.}$$

$$\text{Thermal efficiency on i.h.p. basis}=\frac{254{,}500}{3\cdot58\times13{,}500}=5\cdot28 \text{ per cent.}$$

$$\text{Overall thermal efficiency at drawbar}-5\cdot28\times617=3\cdot26 \text{ per cent.}$$

The remaining two examples are taken from post-war figures.

The first of these are the official figures of a test made of the converted superheater engines 4-6-0 type of the Lancashire & Yorkshire (now London and North Western Railway).

The dimensions of the engine are as follows:—

Cylinders (4), $16\frac{1}{2} \times 26$.
Wheels (coupled), 6′ 3″ diameter.

	T.	C.	Q.
Weight on coupled wheels	59	6	0
Total weight of engine in working order	79	1	0
Total weight of tender in working order	32	6	1
Total engine and tender	111	7	1

Tender tank capacity, 2,480 gallons.
Tender fuel capacity, 5 tons.

Minimum diameter of boiler (inside), 5′ 7″.
Length between tube-plates, 14′ 8″.

H.S. 169—2″ tubes .... 28—$4\frac{3}{4}$″ flues	1,511
Firebox	175
Total evaporative surface	1,686
Superheater	552
Total	2,238

Grate area, 27 sq. ft.
Boiler pressure, 180 lb.

Tractive force taking 85 per cent. of boiler pressure, 28,879 lb.

Ratios:—

Evaporative surface/grate area=62·5.

Tube length/diameter (thickness of 11 wq. assumed) $\frac{176}{1{\cdot}768}=99{\cdot}5$

Superheater surface/evaporative surface=0·327 or 32·7 per cent.

$$\frac{\text{Adhesive weight}}{\text{Tractive force}}=\frac{59{\cdot}3\times 2{,}240}{28{,}879}=4{\cdot}6$$

The published results of trials are as follows:—

Dynamometer car run No. 56, 5th December, 1920.

Manchester (Victoria) to Blackpool (Talbot Road) and return.

	Outward.	Return.	Mean.
Mean weight of engine and tender	109·9125	109·9125	109·9125
Mean weight of train	385·6	385·6	385·6
Total (engine, tender and train)	495·5125	495·5125	295·5125
Coal per train mile, excluding shed duties	75·4	55	65·2*
Coal per sq. ft. grate per hour excluding shed duties	110	83·6	96·8*
Coal per drawbar h.p. hour excluding shed duties	4·94	3·924	4·432*
Average speed	40·7	41·6	41·15
Maximum	65	61·5	
Average drawbar h.p.	745	685	715*
Maximum drawbar h.p.	1,080	1,160	
Overall thermal efficiency at drawbar assuming 13,500 B.T.U's lb.	3·82 per cent.*	4·8 per cent.*	4·26 per cent.*

* Calculated figure.

The final results I have selected are the results given by the three-cylinder "Mogul" (2-6-0) type engines on the Great Northern Railway, which have been made public by Mr. Gresley, the Chief Mechanical Engineer of that railway.

The particulars of the engine are as follows:—

Cylinders (3) $18\frac{1}{2} \times 26$.
Coupled wheels, 5′ 8″ diameter.
Weight of engine in working order.... .... 71·7 tons.
Weight of tender in working order.... .... 43·1 ,,
Total ....114·8 ,,
Adhesive weight, 60 tons.
Tractive force at 85 per cent. w.p., 30,100 lb.

Boiler barrel, 6′ 0″.
Length between tube-plates, $11-11\frac{7}{8}$″.
Heating surface—
$32-5\frac{1}{2}$″ tubes.... .... 527 sq. ft.
$217-1\frac{3}{4}$″ tubes .... 1,192 ,,
Firebox .... 182 ,,
Total evaporative surface .... 1,901 ,,
Superheater .... 407 ,,
Total .... 2,308 ,,
Grate area, 28 sq. ft.
Working pressure, 180 lb.

Ratios:—

$$\frac{\text{Adhesive weight}}{\text{Tractive force}} = 4{\cdot}46.$$

$$\frac{\text{Superheater surface}}{\text{Evaporative surface}} = 0{\cdot}214 \text{ or } 21{\cdot}4 \text{ per cent.}$$

$$\frac{\text{Evaporative surface}}{\text{Grate area}} = 68.$$

$$\frac{\text{Length of tube}}{\text{Internal diameter}} = \frac{143{\cdot}875}{1{\cdot}542} = 93.$$

The loads are heavy for passenger service, the trials being made at a time of industrial disturbance when fewer and heavier trains were the rule.

Records were taken of 18 runs as follows:—

5 Runs Doncaster to Peterborough (80 miles), with 567 tons behind tender; average speed, 47·53 miles per hour.

5 Runs Peterborough to Doncaster (80 miles); average load, 471 tons; average speed 46·15.

3 Runs Doncaster to Leeds (30 miles), load, 248 tons; average speed, 42·86 miles per hour.

3 Runs Leeds to Doncaster (30 miles), with goods train weighing 517 tons; average speed 17 miles per hour.

The total mileage covered was 980, of which

400 was covered at speeds of 47·5 m.p.h. with 567 tons load.
400 was covered at speeds of 46·1 m.p.h. with 471 tons load.
90 was covered at speeds of 17 m.p.h. with 571 tons load.
90 was covered at speeds of 42·86 m.p.h. with 248 tons load.

The average load works out at 468 tons and the average coal consumption 51·1 lb. per mile.

In view of the above figures we shall certainly not be favouring the engine if we assume it to be capable of taking trains of 468 tons weight at 45 m.p.h. with a fuel consumption of 51·1 lb. per mile.

Working from above figures and assuming that the track will average at about level and neglecting resistance of acceleration, we get:

Resistance of six-coupled engine and tender (114·8 tons) at 45 m.p.h. (23·82 lb./ton)	2,740
Resistance of train (468 tons) at 45 m.p.h. (9·4 lb./ton)	4,400
Total	7,140

$$\text{Mean i.h.p.} = \frac{7{,}140 \times 45}{375} = 856 \text{ h.p.}$$

$$\text{Coal per hour} = 51 \times 45 - 2{,}292 \text{ lb.}$$

$$\text{Coal per i.h.p. hour} = \frac{2{,}292}{856} = 2{\cdot}68 \text{ lb.}$$

$$\text{Drawbar efficiency} = \frac{4{,}400}{7{,}140} = 0{\cdot}6175 \text{ or } 61{\cdot}75 \text{ per cent.}$$

$$\text{Coal per sq. ft. grate per hour} = \frac{2{,}292}{28} = 81{\cdot}7 \text{ lb.}$$

$$\text{Coal per drawbar h.p. hour} = \frac{2{\cdot}68}{0{\cdot}6175} = 4{\cdot}34 \text{ lb.}$$

Allowing a calorific value of 13,000 B.T.U's. per lb. of coal in view of prevalent unfavourable circumstances at the time we get:

$$\text{Overall thermal efficiency on i.h.p. basis } \frac{254{,}500}{2{\cdot}68 \times 13{,}000} = 7{\cdot}32.$$

$$\text{Overall thermal efficiency on drawbar } 7{\cdot}32 \times 0{\cdot}6175 = 4{\cdot}52 \text{ per cent.}$$

The last completes the records of trials of simple superheater engines which are given in this paper. The average coal consumptions per indicated and drawbar horse-power hour respectively, for superheater engines, works out at 2·52 (neglecting Lancashire & Yorkshire test where i.h.p. was not given) and 4·197 lb. respectively the corresponding thermal efficienciès being 7·66 per cent. and 4·57 per cent. respectively.

For the saturated steam engines on the London and North Western and London and South Western Railways respectively, the average fuel consumptions per i.h.p. hour and d.h.p. hour are 3·013 and 5·2 lb. respectively, the corresponding thermal efficiences being 6·297 per cent. on the i.h.p. and 3·575 overall on drawbar. It will be noted, however, that the fuel consumption in the former case was 130 lb. per sq. ft. of grate per hour, as an average indicating that the engine was being forced considerably over the economical limit.

For purposes of comparison the following figures giving the results of trials conducted by Mr. Marechal of the Paris, Lyons and Mediterranean Railway, will be of service:—

	Fuel per i.h.p. hr. in lb.	Fuel per d.h p. lb.
Load excluding engine and tender, 278 tons	2·36	4·52
	2·38	4·47
Load excluding engine and tender, 383 tons	2·27	3·84
	2·205	3·73
Load excluding engine and tender, 488 tons	2·36	3·66
	2·14	3·38
Load excluding engine and tender, 646 tons	2·055	3·15
	2·063	2·95

These trials took place between Laroche and Dijon and *vice versa*, a distance of 100 miles in either direction. As no information is given as to the calorific value of the coal used it is difficult to estimate the thermal efficiencies; but taking 13,500 B.T.U's. per lb. we get the following results per i.h.p. and d.h.p. respectively for the maximum and minimum loads:—

Minimum load, 278 tons. Mean fuel per i.h.p. hour, 2·37 lb. Corresponding efficiency, 7·97 per cent.

Minimum load, 278 tons. Mean fuel per d.h.p. hour, 4·495. Corresponding efficiency, 4·2 per cent.

Maximum load, 646 tons. Mean fuel per i.h.p. hour, 2·059 lb. Corresponding thermal efficiency, 9·16 per cent.

Mean fuel drawbar h.p. hour under these conditions, 3·05 lb. Corresponding thermal efficiency, 6·19 per cent.

These figures it will be seen show no great advantage over those of a simple superheater engine except at higher load.

Comparative efficiencies of steam and electric locomotives (Muhlfeld).

The following figures as to relative efficiency of steam and electric traction at various ratings were given by Mr. J. E. Muhlfeld in a paper read before the American Society of Mechanical Engineers and other bodies in October, 1920.

Electric Locomotive Powerhouse to drawbar net Thermal Efficiency for loads of

	100 per cent.		75 per cent.		50 per cent.	
Boiler	—	76·7	—	76	—	72
Engine	18·25	14	18·29	13·9	19·17	13·8
Generator	90	12·6	89·5	12·44	86	11·88
Transformer (step up)	98	12·34	96	11·93	90	10·67
Transmission A.C.	90	11·1	95	11·32	97	10·34
Transformer (step down)	98	10·89	96	10·85	90	9·3
Converter (A.C. to D.C.)	80	8·69	75	8·13	63	5·85
Distribution D.C.	90	7·82	95	7·71	97	5·66
Motor D.C.	91·5	7·15	90·8	7·00	89·5	5·5
Machine efficiency	81	5·79	85	5·95	90	4·54

Steam Locomotive Thermal Efficiency for Load Ratings of

	100 per cent.		75 per cent.		50 per cent.	
Boiler—						
Superheated		42·7		54·9		65·0
Saturated		45		57·4		70·0
Cylinders—						
Superheated	11·9	5·08	11	6·04	10·5	6·92
Saturated	7·8	3·51	8·4	4·82	7·8	5·46
Machine—						
Superheated	75	3·85	80	4·83	85	5·88
Saturated	77	2·70	80	3·86	82	4·47

COMPARISON BETWEEN THREE TYPES OF LOCOMOTIVE.

Kind.	100 per cent. load.	75 per cent. load.	50 per cent. load.
Electric	5·79	5·95	4·54
Steam (superheated)	3·85	4·83	5·88
Steam (saturated)	2·7	3·86	4·47

Mr. Muhlfeld remarked that as 100 per cent. load rating conditions occur in practice only momentarily and as the majority of the drawbar load represents from 30/60 per cent. of the maximum capacity, comparison should rightly be made only with the net thermal efficiencies at 50 per cent. load ratings.

General dimensions of 4–6–4 electric locomotive, North Eastern Railway:—

Type	4–6–4.
Current	1,500 volt. D.C.
Overall length	53′ 6″.
Overall width	8′ 10″.
Overall height (minimum)	13′ 0⅛″.
Driving wheels	6′ 8″ dia.
Bogies	3′ 7¼″.
Rigid wheel base	16 feet.
Weight	102 tons.
Weight per driving axle	18·5.
Weight adhesive	55·5.
Tractive force (1 hour rating)	15,900.
Speed	43 m.p.h.
H.P.....	1,800.
Tractive force (continuous rating)	9,480 lb.
Speed	51·5 m.p.h.
H.P.....	1,300.

General dimensions of 4–6–2– type, three-cylinder superheated express passenger locomotives, Great Northern Railway.

Cylinders (3)	20″ × 26″.
Coupled wheels (diameter)	6′ 8″.
Bogie wheels (diameter)	3′ 2″.
Trailing wheels (diameter)	3′ 8″.
Wheelbase (rigid)	14′ 6″.
Total engine	35–9.
Total engine and tender	60–10⅝.
Heating surface firebox	215 sq. ft.
Large tubes....	835
Small tubes	1,880
Total evaporative surface	2,930
Superheater....	525
	3,455

Grate area	41·25.
Working pressure	180 lb.
Tractive force taking 85 per cent. boiler pressure	29,835 lb.
Tender tank capacity	5,000 gallons.
Tender coal capacity	8 tons.
Weight distribution, L Bogie	17·05 tons.
Coupled axles (20 tons each)	60.
T pony	15·4.
	92·45
Tender	56·3
Total engine and tender	148·75

DISCUSSION.

The President

THE PRESIDENT, Sir Archibald C. Ross, K.B.E., introducing Mr. Grime: Before I call upon Mr. Grime to read his paper on the possibilities of increased efficiency in railway locomotives, I would just like to say that it is a little disappointing perhaps that we should have such a poor audience, but, of course, it is understood firstly that most of the papers that are read here are of more direct interest to marine engineers and shipbuilders than the locomotive, and, secondly, that it is getting rather late in the session. But after all Tyneside is, I think, regarded as the home of the locomotive, the birthplace and the nursery of the railway, and it is eminently suitable to our Institution that the locomotive should still attract sufficient notice to have a paper devoted to it. I have read this paper myself and my own opinion is that it is well worthy of the records of our Institution.

The President then called upon Mr. Grime to read his paper.

Mr. Still

MR. W. J. STILL: I am in very close agreement with the Author of this paper even when he "whispers" that combustion engines can cease to function without giving due or sufficient notice. Heavy-oil engines are more reliable than petrol engines in that their cylinders are more or less independent units, and it is generally one cylinder that misses; there is no part corresponding to a carburettor, the clogging of which will put the entire engine out of commission. Nevertheless, a faulty fuel-pump valve can make one very glad to have a second source of power available, and experience has shown me that, with reasonable boiler power, the addition of a relatively small steam m.e.p. to all the cylinders can compensate for a total loss of combustion in one. This will enable the locomotive to do more than the Author suggests and haul its train to its destination, possibly even to keep scheduled time, merely at the expense of a wasteful use of fuel.

Without an adequate boiler the designs we have produced would all be too light to give sufficient adhesive weight despite the fact that we use no tender and carry ample supplies of fuel and water on the locomotive.

The Author's suggestion that "what is really required is an internal-combustion engine adapted to a locomotive" and "that cards of a steam character should be obtained at starting" agrees so closely with the lines upon which my Company are developing that I have been tempted to show you by indicator cards how far we have got. Card No. 1, Fig. 6, was taken from a 3-cylinder Still engine by holding the pencil on the paper and keeping it there while the engine started up against a heavy brake load; it shows the production of a m.e.p. of 152 lb. per square inch, with a maximum pressure of but 160 lb.—a performance that is hard to beat.

The characteristic of a rope brake is such that the load rapidly drops as speed is gained, and for this reason the steam throttle was partly

Mr. Still

closed on the above trial and the subsequent cards 2 and 4, were decreased in power by wire drawing. Such cards as above can be produced, if required, on both sides of the piston by fitting a small steam starting valve on the combustion side which valve closes and acts as a combustion relief valve when the extra starting steam is shut off and oil is injected. There would be obvious objections to this with cold

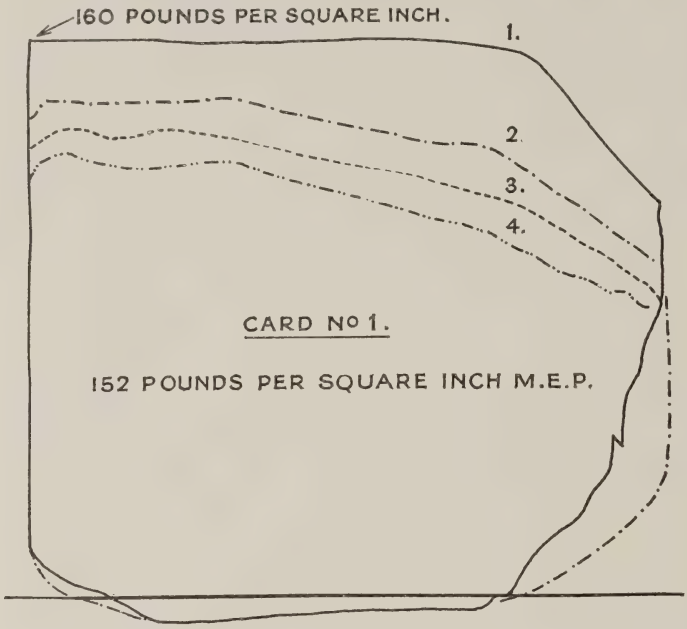


Fig. 6.

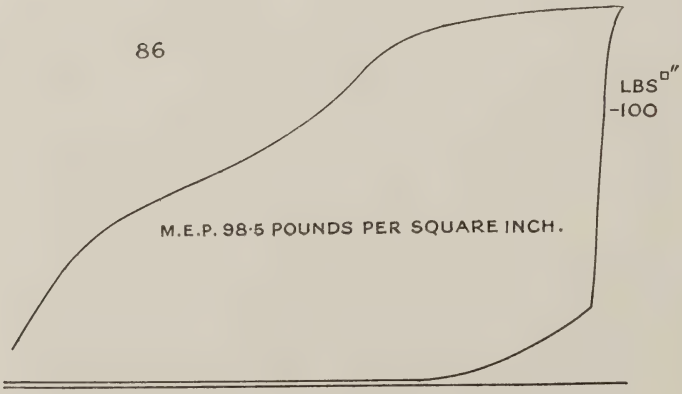


Fig. 7.

cylinders, but when maintained at 370° Fahr. moisture introduced with the steam quickly evaporates, and we find by trial that combustion is practically uninfluenced by the presence of a considerable quantity of steam in the cylinder, so long as the walls are kept hot.

The foregoing indicates a means of producing steam starting cards in a combustion engine, and when used for locomotive purposes the

steam cut-off and pressure at the back of the combustion piston would be reduced as combustion pressure increased, and the desired speed obtained through some such range as follows, all cards being produced on Still engines:— Mr. Still

Card 86 (Fig. 7).—More economical than starting card but still shows overload condition.

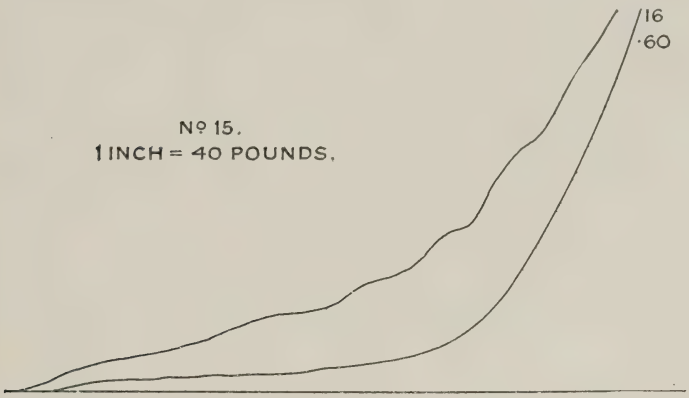


Fig. 8.

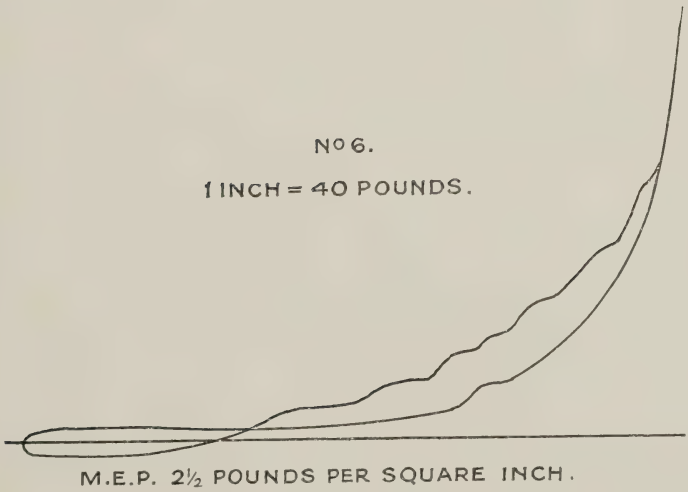


Fig. 9.

Card 15 (Fig. 8).—Steam consumption about that obtainable from waste heat alone; in fact an actual non-condensing waste-heat card from a prolonged trial.

Card No. 6 (Fig. 9).—Card equal to evaporation from waste heat at about half load on the combustion side or one which will probably be used when on full load in order to build up a reserve steam pressure to assist the locomotive on steep banks or when starting, and avoid necessity for burning extra oil under the boiler.

Mr. Still

You can see that waste heat can do far more than "keep a good pressure in the boiler when steam is not being drawn from it," as the Author puts it; trials show that over 2 lb. of steam is evaporated per hour per combustion h.p. on $\frac{3}{4}$ full load, and much more on overload in Still engines.

Messrs. Scotts' single-cylinder 320 h.p. engine evaporates from 800 to 1,000 lb. of steam per hour from that one cylinder, so waste heat produces a fairly considerable amount of steam, and with the locomotives on which we are now working it will be a very considerable item.

I do not think the Author intended to confine his remarks as to "adapting the combustion engine to locomotive service" to mere starting abilities. The modifications we have made in the oil-engine by marrying it to steam are far greater than is commonly understood; the change effected in facility of starting is considerable. In heavy-oil engines the new charge of air drawn or blown into the cylinder at the commencement of each cycle is compressed until hot enough to ignite oil when sprayed into it; there is no other means of ignition in most modern types. With cold walls the loss of heat at slow speeds is so great that the air does not reach a sufficient temperature: the difference due to hot walls is surprising because the results are cumulative. One of our engines standing at rest at bottom dead centre can be started up by steam against full compression and will fire its first oil charge at the end of the first half revolution. That is to say, if you turn on the oil controls of that engine and open the steam stop valve, it will compress the charge of air that is then within it, fire that first charge and give you practically full mean pressure; I would not like to do that on any Diesel engine I have met, but this engine does it very comfortably: it "talks" a little bit about it, but that is all. This shows how readily this engine starts due to its hot cylinder walls. Such a start is, I think, unique, and from other trials we appear to be able to start specially constructed Still engines firing at $\frac{1}{10}$ of full speed with certainty and without fear of obtaining those severe jars which show that the ignition temperature is still insufficient for smooth operation.

The reason for the abject failure of the locomotive designed by Dr. Diesel and produced in Switzerland was that it became so cold by reason of air expansion in starting that it would not ignite oil on injection. It did, I believe, on one occasion start by air and get itself into motion, but it never started a train. You see you are doing two things when you start that way: you are not only starting the engine, but freezing it, which is the worst preliminary you can have for getting it under weigh as a combustion engine; and the result was that that locomotive never started a train unless it was started by a pusher steam engine. I believe it had other troubles, but that one condemned it.

I have been present at many trials run at different temperatures of jacket water—Still-engine trials—and found a curious effect which is that on reducing temperature from the point where you get normal

smooth ignition to the point where knocks occur, the knocks get heavier and heavier until the most violent stage is reached. All knock then ceases and all you can hear is a grumble in the engine, the power produced being then insufficient to raise the speed of the engine when oil is turned on. With increasing jacket-water temperatures there seems to be a well marked period when ignition is insufficient to obtain combustion, succeeded by one where you get very nasty jars. The latter seems to me to be due to spraying the whole charge of oil into the cylinder before combustion has arrived at any considerable stage. It starts to burn reluctantly, slowly raising the temperature of the charge and increasing the rate of combustion until finally what remains of the oil, which is a very large percentage, detonates very violently; thus, what you get is a detonated charge instead of a burned one. Mr. Still

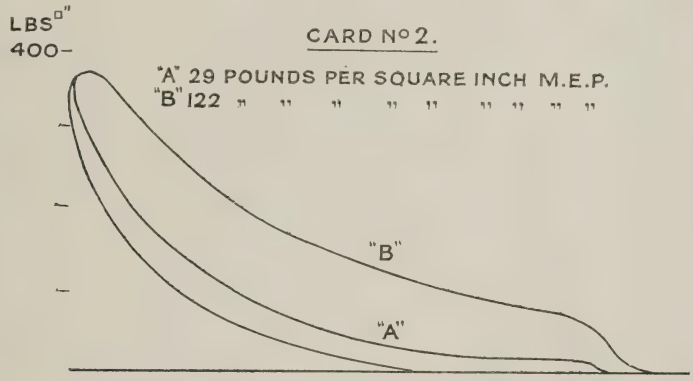


Fig. 10.

The heat generated during the compression stroke is so well conserved by hot walls that we can fire smoothly with a compression of but 180 lb., using blast-air injection as compared with the 550 to 600 lb. usually used, and this enables us to lower our maximum pressures. The result is a loss of efficiency, but the locomotive engine must run smoothly and avoid negative peaks because, when starting, there is no effective fly-wheel to absorb them. The only fly-wheel a direct-coupled combustion locomotive has is its driving wheels; there is not in any case much room in a locomotive for fly-wheels. I am afraid Stephenson never visualized the internal-combustion engine locomotive. Even if we had adequate space for fly-wheels, they would be of very little value at the slow speeds of starting, because at these speeds they cannot absorb much energy.

Card No. 2, Fig. 10, shows what we have been able to accomplish in reducing maximum pressures in one of our engines using air-blast injection. Any power between Cards A and B can be obtained by control of the oil supply.

I have obtained cards of much greater m.e.p. than 140 lb. mean pressure. The small card "A" shows about the minimum that we

Mr. Still

could get regularly in that engine, although smaller cards could be obtained with the fuel valve lifting intermittently.

With a compression pressure of 300 lb. per square inch large mean pressures can be obtained with even lower minimum pressures than those shown, but at the cost of increased fuel consumption per h.p.

These indicator cards resemble steam cards and it seems to me that we have gone a long way towards producing what the Author desires, namely "an internal-combustion engine adapted to a locomotive."

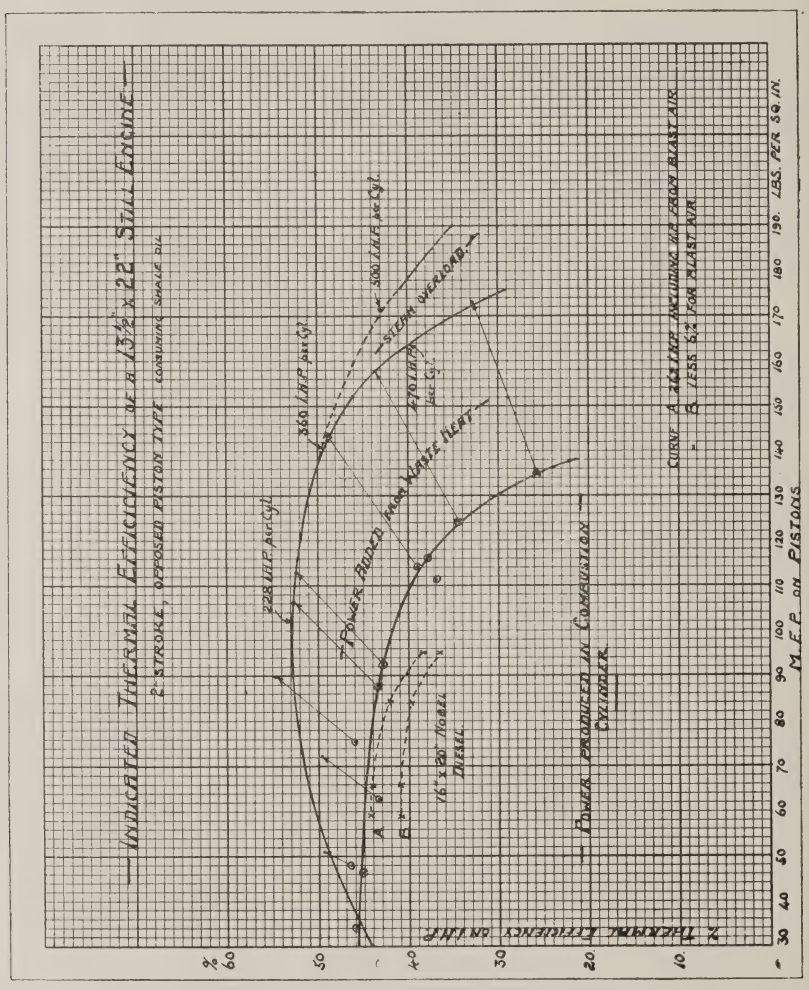


Fig. 11.

On the score of thermal efficiency little need be said of the system I represent; this will be realized from Fig. 11, which gives supervized trial results from one of our high-speed units the indicated thermal efficiency being over 50 per cent. over the range of power it is expected generally to employ. It is evident that there is less loss of efficiency by burning oil under the attached boiler and adding steam to that made from waste heat than one would expect, and on severe overload it increases the overall efficiency to add steam by this means.

It is rather curious that there should be a gain in efficiency by adding to the waste heat steam in the boiler when accounting for the oil consumed under the boiler. The reason is that combustion in the cylinder is rapidly becoming inefficient on severe overload and it is cheaper to add a little steam power at a low efficiency rather than spoil the efficiency of the main engine power by increasing the oil injected into the cylinder. You will see the spots shown are not merely translated upwards by waste heat steam, but also sideways: the gain is not only in thermal efficiency, but also in power. For instance, the increase in mean pressure is from 114 to 144, and we have also increased our thermal efficiency from 39 to 49 per cent. It is quite nice to be able to gain in two ways at once. The result is a very long flat range of thermal efficiency. Mr. Still

You will also see that no steam is added by waste heat at about 36 lb. m.e.p.; this is correct, for at that point we come to the position which the Author mentions, where all we can do is to maintain the high steam pressure on the boiler. These trials are from a single-cylinder opposed-piston engine.

Mr. HAROLD THOMSON: What size is that cylinder?

Mr. STILL: 13½ bore by 22 inch stroke, and the speed is 360 revolutions per minute. It has a normal horse-power of 360, and I have obtained an overload of 540 b.h.p. from it for short periods.

In locomotive service some fall in thermal efficiency from the high standard of this chart is to be expected, but allowing a drop to a fuel consumption of 0·38 per b.h.p. hour, the cost per draw-bar horse-power will compare very favourably with that of any other combustion engine system and be far below steam locomotive costs.

I do not understand why the Author gives the draw-bar efficiency of the directly and indirectly driven combustion locomotives as 65 per cent.; we find that the former type maintains the purely locomotive weight (for equal h.p.) or can even decrease it; in our case it also saves tender weight. Thus the efficiency becomes greater than 65 per cent. The same cannot well be true of the indirect type which contains three units, namely, engine, generator and motors, instead of one.

Consider an electric transmission type, taking Sir Vincent Raven's new electric locomotive as a basis. The weight of this locomotive is given as 102 tons and its h.p. at 1,454; assuming this to be converted to an indirect-type combustion engine of similar power, it will require its present parts plus a combustion engine and generator of about 1,800 h.p.; these will weigh at least 60 tons, and the added platform space and wheels required to support these additions will bring the total weight up to nearly 200 tons, which is far greater than that of a steam locomotive of the same power. This will produce a correspondingly low draw-bar efficiency and increase the net fuel consumption per train mile. It will also increase the first cost of the installation very considerably, and this brings me to a most important point.

Mr. Still

When any prime mover is replaced by one of greater capital cost, the greater outlay is usually warranted by a saving in fuel or an increase in work done. The amount of the saving which it is possible to make depends upon the fuel consumed per annum, and this depends, if considered in terms of capital cost, upon h.p./hours of work done per annum per h.p. installed; it also depends on the cost of the fuel consumed per h.p.

Whilst the steam locomotive does consume a good deal of fuel per draw-bar h.p., it works for very few hours at full load per day. The average steam locomotive does not seem to produce 500 horse-power hours per annum per h.p. obtainable from it at full power; marine engines, on the other hand, work 24 hours for 200 days per annum at full power, namely, 4,800 h.p. hours per h.p. per annum, so that their fuel bill would be 9 times as great as the locomotive engines per h.p. but for their higher thermal efficiency.

Assuming marine-engine thermal efficiency as double that of the steam locomotive, the factor is still $4\frac{1}{2}$ to 1, a practical condition that should not be ignored by combustion—or for the matter of that, steam—locomotive builders because only a very considerable saving in fuel can justify increased capital cost under such conditions.

The whole question is: What is the fuel bill compared with your capital cost? If the capital cost is great and the fuel bill small you cannot save enough out of the fuel to pay for big capital charges. On the other hand, an increase in capital cost can always be justified by increase in work done. If, as I gather from Sir Vincent Raven's remarks in June last, there is urgent need for speeding up the slower-moving but heavily-loaded traffic on our main lines and our draw gear will not enable a greater tractive effort to be applied, there is ample scope for earning a dividend upon an increased capital cost by increasing the ton-miles hauled per annum. This means modifying the well-known tractive-effort diagram which slopes from a high starting peak down to quite a low figure at high speeds, the h.p. remaining fairly constant over a wide range of speed because it is controlled by boiler output.

What appears to be needed is to leave the present starting effort unchanged but swing the high-speed end of the curve up until the h.p. at full speed is about double what it is now. This is what we are arranging to do and it is the natural effect of adding a combustion engine output to a boiler output because on a h.p. basis the former increases as the speed whilst the latter gives constant output. The boiler can therefore aid the combustion engine when the latter can produce but little power and the combustion engine can aid the boiler when its torque falls off because of speed.

We are doing a great deal of work in this direction but it is too early yet for me to show you any concrete results of our locomotive activities. I can safely say, however, that we are engaged in the production of a locomotive capable of giving 1,200 horse-power with an overall weight of about 75 tons; it needs no tender, but carries on the locomotive tanks

for fuel and water. It is to be of the 2-6-0 or 2-6-2 type—something suitable for mixed-traffic haulage. Its starting tractive effort will be as good as engines of its class and its tractive effort at full speeds will be superior to much heavier engines. I am afraid it will cost more than a steam engine, but am hoping it will be within measurable distance of that cost, which can hardly be said of some attempts that have been made to put combustion engines into locomotive service. Some electric transmission types, I am afraid, are going to be quite as costly as electrical-power-house overhead transmission schemes, and that is saying a very great deal. Mr. Still

The PRESIDENT, Sir Archibald C. Ross: Opportunity will be taken later to move a vote of thanks to the Author of this interesting paper, but I do not think we should let the opportunity pass of thanking Mr. Still very cordially for what is really another paper on the subject. I am sure we have all been immensely interested and privileged in the amount which he has thought fit to tell us to-night of the very important work he is doing in the development of the Still locomotive, and as he is now a regular customer here we hope we shall be privileged to have a paper from him on the 2-6-0 or 2-6-2 type when it is built, and if we might have that, then we will get Mr. Williams, the Chairman of the Reading Committee, to make a note of it at once. The President

Mr. J. W. HOBSON, *Member*: The Author can certainly be congratulated on the splendid defensive case he makes out in favour of the steam locomotive against all comers, at the outset of his paper, but it will be noted that towards the end his confidence in its future is somewhat shaken by the attractive possibilities of some of its probable competitors. There is no question that the well-designed and constructed steam locomotive of to-day holds an almost unassailable position but it must be admitted that it has reached the zenith of its development whilst the potentialities of development of other systems of traction make it insistent that railway managers and scientists in addition to the engineer, should give careful consideration and encouragement to the most promising of the alternative systems that are now compelling attention whether we wish it or not. Mr. Hobson

The Author, in the first paragraph, suggests that the advocates of rival systems base their arguments on the question of thermal efficiency only, which is not quite the case, as none knows better than the engineer who has been trained as a steam locomotive man and then seen the possibilities of electric traction or internal-combustion locomotives that other considerations such as reliability, maintenance charges, capital expenditure, etc., must of necessity be taken into consideration. It is the play "Milestones" over and over again. In the old days it was difficult for the established transport authorities to see the advantages of steam over horse flesh and sails, and if the opinion of certain consulting engineers of the period had prevailed, the Liverpool & Manchester Railway would have employed stationary engines instead of locomotives.

Mr. Hobson

The Author states that the factor governing the efficiency of the steam locomotive is the boiler, which is correct; at the same time it might be pointed out that the chief factor making for the inefficiency of the steam locomotive is that 50 to 55 per cent. of the heat value of the fuel is lost in the exhaust steam.

Whilst the fuel bill of a railway company is not the only one featuring on the expenditure side of its account, it is generally to a reduction of the same that attention is increasingly given and in his recent presidential address to the Institution of Locomotive Engineers, Mr. Whitelegg pointed out that if it were possible by economic working to reduce the consumption of coal per engine mile by only one pound a saving of no less a sum than £377,000 per annum would be effected on English and Scottish railways.

No indication is given in the Paper as to how the efficiency of the present steam locomotive can be further improved; however, the Author shows clearly how the thermal efficiency has been improved since the introduction of superheating, and here again it is a question of history repeating itself as records show that as far back as the period 1832 to 1840, Hawthorn's introduced and strongly advocated the adoption of superheating which at that period, unfortunately, was regarded as tending to complicate the then simple machine, and it was not until a later date, when Schmidt, strongly backed by advanced engineers and railway managers, re-introduced the system, that the value of superheating was finally admitted, with the results we all know.

Turning to the question of railway electrification, Sir Vincent Raven's paper of last session, made out a very strong case; with the possible future national developments making for increased efficiency in the power station—particularly as regards the utilization of waste heat—there is no doubt that electrification schemes will demand careful attention.

I am afraid that some of the Author's figures alluding to power-station and electrical-transmission efficiencies, are rather out of date and consequently somewhat understated, but if such is the case, no doubt our electrical friends will give us the benefit of modern experience.

It is appreciated that the cost of change over from steam to electricity will be enormous, but, as there is no likelihood of increased efficiency in the case of the steam locomotive and great possibilities of exceedingly cheap production of electrical power, there is no doubt that electrification is going to make headway. In view of the large reserves held by the railway companies at the present time, it appears that it will not be long before such schemes are put into execution. It must be noted that on every hand the steam-locomotive engineers who have hitherto been the strongest opponents of railway electrification and the adoption of internal-combustion locomotives, are now urging closer consideration and the development of these alternative schemes.

Turning to the question of internal-combustion locomotives, the Author admits that the thermal efficiency of such is most attractive but

apparently considers that investigation of the prime mover is proceeding along the wrong lines. No doubt a directly driven internal-combustion locomotive is the ideal but the difficulties of obtaining the necessary flexibility are so great and developments can no longer be delayed, that no doubt a locomotive having some form of transmission should be concentrated upon. In a recent paper read before the Institution of Locomotive Engineers, Mr. Tritton demonstrated that the most promising lines of development were those in which electrical transmission had been adopted. Furthermore, should the internal-combustion electric locomotive be successfully developed, it would form an excellent transition stage covering the period of change over from steam to complete electrification if afterwards found desirable, as it would be a comparatively simple matter to link up by an overhead line a number of such locomotives working over a system. Mr. Hobson

The question of maintenance must also be recognized as an important one, and whilst it cannot be claimed that other systems would be cheaper to maintain in efficient service, particularly at the outset, the fact must not be lost sight of that the steam locomotive is becoming more and more expensive to maintain as the power and weight factor increases. The chief factor in connexion with the up-keep of the steam locomotive is that due to boiler deterioration and necessary renewals and this particularly applies to railways in certain parts of the world where the water is of such a nature that a boiler has only a life of about 3 years (I have seen longitudinal stays which have been completely corroded through in about 6 months).

In conclusion I should explain that only the Author's close association with myself under the roof of the same firm precludes my speaking of his paper in more laudatory terms and I am desirous of saying at least that I consider this paper a most valuable addition to the Institution's publications, particularly when read in conjunction with Sir Vincent Raven's paper of last session and other papers read recently before kindred institutions.

Mr. KELWAY BAMBER (of the Leeds Forge Co., Ltd.): I am greatly indebted to and thank the Council of this Institution for their courtesy in inviting me to be present at the reading and discussion of Mr. Grime's paper which I venture to think will be recognized as a masterly exposition of the relative values of different forms of motive power to suit the varying conditions and requirements of railways in this and other countries. Mr. Bamber

For heavy suburban traffic electric locomotives will, for well known reasons, rapidly secure and hold the field; but I venture to think that for long-distance traffic, whether passenger, goods or mineral, the superheated simple steam engine will be difficult to rival in general and commercial efficiency and still more difficult to oust.

In his Appendix, page 615, the Author has given some very striking figures of the manner in which the fuel consumption per d.h.p. decreases

Mr. Bamber

with the increases of gross weight hauled behind the tender, ranging and increasing from 278 to 646 tons. One is lead to wonder what further reduction might be expected from gross weights of 1,400 to 1,500 tons such as are common for coal and mineral trains on the railways of the Union of South Africa in the Transvaal where coal trains of that gross weight carry on 23 self-discharging bogie-hopper wagons, 1,035 tons of coal for a tare weight of 414 tons giving a ratio of 70 per cent. for paying load to gross weight compared with 65 per cent. for wagons of 12 ton capacity so largely in use in this country.

Mr. Gresley, Chief Mechanical Engineer of the London & North Eastern group of railways, in a paper on "Wagon Stock on British Railways," read before the Institute of Transport in February last, gave the result of actual practical experiments made by him to determine the comparative tractive resistance of wagons of 10 and 50 tons capacity engaged in the carriage of iron ore and bricks. The percentage of decreased resistance per ton of gross weight for a 1,000-ton train composed of wagons of 50 tons capacity was 32 per cent. and 43 per cent. per ton of paying load, the results in both cases being for trains moving over a practically straight and level track.

The present standard superheated simple mineral 4/82 track engines on the South African Railways exert an attractive effort of 41,000 lb., and as previously stated haul vacuum brake coal trains of 1,480 tons gross weight over gradients 1 in 80 against the load. Their daily duty is to haul that weight a distance of 80 miles out and a full train of empty vehicles the same distance back, the distance covered being approximately 800 miles.

Locomotives of this power and efficiency are possible on many railways in this country, but the real effective use of such power would depend upon a satisfactory relation between the paying load and the dead weight hauled, the attainment of which is again dependent upon up-to-date methods of loading, weighing and unloading, the greater part of which are to-day unfortunately both antiquated and costly.

The President

THE PRESIDENT: I do not wish to appear greedy, but it seems to me that Mr. Bamber too is qualifying for another paper.

Mr. Burn

MR. W. S. BURN, *Graduate*: The subject of Mr. Grime's paper is most important, particularly at a time when the oil engine is coming so rapidly to the fore. It is surprising that the application of the heavy-oil engine to railway operation has been so backward, and it is pleasing to note that Messrs. Hawthorn Leslie's are shortly to have two indirectly-driven oil-engine locomotives on test.

The statement that the Author makes on page 592, namely, that "there is in the majority of cases practically nothing to be gained either in the direction of increased efficiency or improved operation by any change in the type of prime mover employed" is surely a very sweeping statement and one which, if the oil-engine locomotive is

included, cannot, in my opinion, be justified. Fortunately, however, the Author redeems himself later on page 608 by saying that, given a flexible internal-combustion engine, it would only be a matter of time for it to be adopted for every class of railway service. With this latter statement I am in complete agreement, and it was to that end, nearly three years ago, I endeavoured to produce such a flexible oil engine. The result of my investigations was incorporated in the paper "Diesel Engine Flexibility," read before the Institution in January, 1922. I regret, however, that Mr. Grime has criticized my proposal apparently without having carefully examined it. He appears to have chased the shadow and missed the substance in consequence. As he remarks, in my Diesel-air locomotive design, after a speed of 5 miles per hour the tractive effort gradually decreases, due to the air effort falling off. (This applied only when the maximum tractive effort was required for long periods; for short periods additional air could be taken from the starting air reservoirs.) This feature, however, does not affect the principle involved in the slightest degree, as it was possible to make the Diesel cylinders large enough to maintain the maximum tractive effort at *all speeds.* I indicated in the discussion on my paper that the cylinders were on the small side, and I then assumed that the Diesel cylinders gave a constant torque, although I stated in the discussion that overloads at low speeds were possible. Mr. Burn

Since writing the paper, however, I am quite convinced that the oil engine can be made to give the variable torque required for locomotive operation. The conditions required for flexibility of speed also render flexbility of torque or mean pressure possible. The degree of flexibility of mean pressure will ultimately be set by mechanical considerations due to heat effects on the cylinder walls: this will cause the mean pressure to vary inversely as the speed. High mean pressures at low speeds will therefore be safely obtainable.

Within the above limits, provided that—

(*a*) sufficient air is put into the cylinder to burn the oil injected (*i.e.*, to supercharge when high mean pressures are required);

(*b*) the air is raised to a sufficiently high temperature to ignite the oil; and

(*c*) sufficient fuel is injected at the correct moment and in the correct manner (*i.e.*, to obtain a constant pressure diagram),

indefinite variation of mean pressure will be obtainable.

As high maximum tractive effort is only required at comparatively low speeds, good combustion will readily be obtainable.

Turning to my proposal of a Diesel-air locomotive it will be noticed that a supercharge of air can readily be obtained by inserting a throttle valve in the exhaust pipe and increasing the scavenge air pressure by exhausting the air from starting cylinders to the scavenge main. This latter feature is one of the advantages of the air-starting system.

Mr. Burn

Expedients to obtain *b* and *c* are dealt with in my paper on "Diesel Engine Flexibility."

If, then, the diameter of the main cylinders is increased to 12 instead of 11 inches, and assuming that it is possible to obtain double the mean pressure from the Diesel cylinder up to 10 miles per hour, maximum tractive effort could be obtained from the Diesel cylinders alone up to 12 miles per hour. As, however, assistance is available from the air cylinders, the maximum mean pressure required would always be less than twice the normal.

The Author was not correct in stating that my proposal was typical of systems proposed which start by air; that was exactly what it was not! Previous proposals had been made to attempt to propel a locomotive by an oil engine of orthodox design. I attempted, however, to adapt a special oil engine to a locomotive as the Author suggests later in his paper, and the result was the peculiar looking engine illustrated in my paper. In the introduction to my paper I stated that—

> "The chief aim of the writer has been to get to the root of the matter and to obtain ready starting capabilities and flexibility in the Diesel engine itself, dispensing as far as possible with any subsiduary agent such as air or steam although recognizing that the primary start must be made by one of these."

and later on I say—

> "The starting medium may be hot compressed air or steam, the cycle regenerative or otherwise with any incidental advantages, but in the opinion of the Author, the ultimate success of the Diesel locomotive will depend on the flexibility obtainable from the Diesel engine *per se*.

The Author evidently prefers steam for starting purposes as in the Still system; if it were the question of trying to work in an almost orthodox oil engine I should agree with him. I am convinced, however, that the oil engine can be made sufficiently flexible as regards speed and mean pressure; then the starting medium becomes of minor importance.

I therefore prefer a "straight "scheme. In the light of greater experience I should now arrange for solid injection to the main cylinders. the fuel pump being independently driven, tentatively by air, the storage starting air I would keep at 500 lb. per square inch and the auxiliary-compressor engines would be of the cold-starting type. Otherwise the scheme in general would be on the lines originally proposed. I place great importance on heating the starting air which exhausts into scavenging system, as by having the scavenge air sufficiently hot, unlimited flexibility will be obtainable. The jacket water would remain at 212° Fahr., and for starting the jackets would be heated up by the outlet water from the auxiliary engines.

In conclusion, I think all our thanks are due to Mr. Grime for further bringing to our notice such a vital subject. There is no doubt that there is a great market overseas for the oil-engine locomotive, and it behoves us, as an engineering community, to see that this market is supplied.

Mr. JESSEL ROSEN, *Associate Member*: I would like to correct the figure of the average thermal efficiency of a power station over a period of one year given by the Author as 11 per cent., and would refer him to the Electricity Commissioner's report for the year ended March 31st, 1922. The overall efficiency of the Carville Power Station is over 17 per cent., and there are many power stations in the country averaging over 16 per cent. (A power station for an electrification scheme is not likely to be of smaller capacity than group E.) These figures have been obtained during the past few years, and in more modern station with the latest improvements in plant, the thermal efficiency will be further improved, giving a proportionately greater increase in the overall efficiency at the draw-bar of the electric locomotive. Mr. Rosen

With the figure of 17 per cent., the overall efficiency at the draw-bar is increased from 4·4 to 6·8 per cent. This gives the electric locomotive a great advantage over the steam locomotive in coal consumption, and therefore the Author must reconsider his conclusion that, on this score, there is no justification for the electrification of railways. Over and above this draw-bar efficiency there is naturally something to be gained by regeneration which might, in hilly districts, easily result in a gain of 10 to 15 per cent., on the power absorbed by the electric locomotive.

VOTE OF THANKS.

Mr. HAROLD THOMSON, *Vice-President*: There is no need for me to say anything about this paper because it is one which I am sure we have all enjoyed very much. Knowing nothing whatever about locomotives except that they are things that pull trains about, I have found in this paper a great deal of that information which I have often wished I knew when I have seen them doing their work. The Paper deals with the whole subject in a very excellent way, and in addition we have had a very interesting discussion from different people. I am quite sure we are all greatly indebted to Mr. Grime for the great trouble he has taken in writing this excellent paper and I have great pleasure in proposing a very hearty vote of thanks to him. Mr. Thomson

The vote was carried with acclamation.

CORRESPONDENCE.

Col. KITSON CLARK: A very desirable subject for discussion would be found in the various methods of transmission of the force from the power unit to the driving wheels. One trend of the times seems to be towards light, quick-moving, elements actuated by high powers, and the translation of the force involves difficult problems. Colonel Clark

Mr. H. HOLCROFT: The case for the modern steam locomotive has been very ably advocated by Mr. Grime. Those in favour of electric Mr. Holcroft

Mr. Holcroft

traction or other forms of motive power usually take for comparison the published statistics of steam locomotives, of which a large percentage are old and somewhat out of date. The Author of the Paper, however, demonstrates that the modern superheater locomotive is capable of holding its own. I quite agree with him that too much is made of thermal efficiency and not enough attention is paid to all-round efficiency, which includes factors that cannot be directly measured by any standard.

In dealing with the superheat obtained with stopping trains Mr. Grime gives the reason for the drop in superheat after starting as being due to a fall in temperature of elements and header while standing. This can hardly be the explanation, as in any case the specific heat of the metal is small. With the stem of the pyrometer placed in the header I have often seen the gauge indicate a higher temperature when steam is shut off. This rise is not recorded when the stem is placed in the steam chest, so that it is evident that the elements are actually hotter with steam off. Formerly it was thought necessary to fit a damper or draught retarder to prevent the elements being overheated while standing.

I think the true reason for the loss in superheat at starting is to be found in the temperature of the fire-box. When the regulator is closed the rate of combustion becomes very small, and a good deal of the heat from the fire and brick arch is radiated away. After starting it takes a little time to regain the temperature in the fire-box, so that the gases passing through the flues are not so hot. Then again, at starting, steam is passing through the elements in a fairly constant stream, whereas the velocity of the flue gases varies greatly between each exhaust beat. With each puff of the steam the gases pass at high speed and scrub the elements, producing superheat, while the next instant they are almost stationary and impart but little heat to the flowing steam. Thus, the mean temperature of the superheated steam is lowered. It should also be remembered that the boiler steam is usually a little wetter at starting, owing to the irregularity of the demand by the cylinders and also to the higher water level resulting from the injector filling up the boiler while steam is off.

The remarks on steam consumption of locomotives need a little qualification. Some allowance should be made for auxiliaries, such as brake ejector or air pump, steam for train heating, safety-valve losses, blower, boiler leakages, engine leakages—pistons, piston valves and glands. The figures given for steam consumption per i.h.p. are apparently the results with a full regulator and with engine in first-class condition. In actual practice the use of a full regulator is the exception rather than the rule, the cut-off being lengthened. Steam or coal per i.h.p. is not the true measure of a locomotive: draw-bar horse-power is the criterion. It may so happen that a longer cut-off and smaller steam-chest pressure produce, in conjunction with the inertia of the reciprocating parts, a more even turning moment on the axle and also conduce to steadier running, with the result that the internal resistance of the engine is at a minimum, and therefore a larger percentage of the

indicated horse-power reaches the tender draw-bar. The least consumption per indicated horse-power does not necessarily coincide with the least consumption per draw-bar horse-power. I have never found any appreciable difference in coal or water consumption between superheater engines worked with a full regulator and short cut-offs and those in which the cut-off is set at say, 33 per cent. and the regulator only partly opened. A lot, of course, depends on speed, horse-power, output, type of valve gear, etc.; and also, what suits one engine does not necessarily suit another. Mr. Holcroft

In discussing future developments the Author gives three possibilities: (1) condensing steam locomotives; (2) electrification; (3) internal-combustion engines. I will add a fourth, namely, steam engines with surface generators in place of boilers. The modern steam locomotive in its present form is limited by its boiler because weight restriction and loading gauge make it very difficult to carry a larger or heavier boiler on the frames. A large diameter requires thicker barrel plates and so does higher pressure, leading to increased weight. Higher pressures are associated with higher temperatures, and boiler repairs are increased thereby. Since pressure is restricted, cylinders must be comparatively large, and compounding is not resorted to because in Great Britain there is no room outside the frames for the big low-pressure cylinders usual on the Continent and in America.

In the running shed it is the boiler that requires nearly all the attention and repairs. Great care must be exercised, steam raised slowly to avoid strains, and the same care observed in cooling down after a run. Washing out every few days absorbs much time and labour, and many careful records have to be kept of every repair. Fire cleaning, removal of smoke-box ash, cleaning ashpans and tubes all add to the labour costs and require time. The consequence is that the locomotive can only operate for a limited period each day.

If we can successfully replace the boiler by a surface generator composed of tubing or other construction capable of resisting large internal pressure, a new field is at once opened. Pressure can be increased considerably, so that smaller cylinders can be used and compounding adopted with greater prospect of success, especially as it becomes possible to re-heat the steam between the stages. The steam generator no longer being confined to a circular cross section for the main part can be enlarged to fill a rectangle. The absence of any water, as in the boiler barrel, saves weight and allows of greater surface for the generator; hence, there is more horse-power. The problem now begins to narrow itself down to the question of fuel and how to burn it.

The multitubular boiler and its fire of solid fuel have a large measure of flexibility. The rate of combustion is largely automatic, as it is nearly proportional to the discharge of exhaust steam in the smoke-box. A certain amount of control over the fire is exercised by the damper and admission of air at the fire-hole door. The main flexibility, however, is derived from the large quantity of hot water in the barrel.

Mr. Holcroft

which acts as a reservoir. A further control is obtained by the regulation of the feed water. The locomotive can therefore withstand a considerable overload or, on the other hand, it can store up energy at times by increasing the quantity of hot water.

With a surface generator this flexibility is totally absent, and the whole of the variation must be derived from the rate of combustion of the fuel. There are several combustibles with which this could be accomplished: (1) liquid fuel; (2) pulverized solid fuel; (3) colloidal mixtures; (4) gas producer using solid fuel. In view of the fact that the bulk of oil is imported into the country the first is not altogether a very desirable proposition. The difficulty with pulverized fuel lies mainly in its storage and distribution. No doubt these troubles will eventually be overcome. Colloidal mixtures require a certain amount of oil but they obviate the difficulties with the pulverized fuel. The possibility of developing a gas producer capable of using low-grade fuels should not be lost sight of. With any of these proposals flexibility is secured by regulating the supply of fuel to the burners and by varying the number of burners in use.

Having eliminated the boiler, the steam locomotive loses most of its disadvantages. There are great savings in time and labour. Repairs are reduced considerably, there is no clinker or ash, no washing out or tube cleaning: the time spent in shed is very much less, there being no long period of steam raising, so that the engine can be out on the road more. A great increase in horse-power output would be secured, while retaining the characteristics of the steam locomotive for simplicity, reliability and maximum starting effort. In its new form the steam locomotive need not fear competition from the electric locomotive and the internal-combustion engine, both handicapped with heavy capital charges. Ultimately the problem resolves itself to fuel, and how to burn it.

Mr. Lucas

Mr. S. J. LUCAS (Airedale Foundry, Leeds): The competitors of the steam locomotive claim as an advantage over it, that their thermal efficiencies are greater; and this raises the question why, with these advantages, they do not "out" it quicker than is occurring. Its most serious competitors are the electric and the prospective heavy-oil locomotives, and it will no doubt be conceded that of the three types the steam locomotive is cheapest and simplest for equal powers; so that the thermal efficiency has to be paid for in first cost. All these types have their respective first costs increased by what may be called an "operative" efficiency, measured by the cost of maintenance of upkeep and certainty of action. This might perhaps be expressed by asking: Which type will maintain its original efficiency the longest without attention?

The steam locomotive has its picturesque appearance and the electric locomotive has æsthetic advantages which are most readily realized in the small powers of street and suburban services; but how will the heavy-oil

locomotive be appreciated in sight, sound and odour? The advantages or disadvantages in these directions may also be measured by what may be denoted as a "comfort" efficiency, which will probably be determined by the travelling public. Perhaps Mr. Grime could express an opinion on this. Mr. Lucas

The sum of these efficiencies in combination with commercial efficiency will possibly decide how the total increased efficiency of railway locomotives may be obtained.

Electric, like compound steam locomotives, have their greatest efficiencies in services which are long and uniformly continuous, both in load hauled and resistance overcome, and for which the mechanism can be specially designed, as is the case in marine engines.

It may be of interest to note that the Delaware, Lackawanna & Western Railway recently proposed to electrify part of their line, but the cost was found to be prohibitive, and a large increase in the number of steam-locomotive units and their power has been adopted in its place.

These general remarks are submitted for consideration in conjunction with the appropriate points of the Paper. Regarding the internal-combustion locomotive for main-line services, I would advise perusal of Mr. Julian Tritton's paper on "The Internal-Combustion Locomotive for Railway Work," which was read before the Institution of Locomotive Engineers in London on January 25th, 1923, in the discussion on which I submitted some considerations.

Mr. G. A. MUSGRAVE (London and North Eastern Railway Company): The points put forward in the Paper are of the utmost importance and undoubtedly will have to be considered sooner or later as a great practical and commercial proposition. The Author rightly states that the steam locomotive has, up to the present maintained its position against its rivals, and taking into consideration the general conditions of railway working, varied traffic—fast and slow, heavy and light—with safety, minimum cost of working, and maintenance, there is every reason to believe that it will maintain that position for some time to come. Mr. Musgrave

Superheating has undoubtedly brought the steam locomotive to a much higher level and has enhanced its merits by far more than could otherwise possibly have been obtained, both for long distance and local working, although, as stated by the Author, not so much in the latter as the former. The fact that engines fitted with superheating apparatus have been built in great numbers for suburban working proves the utility of such machines. I feel, therefore, that I must argue against the figures quoted by the Author as indicating that the coal efficiency of such engines on suburban workings falls so low as 3 per cent.

The factors governing a fair standard efficiency of locomotive boilers, may, in my opinion, be put in order of merit as follows:—

1. Combustion.
2. Firing.

Mr. Musgrave

3. Coal.

4. Manipulation of air.

5. Working of injectors and regulator.

The first mentioned is governed by the design of the boiler; the second is dependent upon the individual employed; the third is a commercial proposition; the fourth and fifth, again, are conclusively dependent upon the human element. In the case of items 2, 4 and 5, there is the possibility of error to the extent of making an inefficient boiler and machine.

With regard to compounding, the first cost and complication of a saturated compound engine is great, and in this country it has not shown such efficiency as to warrant its being used extensively and it does not adapt itself to the variation of loads and traffic as do the superheated engines. From observation of compound superheater engines—with which personally I have had no more experience than the ordinary layman—the complication would appear to be enormously increased, thus adding to maintenance. I have seen many of these compounds at work in France but really have seen nothing in their performance in excess of that of the superheater engine.

With regard to the lines upon which development may be expected, railway electrification is the next proposition put forward and truly has received a great amount of attention and costly experiment in the past, with a corresponding good result in consequence, though one must be ready to admit that this experiment is in its infancy and may to some extent be made a more tempting proposition, though personally I fear that that time is not yet.

With the foregoing suggestion in mind, it is well to remember that in addition to proving their efficiency for power what has to be proved of these machines, is whether they will be able to endure the conditions under which the prime mover of to-day has to operate, *i.e.*, rough handling, long periods of working, and unavoidable inattention to the repairs. Before condemning the steam in favour of some other type of locomotive the latter must undergo very severe criticism. The possibilities of increased efficiency are many, but the probabilities are few.

Mr Rowland

Mr. WILLIAM ROWLAND (of the London and North Eastern Railway Locomotive Department): The Author's statement that the principal factor governing the efficiency of the steam locomotive is the boiler, is a little too general, since it is one particular portion of the boiler, *i.e.*, the fire-box which is mainly responsible for efficiency. The boiler tubes, cylinders and valve motion exert quite a secondary influence on the performance of the engine compared with the fire-box. Unless the fire-box can completely burn the coal the ultimate efficiency of the engine is adversely affected from the beginning and no amount of extra efficiency in the other parts can go very far to make up for the failure of combustion. This is fairly obvious when it is remembered that a

combustion efficiency of as low as 60 per cent. is quite usual when a locomotive is working at full power. Mr. Rowland

Almost any usual set of tubes will absorb anything from 80 to 90 per cent. of the absorbable heat (*i.e.*, heat at above steam temperature) offered to them, and mere extent of tube heating surface does not in any way affect their capacity to do this. The experiments of Isherwood in New York as long ago as 1864 proved conclusively that cutting off three quarters of the tube surface of a boiler whilst keeping the rate of combustion constant was absolutely without effect on its evaporative efficiency, the only difference being the extra draught required in the chimney as the flue-way was reduced. It is difficult to understand why these, as well as other trials which prove the same thing are so generally ignored, and why we still quote heating surface, which is really a by-product of flue-way, as if it were a vital factor.

The Author goes on to suggest that rate of firing per square foot of grate is of the greatest importance in determining efficiency of combustion. It is perfectly true that successive increases in the rate of firing per square foot of grate in any particular fire-box are followed by corresponding decreases in the efficiency of combustion but it has been experimentally proved that it is not the rate of firing per square foot of grate that matters, but the rate per cubic foot of fire-box volume. Some years ago experiments were carried out on the testing plant of the Pennsylvania Railway at Altoona in which a locomotive was tried first with its full grate area, then with three quarters of it and finally with only half, the rates of firing per square foot of grate being in the ratio of 1, $1\frac{1}{3}$ and 2, and the total coal fired per unit of time the same in each trial. The results so far as total evaporation and evaporative efficiency were concerned were absolutely identical, though, of course, a slightly heavier draught was required to burn the same amount of fuel on the smaller grate. Yet we still hear rate of combustion per square foot of grate solemnly quoted as a governing factor.

As a matter of fact there is a tendency for slightly higher over-all efficiency with high rates of firing per square foot of grate than with low, on account of the smaller percentage of excess air used which raises the gas temperature and presents the tubes with rather more absorbable heat in the gases than is the case when more excess air is present. This results in a rather higher chimney temperature but owing to the smaller weight of gas per lb. of fuel fired, the total heat lost to the chimney is not so great as when the rate of firing is lower and the percentage of excess air is higher.

The efficiency of combustion is also governed to some extent by the ratio of the water-cooled surface of the fire-box to its volume, and therefore not only its volume but its form enter into the question. If the fuel were solid and free from all volatile combustible matter so that it could all remain on the grate until completely consumed or at any rate until each piece were reduced to such small dimensions as to be carried away by the draught, the fire-box volume and shape would be

Mr. Rowland

of little moment. Actually, however, the fuels with which we have to deal have a very large proportion of their combustible constituents in the volatile form; some of them are entirely volatile, and these must have time given them in the fire-box for combustion, which time can be afforded either by providing a sufficiently large volume, and if that be impossible as in the locomotive it often is, by so designing the fire-box as to reduce as much as possible the ratio of its water-cooled surface to its volume.

Fortunately mere increase in the size of a fire-box of any particular shape automatically involves a smaller ratio of water-cooled surface to volume, and from this it follows that increase in the size of a locomotive is generally accompanied by increased evaporative efficiency. The small dimensions of the fire-boxes of the very early locomotives made them unsuitable for the use of bituminous coal at high rates of firing, and the steam-wagon boiler affords an example of the same disability, when fired at all hard.

The absolute necessity for large fire-box volume per lb. of fuel fired is particularly evident in the case of pulverized coal which must necessarily be all burned in suspension, and the enormous furnaces now provided for this fuel by American engineers show that they quite appreciate what is required.

On the other hand one sees only too often locomotive fire-boxes intended for the use of bituminous coal, of which anything from 30 to 50 per cent. is burned in suspension, designed as if it were the aim of the engineer to give the maximum water-cooled surface and the minimum volume per square foot of grate.

It would therefore seem that one of the most possible methods of increasing locomotive efficiency has yet to be thoroughly worked out. The locomotive is, as the Author says, a very efficient and simple transport appliance, but in far too many cases its possible efficiency is crippled by poor digestion at its higher and more useful powers. The subject is one that should well repay thorough investigation.

Mr. Smeddle

Mr. J. H. SMEDDLE (Running Superintendent, London & North Eastern Railway, North-Eastern Area): In my opinion the Author does not disclose sufficient evidence to warrant his opinion that "there is practically nothing to be gained either in the direction of increased efficiency or improved operation, by any change in the type of prime mover employed." I agree with him, however, that the question of thermal efficiency may be over-rated, and that the real basis is commercial efficiency.

The extension of electric traction in this, and other countries, is, in my opinion, absolute proof of its commercial efficiency under some, if not all, conditions of transport. With regard to the advantages claimed for railway electrification enumerated by Mr. Grime on page 600 of his paper, I would suggest that an important factor of economy in the use of electric locomotives is the lower cost of shed and general repairs

which, from our experience over a number of years, shows a very decided improvement in favour of the electric as compared with steam locomotives. Mr. Smeddle

I should expect the thermal efficiency of an engine, tested on a test plant, to be much greater than the same working under everyday conditions, as the loss of heat, due to radiation, is very much less in the first case than in the second. Then again, on test plant experiment the coal consumed while the engine is running, is used to obtain the thermal efficiency. In practice the steam engine burns coal from the time it is lit up to the time it returns to the shed, and often for one hour's running the fire is actually burning three hours, due to lighting up in the shed, coasting, and standing, and to obtain the commercial thermal efficiency, this would have to be taken into account for average conditions.

When an electric engine is standing or coasting, no coal is used so that if their overall thermal efficiency was the same when doing the same work, and the cost of coal per ton used at the power station was the same as cost of coal used on a locomotive, the cost of coal used for lighting up, and coal used while standing and coasting, would be saved, in addition to the cost of water on the tender.

I assume that the Author means by tractive effort, the pull that the engine can exert on the draw-bar on a level road. If this be so, then the Author (on page 601) compares the horse-power (draw-bar) per ton of weight of the electric engine with his rough estimate of the indicated horse-power of steam engine and tender. As a matter of fact the N.E. electric engine can exert up to 1,700 draw-bar horse-power when going at speeds from 40 to 60 miles per hour, with a mechanical efficiency of about 80 per cent. from collectors to draw-bar which makes the i.h.p. 2,130, and the i.h.p. per ton of weight $\frac{2130}{102}$ or 24·5.

The mechanical efficiency of the electric engine is 80 per cent., and not 57 per cent., as estimated by the Author on page 601. Then taking the Author's figures of efficiency of transmission from power station to collectors as being correct, the thermal efficiency of the draw-bar is 7·68 by 0·8 or 6·14 instead of 4·4 (see page 601).

Mr. J. C. ARTHUR, *Chairman of the Graduate Section*: Mr. Grime is to be congratulated upon what may well be termed a very gallant defence of the steam locomotive. In some of the comparisons made with the electric locomotive, however, he makes statements which, to one who is not a locomotive engineer, appear to be a little unfair to the latter. In correcting the power developed by the locomotive described in Sir Vincent Raven's paper, for traction resistance losses, Mr. Grime evaluates these losses at 35·18 lb. per ton the figure given by his curves for a steam locomotive. Since the horse-power of electric motors is normally expressed as b.h.p. it is difficult to see why the resistance to traction per ton of this locomotive should exceed that of ordinary rolling stock, plus the gearing losses. Assuming the low figure of 90 per cent. Mr Arthur

Mr. Arthur

for the efficiency of the gearing and the figure of 15 lb. per ton resistance, the loss in the electric locomotive would be 1,700 lb. total which corresponds to about 80 per cent. draw-bar efficiency and 6·14 per cent. overall thermal efficiency which is about 40 per cent. better than that which could be expected from a superheated steam locomotive. A somewhat similar correction to the above would appear to be necessary to Mr. Grime's horse-power-per-ton comparison; he does not state whether the horse-powers given are i.h.p.'s or b.h.p.'s though that for the steam appears to be the former.

In general, in dealing with the claims of electrification, Mr. Grime appears to have assumed, though he does not say so, that the power stations supplying the current are burning coal, and to have taken very low figures indeed for the station, transformation, and distribution efficiencies. Even admitting these figures, however, if the power station be run on water power—which power, incidentally, has a fuel bill made up almost solely of interest on capital and maintenance charges—the final efficiencies given in the table on page 616 must immediately be multiplied by 7. If the Diesel-electric locomotive be carried to its logical conclusion with the Diesel engines in the form of large units situated in the power station instead of on the locomotive, the final figures are forthwith increased threefold, while with the use of gas engines and producers running on coke, the efficiencies would be doubled.

I cannot altogether agree with Mr. Grime's idea that the internal-combustion engine should be adapted to the locomotive rather than the locomotive to the internal-combustion engine. Early locomotive history relates that an attempt to suit a steam engine to transport requirements by fitting it with legs, ended disastrously "whereby two spectators were killed." Were the internal-combustion engine inflexible so far as torque is concerned and perfectly flexible so far as speed is concerned, the problem of the internal-combustion locomotive would have been solved long ago.

Actually it possesses an almost perfect torque flexibility while being fundamentally less flexible in speed than a steam engine. What is required, therefore, is some form of transmission gear—on the score of compactness, simplicity and efficiency preferably not steam, air or electric, or clutch—that will permit of the internal-combustion engine running at a speed consistent with steady operation, when the locomotive is starting, while transmitting either its full horse-power, or in the case of a clutch, its full torque, to the driving wheels.

Mr. Grime states that the locomotive designed by Mr. Burn fails to some extent because its tractive effort at 3 m.p.h. is little more than half that at starting. If one refers to to Fig. 3, however, this would appear to be almost precisely the variation required to coincide with the train resistance curves.

The Appendix to the Paper is of great interest as showing what excellent results can be achieved by the modern steam locomotive. There is, however, rather a startling discrepancy between the results obtained

upon the outward and upon the return Manchester-Blackpool run quoted. There does not appear to be a sufficient difference in level between the two towns to account for the variation, and, even admitting the difficulties of accurate observation of locomotive performance, to average two results differing by as much as 30 per cent. appears to be a very questionable proceeding. It would be of great interest to have Mr. Grime's opinion upon this point. Mr. Arthur

Mr. JAMES CALDERWOOD, *Graduate*: On page 610 there is a summary of trials of a London and South Western 4-4-0 type locomotive. These, Mr. Grime states, show a good performance, but one which he does not appear to consider exceptional. A careful analysis of these trials, however, shows a performance so remarkable that one can hardly accept the figures as being correct. The efficiency ratio of the engine compared with the theoretical efficiency is about 75 to 80 per cent., a figure which, I think it may be safely said, is quite unattainable in any single-expansion engine. Again, the boiler efficiency given in this case is about 75 per cent, and this is a figure which Mr. Grime appears to consider quite normal; yet it is an efficiency that would be considered good with a low rate of firing (say 20 lb. per square foot per hour) in a more efficient type of boiler. It is remarkable that Mr. Grime gives almost exactly the same figure for the efficiency of both locomotive and electric-generating-station boilers, though the latter are of a more efficient type working under much more favourable conditions. Mr. Calderwood

On page 616 there is a table giving the efficiency at various loads of the electric locomotive. Mr. Grime appears to have again accepted published figures as correct without examining them. The table apparently assumes that one power station and transmission system will be used to supply each separate locomotive as the power-station efficiency drops considerably with the variation of load on the locomotive; such a drop of efficiency does not occur in the more usual arrangement where one power station supplies current for many purposes.

A more reasonable table of approximate values would be:—

		100 per cent.		50 per cent.	
Power station	...		14		14
Transformer (step up) ...	...	98	13·72	98	13·72
Transmission...	...	95	13·03	95	13·03
Transformer (step down)	...	98	12·79	97	12·79
Transmission (low tem.)	...	90	12·52	90	12·4
Motor A.C....	...	91	11·4	88	10·9

This table shows a much more favourable case for the electric locomotive on thermal efficiency. In comparing electric with steam locomotives Mr. Grime does not consider the great difference in cost between the coal used at the power station and that in a locomotive, but

Mr. Calderwood when the relative advantages of steam and oil engines are considered Mr. Grime brings in the fuel cost, as of course he should.

On page 599 Mr. Grime states that turbines are unsuitable for locomotives on account of their low starting torque; he seems to have confused power with efficiency, for while the efficiency of a turbine when starting will be exceedingly low the actual starting torque will be high.

On page 602 Mr. Grime has compared the weight per h.p. of steam and electric locomotives, and he appears to have taken i.h.p. in the former case whereas in the the latter the h.p. has been calculated from tractive effect; this surely is unfair to the electric locomotive.

The statement is made on page 606 that "the internal-combustion engine as at present used is largely a constant-torque proposition." This, of course, is not the case, the torque being variable between zero and the maximum attainable. The direct-driven oil engine locomotive is at present impossible unless the Still engine or a proposal such as that suggested by Mr. Burn be adopted. The disadvantages of either of these schemes is the complication involved. The indirect drive, either through fluid or electric transmission systems, suffers from the same disadvantage. There is no doubt that the most important requirements of an oil-engine locomotive are, as Mr. Grime states, simplicity and reliability, and the way in which these can best be obtained is, I think, by driving through a hydraulic clutch; the latter is exceedingly simple and reliable in operation and would, in any case, only be in use at speeds below 5 m.p.h., when it would be allowed to slip. As a hydraulic clutch is usually fitted with an additional dog-clutch direct drive is obtained when no slip is desired; that is, at all except the lowest speeds. With this type of drive the engine would, of course, be started first and then gradually clutched in when the locomotive was required to start. The above transmission in conjunction with a two-stroke oil engine with solid injection would give a locomotive as simple and reliable as the present steam locomotive, at a cost little if any higher than that of the latter.

If an experienced locomotive engineer could be persuaded to work in conjunction with a first-class oil-engine designer there can, I think, be little doubt that we should shortly have available oil-engined locomotives which could successfully compete with the steam locomotive or with electric traction for all classes of traffic except, perhaps, suburban lines where electric traction would probably prove cheaper.

In conclusion, I wish to thank Mr. Grime for the large amount of information made available by his paper.

Mr. Sisterson Mr. G. R. SISTERSON: I was on the technical staff of the L.S.W. Railway when the trials of that Company's locomotives, referred to in the Paper, took place. The excellent results quoted by the Author were those actually obtained. Every precaution was taken to ensure accuracy: all the operators were skilled in the testing and handling of locomotives under running conditions.

The loads and timings were, of course, not excessive, and the engines were consequently not over-loaded; but the trials took place with trains under ordinary schedule and with loads determined solely by the traffic department. Mr. Sisterson

The results show conclusively that under the given conditions no prime mover on variable load can equal the steam locomotive for efficiency—reduction of power, of course, being obtained by shortening the cut-off with consequent automatic regulation of the blast. In the electric motor reduction of power is secured by switching resistances into circuit, which entails waste and is not so perfect as the method of the steam locomotive.

The results of these trials were communicated in the usual way to the Institution of Civil Engineers, and have never been challenged.

Mr. C. S. DARLING, *Associate Member*: The electrification of railways in this country is looming on the horizon, and it will probably remain there for some time as it is a project that needs careful consideration in many respects, and if adopted will not prove an unmixed blessing. Mr. Darling

While it is true that most, if not all, of the past electrification work has been done for reasons apart from the expected increased financial return, it must also be admitted that here and abroad electrical locomotives have handled traffic as reliably and in many cases more advantageously than steam locomotives. Published records of the NY.NH & H. NYC.R. & P.R.R., to take American railways only, show decreased expenses, decreased repairs and better service. However, in view of the heavy expenditure necessary it is not wise to accept any statement either way without reference to the local conditions of any project. It should also be borne in mind that (1) it is at present the exception to find a power station with an average thermal efficiency over 12 per cent. and (2) it will be a long while before all the power stations come up to this figure and a long period will elapse before we reach the height of efficiency of some of our new plants—in the neighbourhood of 16 per cent.—and (3) it is very unlikely that as long as we use steam the overall efficiency will increase much above that figure, since to do so means increased capital expenditure out of all proportion to the gain, and recourse must be had to large generator units which have not proved exceptionally reliable and which represent huge capital sums which periodically lie dormant while overhaul or repair takes place. (4) The drop in efficiency at light loads is large. (5) The no-load steam consumption is large. (6) Power stations employ auxiliary machinery which does not reach the efficiency of the main units.

One hope for the electrification of railways lies in the employment of substations equipped with synchronous-motor generator sets. Since the greater portion of the present load of supply companies consists of induction motors with a large lagging component current involving capital and operating costs in excess of the corresponding cost of equal

Mr. Darling

energy supplied at unity power factor, it often pays companies to instal large synchronous motors and run them idle with strong excitation to neutralize and correct the lagging component of the current consumed by the induction-motor load. If the railway companies could be persuaded to incur the expense of installing these machines when supply is taken from a.c. mains, it is justifiable to assume that exceptionally low prices could be agreed upon for the current supply. It is essential that the railway companies co-operate with the supply companies in order to obtain the best results.

Before committing ourselves to any electrification scheme it is well to consider the other alternative proposed by the Author, *i.e.*, the internal-combustion locomotive.

For heavy loads at slow speed the steam locomotive is very inefficient and at high speeds is limited by the stoking capacity or the fire-box capacity, whichever is the smaller. The internal-combustion engine of to-day demands a transmission system to adapt it to locomotive work, and as long as the loss due to transmission is sufficiently low to keep down weight and gear the increased efficiency of the internal-combustion engine will allow the whole combination of wheels, transmission, and prime mover to become a commercial proposition since we will not be limited as the steam engine is limited at low speeds, and can exert continuously maximum horse-power at one end and vary speed and load at the other. The combination demands a prime mover to give maximum power for minimum weight.

In this connexion it can be noted that ten years ago Messrs. L. Nobel Ltd., constructed a 200 b.h.p. four-stroke engine weighing 22 lb./b.h.p. without utilizing any light-weight alloy or heat-treated steels. Progress in this direction is, however, slow. The writer has figures relating to a submarine engine constructed in 1913, which works out at 58 lb./b.h.p. including air bottles, piping, exhaust silences, etc., and has taken out net weights of over a dozen engines of the Germania Werft which vary between 35 and 45 lb./b.h.p., the former relating to four-stroke engines of high speed and the latter to slower speed engines. There weights do not include auxiliary plant or piping. It seems that a submarine engine could be made to form the basis of a design, and weights could be reduced by using opposed pistons, and high speed with a dynamo rotor constructed to give the necessary fly-wheel effect. The electrical equipment of existing electric locomotives cannot be reduced much below 50 lb./b.h.p. of continuous output at the drives, so that we have a total mass of gear of—

Engine.		Generator.		Motor.	
30	+	50	+	40	= 120lb./h.p.

at the least, to replace the boiler and gear of an ordinary locomotive.

Electrical transmission would appear to be the only one for large locomotives, and could be made to affect the first step from unit locomotives to central power distribution by linking locomotive generators

by an overhead line. Pneumatic transmission, hydraulic transmission and other systems are only speed changes and do not provide for variable power transmission. Mr. Darling

Direct driving by internal-combustion engine cannot be successful unless a secondary medium is used for a portion of the speed range, but the writer believes that eventually we will be able to control the process of combustion and reduce the percentage of negative work so as to obtain the flexibility of the steam engine. The use of the "Brayton" cycle would go far to giving us an indicator diagram similar to the steam engine, but it is only suitable for gaseous fuels, and in that respect is inferior to the Diesel cycle. The writer's idea is to transfer the negative work and combustion processes to an external compressor and combustion chamber, and with that object in view to prepare special cylinder and combustion-chamber arrangements which could be worked in conjunction with a gas producer. The National Physical Laboratory have already developed an apparatus for combustion processes which controls combustion as desired and this could be adapted to suit the present purpose.

The Still system does not seem to offer any great advantages over an orthodox locomotive since, the writer believes, the boiler capacity has not been reduced more than 50 per cent. and this reduction will not entail any appreciable reduction in maintenance. Another point about the Still locomotive is that it is not a closed system; the steam exhausts in the normal manner into the atmosphere and thus a goodly supply of water is essential which would disqualify the scheme entirely for arid regions. The reduction in efficiency of the internal-combustion side would only be slightly compensated for on the steam side and the complication involved would not be justifiable if the fuel consumption is compared with a normal internal-cumbustion engine although it might be on comparison with a coal-fired steam locomotive. Further, it does not seem to matter to the writer whether we use air or steam for starting provided we do the starting expansion so as not to cool the cylinder too much. Furthermore, since boilers are not always above suspicion, a failure of a tube would hold up the whole cooling system and cause an out-of-balance effect, and the time taken to raise steam to start would not be greatly under the time taken at present.

Although the Paper deals with main-line locomotives, it would be of interest to state the present status of the steam locomotive with regard to industrial work. At this time when most works use electrical power it is somewhat antiquated to have the steam locomotive wasting coal for occasional haulage duty, and the battery locomotive is slowly but surely taking its place as its running cost is less than 50 per cent. of the steam engine's, and furthermore it can be made to improve the load factor of the works system by taking power for charging at periods of no load, *e.g.*, mid-day stop. Other advantages too numerous to mention and other than financial can be gained by the adoption of this system of traction in industrial plants.

Mr. Mills

MR. F. MILLS, *Hon. Secretary of the Graduate Section*: In view of the Author's opening remarks it is rather surprising that he makes no suggestion for improving the efficiency of the steam locomotive. With his final conclusion, that each form of prime mover should be used in the class of work to which it is best suited, we must all agree.

The logical development of locomotive efficiency, viewed as a commercial problem, would appear to be as follows:—

1. Further development of the simple expansion locomotive. Increase in boiler efficiency, and reduction or elimination of idle tender weight in order to increase draw-bar efficiency, offer the most obvious lines of development.
2. Development of other forms of steam locomotive in order to utilize existing coal and water stations while gaining in efficiency.
3. Complete change in the form of prime mover employed, *i.e.*, electrification or the adoption of internal-combustion engines.

It is often stated that the simple expansion locomotive has reached the utmost limit of its development and that it is as large and powerful as the loading gauge of this country will allow. Few, if any, locomotive engineers really believe this. Much more powerful machines could be built were they demanded. As the Author has indicated, efficiency of combustion is one of the weakest points in the steam locomotive's economy, being rarely higher than 75 per cent. when the engine is working at full load while excess air ranges from 30 per cent. as a test minimum up to 100 per cent. and over in actual working. To the writer, powdered fuel seems to offer the best possibilities of relief in this direction. So far as this country is concerned, oil burning on locomotives is not commercially practicable, and, even though it were, considerable courage would be required to advocate the "destruction of oil under boilers" before this Institution where so many gentlemen have already pointed out that oil ought to be treated kindly in internal-combustion engines. In view of Dr. Dunn's recent paper* little need be said here on powdered coal. This form of fuel has been used to a fairly wide extent on locomotives in America with very encouraging results. In the writer's opinion the Unit System is most suitable for railway purposes though it has yet to be adapted to this service. The cost of erecting central pulverizing stations and fitting tenders with air-tight tanks for carrying powdered coal appears prohibitive.

The steam consumptions given in the Paper confirm those recommended as bases for design by the American Master Mechanics' Association, viz., 20·8 lb./i.h.p./hr. superheated, and 27 lb./i.h.p./hr. saturated. The writer does not agree with Mr. Grime in assuming a flat rate of evaporation over the whole of the boiler heating surface. The size and disposition of tubes and the extent of fire-box heating surface have a profound effect on total evaporation and on boiler efficiency. The charts given by the

* "Powdered Coal as Fuel," *Trans.*, p. 571.

Author may afford a ready means of making approximate comparisons of thermal efficiency between steam locomotives of *similar design*; but it is doubtful whether they can afford accurate comparisons with other forms of prime movers. The thermal efficiency of the engine cylinders, as the Author has shown, ranges up to 11 and 12 per cent. in actual practice, and improvement seems hardly possible. The boiler efficiencies quoted in the Appendix are remarkable. It is not usual to regard a locomotive boiler as equal in efficiency to a power-station boiler. While it is realized that the figures given by Mr. Muhlfeld are quoted in good faith the writer would suggest that much better figures could be obtained nearer home in everyday practice. In this connexion the Author might also have substituted his own figures for cylinder efficiency in place of those given by Mr. Muhlfeld. One feels that the comparison has not been carried as far as the Author's knowledge of the subject would warrant. In passing it may be noted that the first Summary of Results given in the Appendix does not show any crying need for superheating nor is the superiority of superheater engines shown to be very marked at any point in the Paper. The figures referred to are— Mr. Mills.

Saturated engine 7·3 and 5·28 per cent.

Superheated engine 8·0 and 7·32 per cent.

from which it appears that the best saturated results come too near the superheater results to justify the extra cost of superheating.

With the Author's remarks on compounding one must express general agreement, though it may be noted that the superheated compound results quoted seem to offer possibilities for heavy goods work.

To the writer Mr. Grime appears unduly complacent regarding the turbine locomotive. The geared-turbine locomotive built by Messrs. Ljüngström is the most efficient railway machine yet put into operation. This engine weighs 126 tons in working order and is fitted with an impulse turbine developing 1,800 horse-power. From trials conducted by officials of the Swedish State Railways and published in *Engineering* of August, 1922, we have—

Overall thermal efficiency of locomotive, 14·7 per cent.

Actual draw-bar efficiency at 80 km. per hour, 76 per cent.

Thermal efficiency at draw-bar, 11·17 per cent.

Agreeing for the moment with the Author's assumption of 12 per cent. thermal efficiency *at the turbine* in average running we have a draw-bar thermal efficiency of 7·75 per cent. assuming 85 per cent. mechanical efficiency, *i.e.*, we are presented with the prospect of a considerable improvement on the best superheated compound results even in ordinary running. It should also be borne in mind that the figures quoted were obtained with the first engine built. Surely it is reasonable to expect further development and improvement. The starting torque exerted is greater than that given by a reciprocating engine of equal power because the impulse turbine works as a pressure engine at very low speeds. Although the thermal efficiency of the turbine at starting is practically nil this stage is very quickly passed. It appears that the two types of

Mr. Mills

engine are equally efficient at 7 km. per hour, the superiority of the turbine engine becoming increasingly marked beyond that speed. Obviously, a turbo-electric locomotive cannot be quite so efficient as the geared turbine, though, as the Author has indicated, the figures given in the Paper ought to be surpassed.

The writer feels compelled to query the Author's statement that the L. & N.E.R. "Pacific" engine gives a thermal efficiency equal to that credited to the electric locomotive, *i.e.*, 4·4 per cent. The engine under consideration is rated at 1,843 boiler horse-power, and at 65 m.p.h. has a draw-bar efficiency of 49·1 per cent. Assuming the thermal efficiency on i.h.p. to be 8 per cent. (the Author's best figure) the thermal efficiency at the draw-bar is 3·93 per cent. Taking the Author's figure of 1,675 boiler horse-power, the thermal efficiency at the draw-bar is 3·91 per cent. The considerable difference in weight between steam and electric locomotives is worthy of some emphasis, and when taken into account, as in estimating draw-bar efficiency, would considerably modify Mr. Muhlfeld's figures which actually give thermal efficiency at the wheel.

The Author has not thought it necessary to estimate the thermal efficiency of proposed oil-engine locomotives but has shown the possible economies much more forcibly. When the internal-combustion engine is successfully adapted to the locomotive it will be superior to any other known form of railway engine in point of thermal efficiency. Successful adaptation, however, still lies in the future, while the turbine locomotive is an accomplished fact. The Ljungstrom engine appeals to the writer as the most promising railway proposal yet put into concrete form and it certainly appears to provide a sufficient saving to cover extra interest charges.

It is most unfortunate that Mr. Grime has not been able to formulate any statement of the much discussed, though very vague, capital charges involved by the alternatives to steam traction as at present generally used. The writer hopes that he will, at some future time, complete his review by discussing the more purely commercial side of the subject.

In conclusion the writer wishes to compliment Mr. Grime upon a most interesting Paper on a very absorbing subject.

AUTHOR'S REPLY.

The Author

The Author thanks all the gentlemen who have so kindly contributed to the discussion of his paper; especially Mr. Still and Mr. Kelway-Bamber who undertook the journey from London to Newcastle in order to attend the meeting. A gratifying feature also is the number of well-known members of our kindred association The Institution of Locomotive Engineers who have favoured the Author by forwarding contributions. It is pleasing to note that, speaking generally, the trend of criticism is constructive and the Author is pleased to say that he personally has

gained a considerable amount of information from perusal of the many contributions forwarded and this, after all, is the real purpose of delivering a paper of the present nature. The Author

Reply to Sir Archibald Ross.—The Author is deeply gratified by the favourable opinion expressed by our President and hopes that the suggestions put forward by Mr. Still and Mr. Kelway-Bamber will ultimately bear fruit and that locomotive engineering will continue to occupy a place, if only a minor one, in the *Transactions* of the Institution.

Reply to Mr. Still.—The Author has studied Mr. Still's contribution with great interest, and wishes to thank him for his authoritative treatise, which is in effect an independent paper on one branch of the subject. The information given is most valuable, and confirms the Author's opinion that in circumstances where the employment of a boiler is no serious drawback, the use of steam in conjunction with an internal-combustion engine offers, for the present, the best means of applying this type of prime mover to locomotive service.

Mr. Still does not appear to agree that draw-bar efficiencies will be the same for both the Still and the electrical-transmission types of internal-combustion locomotives. The Author reasoned that the boiler and water which are necessary in the Still proposal, would be at least as heavy as, and probably heavier than, the generators and control gear of the electrical-transmission type, and that the engines being of slower speed would be heavier in the Still locomotive thus compensating for the weight of the motors in the electrical-transmission type. In a conversation with Mr. Still following the reading of the paper, the Author learned that the proposed arrangement of the Still locomotive would be such that a gear ratio of two to one was effected between the engine shaft and the driving wheels, so that the Still locomotive would have a slight advantage over the electrical-transmission type in respect of weight, but not, in the Author's opinion, to anything like the extent suggested.

The type of machinery adopted for an electrical-transmission type of locomotive, would be such that the weight need not exceed that of a main-line express locomotive and tender of corresponding capacity. The control gear used, for instance, would be of much simpler construction than in the case of a purely electric locomotive due to the generator itself being under the control of the driver, this alone making some reduction of weight possible.

The Author considers that where water is plentiful and suitable for boiler use, an excellent opening is afforded for the Still locomotive, but that where, as is often the case, the boiler constitutes the greatest objection to the use of a steam locomotive, then for the present the electrical-transmission type seems to offer the greatest advantages.

Reply to Mr. Kelway Bamber.—Mr. Kelway Bamber raises an important question, namely, the more efficient utilization of the motive

The Author

power available and this largely resolves itself into a question of increasing the draw-bar efficiency. To do this in passenger services involves increased loading and reduced speeds, and the latter, at least, would not be very favourably received by the English travelling public. As regards goods traffic the operating efficiency could be considerably increased by the more extensive use of high-capacity stock and by the adoption of heavier draw gear and continuous braking. Unfortunately under existing British conditions of dispatch there would be considerable difficulty in providing a paying load for high-capacity stock, and further the complete conversion of our four-wheeled loose-coupled stock to a type suitable for continuous braking, involves such a high capital expenditure that it becomes a debatable question whether the operating economies that might reasonably be expected, would suffice to give sufficient return for the capital outlay involved.

There is however no question that sooner or later the present type of loose-coupled freight train will have to be superseded and in the Author's opinion steps should be taken to bring about this change by insisting that all new stock built should be provided with heavier draw gear and continuous brakes. It will then only be a matter of time before a sufficient quantity of this class of stock is available to enable heavier loads with higher average speeds, due to safer control, to be a standard feature of main-line freight traffic with consequent increased efficiency.

Reply to Col. Kitson Clark.—The suggestion of Col. Kitson Clark is so full of possibilities that to deal with the matter fully would involve the writing of practically another paper. The Author assumes that the remarks put forward apply primarily to the internal-combustion locomotive and if so the reason for adopting high speeds lies in the much greater weight of the slow-speed internal-combustion engine as compared with steam engines. A directly acting, internal-combustion goods locomotive, having wheels say 4 feet 6 inches in diameter, would do most of its full-power work at a speed of roughly 15-20 miles per hour, corresponding to 93 to 124 revolutions per minute of the driving wheels. Internal-combustion marine engines at corresponding speeds run to well over 200 lb. per b.h.p. for engine alone as compared with roughly the same figure for the steam-locomotive-engine boiler and tender combined. The necessity for increasing the speed of the prime mover with the object of reducing its weight is therefore apparent. It is interesting to note that even in Mr. Still's proposal which utilizes steam for starting, the engine cylinders are geared up in the ratio of two to one in order to overcome the weight difficulty to some extent. The matter of weight is almost as great a difficulty in the application of the internal-combustion engine to locomotive purposes as is that of flexibility, and in the Author's opinion it is only by evolving a system that will give a large indicator diagram area with certainty and reliability of firing at starting and slow speeds of revolution, regardless of efficiency, that the

direct-acting internal-combustion locomotive will become a commercial possibility. In the meantime light, high-speed, internal-combustion engines in conjunction with electric or other transmission, or alternatively steam operation at slow speeds as in the Still engine, appear to be the only possible solution of the question. The Author

Reply to Mr. S. J. Lucas.—It gave the Author great pleasure to peruse Mr. Lucas's excellent contribution and it is hardly necessary to state that with the general remarks put forward the Author is in entire agreement. In the matter of sound and smell there is no doubt that the oil-engined locomotive is at a serious disadvantage as compared with the steam or electric locomotive while in the matter of appearance it is, the Author considers, generally admitted that a well proportioned steam locomotive is an imposing and pleasing spectacle. As regards the travelling public there is little to chose between the three systems in the way of comfort efficiency except that the internal-combustion locomotive at present is decidedly noisy; but this can be overcome to a considerable extent by improved methods of combustion and silencing. The matter of smell would not appear to be a serious disadvantage in view of the vile odours tolerated from some of the internal-combustion vehicles using our public roads. From the point of view of the men who have to operate them the cramped spaces and the mechanical noises existing in the cab of an indirectly driven internal-combustion locomotive are a decided drawback and an improvement will have to be effected in comfort efficiency before the internal-combustion locomotive can compare with its steam and electric contemporaries from the point of view of the operator.

In Mr. Lucas's comparisons between electric and compound locomotives the Author entirely concurs. He regrets that he has not yet received a copy of the discussion on Mr. Julian Tritton's paper on "The Internal-Combustion Locomotive for Railway Work." The Author also contributed some remarks to this discussion and he looks forward to a comparison of Mr. Lucas's remarks with his own in this connexion.

Reply to Mr. J. H. Smeddle.—Mr. Smeddle's contribution is of great value and has enabled the Author to verify some of the estimated figures given in his paper. That under conditions not infrequently met with, electrification is decidedly advantageous, the Author freely admits; but he certainly considers that any general change to electrification will leave us decidedly worse off financially than before. Mr. Smeddle appears to assume that the steam-locomotive figures quoted by the Author are test plant results. So far as the figures relating to British railways are concerned this is not the case. The results are average results obtained on the line, and stand-by losses and coal consumed in lighting up, etc., are included except in the Lancashire and Yorkshire results where lighting-up coal is specified as being excluded. It is the case, though not generally realized, that stand-by losses in power stations

The Author

form an appreciable proportion of the total coal consumed. Mr. Shaw (Paper No. 278, Institution of Electrical Engineers, 1919), gives the figure of 22,000 lb. of steam per hour as the no-load consumption for a 20,000 kw. set. The running consumption is 9·54 lb. per kw. hour and the total hourly consumption inclusive of the no-load steam is 212,800 lb. the no-load consumption or stand-by loss amounting to 10·32 per cent. of the total. In the case of a railway electrification scheme the load factor is usually poor and it is probable that, on the average, the no-load consumption would be decidedly increased. The particulars given of the North Eastern Railway electric locomotive are of special value. The difference between the electrical and drawbar horsepowers amounts to 430 h.p. which represents the power required to propel the locomotive. The definition of speeds from 40 to 60 miles per hour is vague, but the Author presumes that 430 h.p. is the average power required to propel the locomotive between these speed limits. The Author considers that it will be reasonable to take the mean speed as that of the average for fast express trains, or say 54 miles per hour. The tractive effort corresponding to 430 h.p. at this speed amounts to $\frac{375 \times \text{horse-power}}{\text{speed}} = \frac{375 \times 430}{54} = 2{,}985$ lb. corresponding to a resistance of $\frac{2{,}985}{102} = 29{\cdot}3$ lb. per ton weight of engine at 54 miles per hour.

On referring to the curves of train resistance given it will be seen that the resistance of a six-wheel coupled steam locomotive and tender is given as 28·75 lb./ton at 54 miles per hour which abundantly justifies the Author's assumption that there is no great difference in mechanical efficiency between steam and electric locomotives. It is reasonable to assume that this agreement continues throughout the speed range so that the Author's figure of 57·3 per cent. as the draw-bar efficiency at 65 miles per hour based on Sir Vincent Raven's figure of 8,400 lb. maintained tractive force (which falls between the continuous and hourly rating) is substantially correct. Mr. Smeddle's figure of 80 per cent. draw-bar efficiency has evidently been obtained by heavily overloading the engine, the figure of 2,130 given as the electrical horse-power being 48·3 per cent. in excess of the continuous and 18·35 per cent. in excess of the hourly rating—a state of affairs which could not continue long without fall of efficiency and risk of break-down due to overheating of the motors. At the same time the Author freely admits that the possibility of these temporary heavy overloads constitutes a decided advantage, and it is where these are of frequent occurrence, as with numerous heavy grades or very frequent stops with a mile or less between, that electrification can and often does show a decided advantage as regards operating efficiency.

Reply to Mr. W. Rowland.—The Author welcomes a contribution from such a recognized authority on locomotive boiler design as Mr. Rowland, a large proportion of his own knowledge of the theoretical considerations governing boiler design being attributable to a perusal

of Mr. Rowland's contributions to the *Transactions* of The Institution of Locomotive Engineers. Of the theoretical correctness of Mr. Rowland's statements as applied to boilers having fire-boxes of various types, there can be little dispute, but in the case of the locomotives taken for efficiency calculations in the Paper, the proportions of boilers and fire-boxes are such that no appreciable error will result from the acceptance of an average evaporation per square foot of heating surface. The ratios of heating surface to grate area do not vary greatly and the ratio of tube length to internal diameter in the case of all the locomotives whose efficiency is considered, falls within the usually accepted maximum of 100-1. The fire-boxes, too, are all of the British between-frames type, so that for practical purposes it is sufficiently accurate to accept the curves of boiler performance given in the Paper for British practice as correct. At the same time when we arrive at the necessity for obtaining much higher power within English load-gauge and axle-weight restrictions, the question of special fire-box design will undoubtedly have to be considered on the lines suggested by Mr. Rowland. The Author

Reply to Mr. J. W. Hobson.—Mr. Hobson's valuable contribution constitutes a fairly accurate general survey of the position, but there are one or two points which he raises upon which the Author wishes to comment.

As regards Mr. Whitelegg's statement quoted in the contribution, the Author has no hesitation in claiming that a considerably greater saving in fuel than the 1 lb. per mile mentioned would be effected if all locomotive drivers and firemen handled their charges in the most efficient possible manner without any change in the locomotive at all. The disparity between average results and the best results obtained under the direct supervision of responsible officials is conclusive proof of this.

With regard to electric power station and transmission efficiencies Mr. Hobson is referred to the reply to Mr. Jessel Rosen.

In the Author's opinion the large reserves held by railway companies should rather be devoted to the reduction of fares and rates than to the furtherance of electrification schemes of doubtful ultimate value.

As is suggested the question of maintenance is of great importance. The electric locomotive *per se* is unquestionably cheaper than the steam in this connexion, but to obtain a fair comparison maintenance of generating and sub-stations transmission lines and third rails or overhead lines must also be included. From such proposals for internal combustion locomotives as have up to the present been put forward the Author considers that maintenance and operating expenses apart from fuel are far more likely to be increased than reduced as compared with the corresponding steam locomotives except, of course, where the exceedingly bad conditions mentioned by Mr. Hobson are prevalent.

As Mr. Hobson suggests the Author does not consider that any decided increase in the thermal efficiency of the steam locomotive is possible except by a prohibitive increase both in first cost and compli-

The Author

cation. It must be clearly understood that this remark refers to efficiency only; as regards power enormous developments are possible without exceeding the British loading gauge and axle restrictions the only thing involved being a departure from the stereotyped wheel arrangements to which we have become accustomed.

The author thanks Mr. Hobson for his favourable concluding remarks.

Reply to Mr. G. A. Musgrave.—Mr. Musgrave's general opinion appears to be practically identical with that of the Author but one or two points which he mentions call for some comment. The Author's statement that the economy from superheating falls as low as 3 per cent. is apparently disputed. Presumably Mr. Musgrave bases his remark on Mr. Hookham's paper on "Superheated and Non-superheated Tank Engines" read before the Institution of Locomotive Engineers in 1922. Mr. Hookham obtained savings in fuel consumption as high as 20 per cent. from small 0-6-2 tank locomotives engaged on local service and in the discussion in London Mr. H. N. Gresley, Chief Mechanical Engineer of the Great Northern Railway mentioned that in his own tests economies in fuel consumption ranging from 15·8 to 18·7 per cent. were obtained. On the other hand Mr. A. J. Hill of the Great Eastern Railway mentioned that he had experimented with superheating on the same lines in 1915 and 1922 and had only obtained a fuel economy of 3 per cent. In Mr. Hill's experiments the locomotives had to take trains over a distance of 16½ miles in 55 minutes with thirteen intermediate stops and as this is the kind of service in which electrification offers a serious challenge to the steam locomotive, the Author chose Mr. Hill's figure of 3 per cent. as the minimum for suburban service in order that he could not be accused of unduly favouring the steam locomotive.

Mr. Musgrave rightly mentions the personal element and the Author as mentioned in his reply to Mr. Hobson, certainly considers that there are great possibilities in improvement of locomotive efficiency by a more scientific training of the men responsible for handling the product of the locomotive designer. With the remainder of Mr. Musgrave's remarks the Author entirely agrees and must congratulate him on his concluding sentence which is so apt as to be almost a classic.

Reply to Mr. H. Holcroft.—The Author must thank Mr. Holcroft for his valuable contribution and particularly for his explanation of the fall in temperature of superheat after a stop which is quite the best and most rational that the Author has yet seen. The Author was not quite correct in mentioning the fall in temperature of header and elements: the header only should have been mentioned. The fall of temperature of the header was referred to by Mr. Rowland in his contribution to the discussion of Mr. Hookham's paper on superheated tank engines previously mentioned. Mr. Rowland then expressed the opinion that the pyrometer readings really showed the fall of the header temperature and that the variation of steam temperature was not so great. The question is one upon which there has always been great diversity of opinion, but,

as first stated, the Author prefers Mr. Holcroft's present contention to any other he has seen. The Author

As regards the steam consumption per i.h.p. hour given for locomotives, this is the actual water consumption and therefore includes all supplies to auxiliaries, etc. The actual steam passing through the cylinders would be somewhat less.

Mr. Holcroft's statement that high indicated efficiency does not necessarily involve high draw-bar or actual efficiency is quite correct. The Author has records of results of British locomotive performances where the actual indicated thermal efficiency of a two-cylinder, simple, superheated locomotive works out at well over 10 per cent., but on referring this to the draw-bar the overall efficiency was substantially below the average superheater results given in the Paper. This is not an unusual occurrence when a heavy modern express locomotive is hauling a light train well below its normal capacity. Passing reference is made to this matter in the Paper (see remarks on suburban electrification). The notes as to position of regulator and reversing gear are of interest and the Author would like to suggest that the running characteristics of the engine have a great deal to do with this. Many locomotive valve gears do not appear to work as sweetly as could be desired with very early cut-offs, and drivers often prefer to advance the cut-off and reduce the regulator opening for their own personal comfort even though theoretically reduced efficiency is the result.

Mr. Holcroft does a service in raising the question of high-pressure surface generators and calls attention to great possibilities in the direction of increased power; there can be no doubt that given a practical and reliable steam generator on these lines the steam locomotive would be placed in an absolutely unassailable position. The Author looks forward to seeing a steam generator on this system take practical shape and considers there are great possibilities in the application of a gas producer in conjunction with a generator of the type to locomotive service.

Reply to Mr. Jessel Rosen.—It was hardly to be expected that the figures for electric-power-station efficiency would escape the notice of our electrification friends and the Author is pleased that Mr. Rosen has raised this point. The Author is well aware of the figures embodied in the Electricity Commissioners' reports both for 1921 and 1922. The average coal consumption of all stations in 1921 was 3·33 lb. per kw. hour or 2·48 lb. per h.p. hour delivered, the average thermal efficiency being 9·78 per cent. Neglecting all stations with an output of under 3,000,000 kw. hour per annum the consumption was 3·17 lb. per kw. hour giving an average thermal efficiency of 10·25 per cent.

In 1923 the consumption of the two largest stations (459,153,708 kw. hour combined output) gave 2·1 lb. per kw. hour corresponding to 15·5 per cent. thermal efficiency, the average for the year being 3·11 lb./kw. hour or 10·4 per cent. In these calculations the calorific value of the fuel has been taken at 10,500 B.Th.U.'s per lb. which the Author believes to be well on the low side.

The Author

It was interesting to note that according to these returns internal-combustion plants do not show so high a thermal efficiency as steam plants. Mr. Rosen does not appear to have taken into consideration the poor load factor obtaining in electrification schemes for main-line work which should be done before quoting any definite thermal efficiency. The figure of 17 per cent. quoted is in the Author's opinion much in excess of the average that could reasonably be expected; but even if attainable it is very doubtful whether the 2·4 per cent. increase in thermal efficiency mentioned would go very far towards meeting the capital charges involved unless, as in suburban service, operating conditions were exceptionally favourable to electrification. As to regeneration, this is of some advantage where the contour of the route is formidable, but there are few examples in this country where grades are sufficiently severe for any great benefit to accrue from its adoption.

Reply to Mr. W. S. Burn.—The Author anticipated receiving a valuable contribution from Mr. Burn and is pleased to find that he has not been disappointed. He does not, however, agree that in his consideration of Mr. Burn's proposal he has chased the shadow and missed the substance and contends that as far as the claims put forward by Mr. Burn in his paper are concerned his criticism contained in the discussion was quite correct.

Mr. Burn himself is evidently well aware that his scheme as at that time proposed possessed many shortcomings as he now suggests an increase of cylinder diameter to 12 inches and also makes the claim that double the mean effective pressure can be obtained from the Diesel engine cylinders up to 10 miles per hour. This, to the Author, seems a high claim to make; but, for the purpose of this discussion he proposes to accept Mr. Burn's suggestion as correct, in view of the many details provided to obtain flexibility, and to compare the performance of this engine revised as suggested, with a standard steam locomotive with which the Author is thoroughly acquainted and which, though of much smaller nominal power, is in his opinion a more practical operating unit.

The assumptions made as regards Mr. Burn's proposed locomotive are as follows:—

(1) That the Diesel cylinders are increased to 12 inches diameter.

(2) That double the normal mean effective pressure of 80 lb. per square inch available at rail, *e.g.*, 160 lb. per square inch, can be obtained at the rail at all speeds from the lowest firing speed up to 10 miles per hour.

(3) That the fall of mean effective pressure can be made to follow a straight-line law until the speed reaches 40 miles per hour when the mean effective pressure at rail is 80 lb. per square inch.

The main proportions of Mr. Burn's engine as modified will be as follows:—

Cylinders: four in number, 12 inches diameter by 16 inches stroke by 2 (opposed-piston type).

The Author

Coupled wheels, diameter 3 feet 9 inches.

Maximum tractive effort with oil engine working and a mean effective pressure equivalent to 160 lb./per square inch at rail, 16,400 lb.

Rated tractive effort corresponding to 80 lb. mean effective pressure at rail at 40 miles per hour, 8,200 lb.

Rated h.p. at full speed, 874.

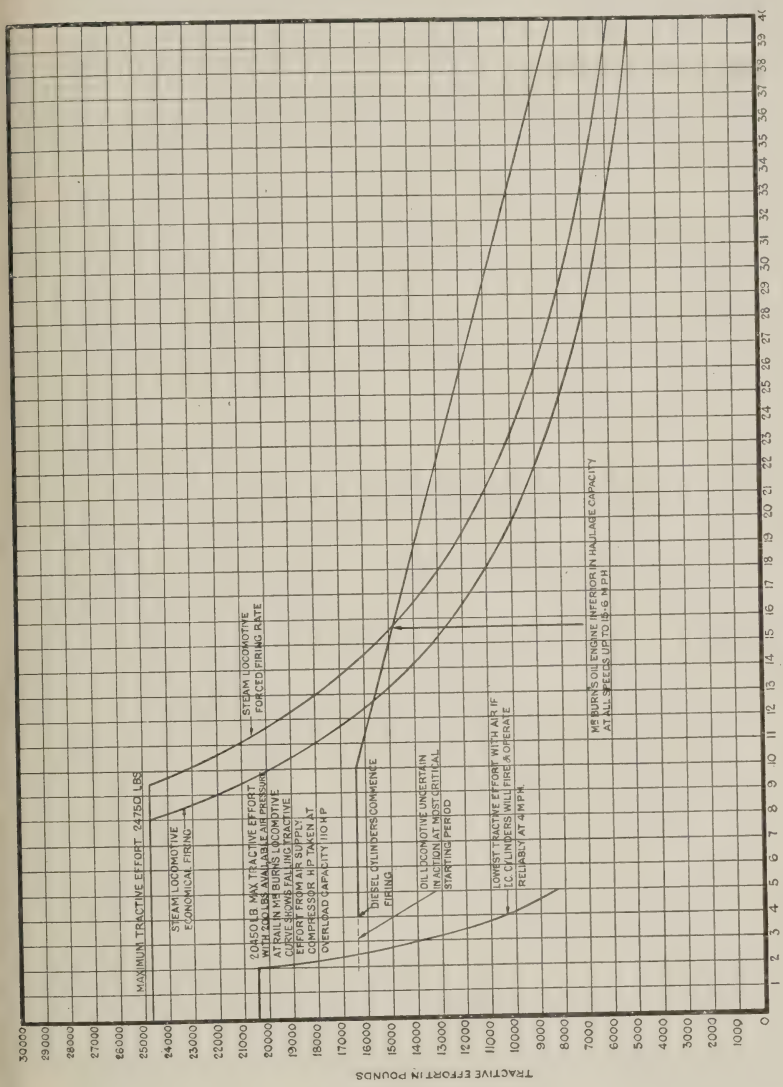


Fig. 12.—Curve Showing Calculated Performance of Mr. Burn's Oil Engined Locomotive as Modified Compared with that of Steam Locomotive.

The chart given Fig. 12, shows the tractive effort exerted at all speeds up to the maximum of 40 miles per hour and also that due to the auxiliary engines and air compressors at starting. On this chart are also plotted the maximum and economical tractive efforts of one of Messrs R. & W. Hawthorn, Leslie & Co.'s standard type of tank locomotives at corresponding speeds.

The Author

Particulars of this engine are as follows:—

Type	0-6-0		
Cylinders... ...	diameter 18 in. stroke 24 ,,	Tube heating surface	1,002 sq. ft.
		Firebox	90 ,, ,,
Coupled wheels, diameter	4 ft. 0 ,,		——
Tank capacity	1,200 galls.	Total	1,092 ,, ,,
Fuel ,, ...	2 tons.	Grate area	18 ,, ,,
Weight in working order	52·6 ,,	Working pressure ...	180 lb. sq. in.

Maximum tractive effort taking 85 per cent. working pressure 24,750 lb.

Boiler horse-power at an economical rate of firing of 120 lb. per square foot of grate per hour with an evaporation of 8·25 lb. of water per lb. of coal from and at 212° Fahr. with an evaporation factor of 1·21 and taking 28 lb. steam per i.h.p. hour = 528 i.h.p.

Maximum boiler horse-power with a rate of firing of 180 lb. per square foot of grate per hour with evaporation of 6·5 lb. water per hour from and at 212° Fahr. with evaporation factor and steam consumption as above = 622 i.h.p.

Speed up to which boiler will maintain full tractive effort at economical firing rate = 8 miles per hour.

Speed up to which boiler will maintain full tractive effort at forced firing rate = 9·45 miles per hour.

A glance at the chart will show the marked advantage possessed by the steam locomotive at the ordinary speeds expected from engines of the type as regards tractive effort and corresponding haulage capacity.

An estimate of the actual haulage capacity cannot be made without knowing the weight of Mr. Burn's locomotive. This is difficult to estimate as the engines and general arrangement are of a type differing radically from the proposals usually met with. From a comparison with stationary engines of similar power and also submarine and other internal-combustion engines running at corresponding speeds and from personal knowledge of weights of locomotive mechanical and constructional parts, the Author would estimate the weight to be roughly, 68 tons or about 15½ tons more than the steam locomotive selected for comparison.

It will thus be seen that even if the claims now made can be substituted in practice, the performance of an engine of this type still leaves much to be desired from an operating standpoint. It must also be pointed out that there is likely to be considerable difficulty in arranging the internal-combustion engines to fire with absolute certainty at the low speed of 2½ miles per hour when the tractive effort that can be exerted by the air engines provided alone falls below the 16,000 lb. figure and there is at this period a danger point in the tractive force line (see Fig. 12).

Reply to Mr. C. S. Darling.—With Mr. Darling's opening remarks on the question of electrification the Author quite agrees. Mr. Darling appears rather to exaggerate the limitations of the steam locomotive boiler. This is usually designed to provide sufficient steam for the work the engine is designed to do and it is only when the engine is required

to perform more onerous duties than those for which it was intended that boiler limitation is felt. In any case a locomotive boiler possesses a greater capacity for overload than any internal-combustion engine and the steam locomotive as a whole is more capable of continuous overload than any other machine, although the electric traction motor will take a much greater overload for short periods. The Author

Mr. Darling's figures as to weights of internal-combustion engines and electrical equipment are very valuable but it must be understood that they are minimum values. The Author has figures which show that the weight of electrical equipment for electric locomotives reaches as much as 100 lb. per h.p. on the hourly rating in some cases. The average figure seems to be from 55-60 lb. If Mr. Darling can as he seems to suggest obtain a diagram corresponding closely to that of the steam engine from the internal-combustion cylinders direct we should soon be well on the way towards the production of a successful direct-acting internal-combustion locomotive and the Author hopes he will follow the example of Mr. Burn and favour us with some actual particulars in the near future. The Author does not entirely agree with Mr. Darling's remarks concerning the Still engine. Certainly under unfavourable natural circumstances, such as bad water, etc., the necessity for a boiler imposes a severe handicap and electrical or other transmission is more suitable. Where water is good and plentiful, however, the boiler is cheaper, simpler and lighter than electrical or any other indirect transmission the total weight in full working order not exceeding 60 lb. per horse-power on easy steaming which is lighter than dynamos and motors can be at any normal engine speed.

There is undoubtedly a good field for the electric battery locomotive under intermittent service conditions but for continuous work they are a rather inefficient method of utilizing electrical power.

Reply to Mr. F. Mills.—The Author is in perfect accord with Mr. Mills that far more powerful locomotives are possible within the limitations of load-gauge and axle-weight restrictions obtaining in this country (see reply to Mr. J. W. Hobson). At the same time increased efficiency, as distinct from power, cannot be obtained without the introduction of considerably more complication, and it must be remembered that its relative simplicity and reliability are two of the greatest advantages possessed by the steam locomotive.

The Author does not consider the use of powdered fuel to be a great advantage. It enables a lower grade of fuel to be utilized in an existing fire-box designed for high-grade fuel, but in the case of a new design the Author would prefer to design a fire-box expressly for inferior fuel rather than resort to pulverizing.

The Author regrets to find that Mr. Mills attaches any importance to the slogan of " destroying oil under boilers " which is after all merely cheap internal-combustion propaganda. If oil-fuel cost the same as coal in relation to its calorific value it would be an advantage to have all

The Author

steam locomotives oil fired inasmuch as oil fuel provides the cheapest and most reliable form of mechanical stoker and permits of a greater amount of forcing in an emergency. The relative commercial efficiency of the two systems would be unaltered and the steam locomotive would continue to hold the field until the internal-combustion-engined locomotive reached a much higher stage of development than at present. In fact as Mr. Mills is well aware, in countries where oil fuel is a native product it is actually used for this purpose and rightly so as long as the steam locomotive remains the most suitable and reliable transportation agent.

The Author is well aware that it is not theoretically correct to base evaporative capacity on heating surface alone, but as regards the general design of boilers in British practice no serious error results (see reply to Mr. Rowland). The Author does not consider the boiler efficiencies given to be remarkable. It is generally recognized that for firing rates of 30-80 lb. of fuel per square foot of grate per hour the steam locomotive boiler, when actually used under locomotive conditions, is the most efficient of all steam generators. It is well known that if a saturated engine is carefully handled with an economical firing rate, a low water level, and a thin even fire, the efficiency will closely approach that of a superheated locomotive. Under average modern conditions with overloaded engines and firemen whose main interest often lies in other directions than the study of fuel economy, the value of superheating is most marked and indisputable as the comparative figure for superheated and saturated engines on the old London and North Western Railway clearly demonstrate.

The turbine locomotive built by Messrs. Ljüngström has unquestionably given some exceptionally good test results, but they are hardly likely to be maintained in average service. The particular engine referred to also embodies other interesting features such as the heating of the combustion air on its way to the ashpan, etc., all of which tend to increased efficiency at the sacrifice of simplicity and involve increased first cost and it is probable that an even more efficient internal combustion locomotive could be built for the same initial outlay. It must be remembered that cost and complication are the chief obstacles to the latter.

The Author did not draw any comparisons between the Great Northern Railway Pacific locomotive and the North Eastern 4-6-4 electric locomotive at 65 miles per hour but in view of Mr. Mills's comments it will be desirable to do so. The figure of 1,675 boiler horse-power given by the Author for the G.N.R. engine is the power that can be maintained under efficient firing conditions although he is well aware of the rated figure of 1,843 h.p. which requires a combustion rate somewhat greater than that taken by the Author and may be compared to the hourly rating of the electric motor. The exertion of 1,843 h.p. at 65 miles per hour involves a continuous tractive effort of 10,650 lb. The resistance of the engine is $148{\cdot}57 \times 35{\cdot}18 = 5{,}225$ lb. leaving 5,425 lb. available for

hauling the train, the corresponding draw-bar efficiency being 51 per cent. The Author

The Author is aware that Mr. Muhlfeld's figures do not compare very well with English results and the reason is to be found in the smaller boiler efficiency largely due, doubtless, to the steel fire-boxes, with smaller volume/grate-area ratios, longer tubes, and less efficient workmanship obtaining in American practice. It will be seen from Fig. 1 that the best boiler efficiency given for British practice is considerably in excess of the American results plotted, though on reference to Fig. 2, it will be seen that owing to the lower ratio of heating surface to grate area in the American example, the evaporation per square foot of heating surface for a given rate of firing is greater.

Taking a calorific value of 18,000 B.Th.U.'s per lb. of fuel the overall thermal efficiencies for directly and indirectly driven internal-combustion locomotives is as follows :—

	Per cent.		Per cent.
Directly driven locomotive at rail ...	32·4	At draw-bar with 65 per cent. draw-bar efficiency ...	21
Indirectly driven locomotive at rail	24·25	At draw-bar with 65 per cent. draw-bar efficiency ...	15·8

It is difficult to obtain reliable figures for electrification costs. Figures which have been given in various papers before American technical institutions have been subjected to strong criticism on the grounds usually that the equipment covered for in the estimates was not of sufficiently reliable and robust construction the aim of the Authors having generally been to make out a good case for electrification. It is significant that few electrified lines are paying much in the way of dividends. Possibly when prices of material become more stable it will be possible to obtain actual figures based on the high standard of workmanship and construction obtaining in British practice.

Reply to Mr. J. Calderwood.—It is regrettable that Mr. Calderwood has challenged the London & South Western trial results given. Quite apart from any opinions that may be held to the contrary the fact remains that these results were actually obtained. They were vouched for by the Chief Mechanical Engineer of the railway company and are to be found in full detail in the Proceedings of the Institution of Civil Engineers, 1895-6, Vol. CXXV. The Author is acquainted personally with one of the gentlemen who was responsible for the conduct of these tests and who has kindly forwarded a small contribution in regard to these tests. The Author considers that as good results can be obtained from almost any well designed saturated locomotive, given the same ratio of firing, a fireman not afraid to work with fairly low water, and a load and speed bearing the same relation to the capacity of the engine.

With regard to boiler and power station efficiencies see replies to Mr. Mills and Mr. Rosen.

Electrical transmission efficiencies are variables. Mr. Calderwood's values may be in order under exceptionally favourable circumstances but

The Author

the Author prefers those of Mr. R. T. Smith as more likely to represent average values. Mr. Calderwood's figure for high voltage transmission efficiency appears to be on the high side and it is interesting to note that he has assumed a.c. locomotives to avoid having to include conversion losses, which is hardly admissible in view of the fact that d.c. has been recommended for adoption in British electrification work. As regards the fuel question thermal efficiency has been taken as a basis. It must not be forgotten that a greater amount of coal has to be burned when the calorific value is less but the thermal efficiency remains unchanged. Allowance has been made for the inferior value of power-house coal in the consideration of power-house efficiencies put forward in the reply to Mr. Jessel Rosen.

In estimation of the indicated horse-power of steam locomotives this has been taken as $\frac{\text{Tractive effort} \times \text{speed}}{375}$ and is therefore really tractive effort horse-power, and the mechanical resistance of the engine is included in the total engine resistance which, in conjunction with air head resistance, accounts for the large engine resistance as compared with that of the train. The same procedure is followed in the case of the electric locomotive the brake horse-power of the motor being taken as equivalent to the indicated horse-power of the steam locomotive and the accuracy of this is borne out by the analysis of the results mentioned by Mr. Smeddle in connexion with the North Eastern locomotive.

The Author has not made it quite clear what was meant in referring to the internal-combustion engine as "largely a constant-torque proposition." He is aware that within a narrow speed range considerable torque variation is possible, but that is a very different matter to the steam engine where almost any mean effective pressure from zero to 85 per cent. of the boiler pressure can be obtained at any speed within the limits of the boiler evaporative capacity.

The hydraulic-clutch proposal is by no means new. Messrs. Beardmore have in fact prepared designs for engines embodying this feature for horse-powers up to 600. The difficulty is to obtain a sufficient speed range in the internal-combustion engine. If an oil engine can be produced capable of giving with absolute certainty a mean effective pressure of 160 lb. at 40 r.p.m., and of operating equally well at a speed of 400 r.p.m. and a m.e.p. of 30 lb., we could use a hydraulic clutch at the starting speeds, and obtain an engine of workable characteristics though still not possessing the speed range of the modern express passenger locomotive.

For engines operating at slow speeds the hydraulic clutch is a workable proposition and has in fact been employed. An experimental engine of this type was built by Vickers Ltd., and functioned satisfactorily, but this was of 75 h.p. only and had a maximum speed of 15 m.p.h. For the high powers required in main-line work considerable difficulties arise especially in the matter of dissipating the heat generated. Some con-

siderations of this subject will be found in Mr. J. S. Tritton's paper read in the current session before the Institution of Locomotive Engineers. The Author

The matter of internal-combustion design is being well investigated by Messrs. R. & W. Hawthorn, Leslie and Co., and an experienced staff of steam-locomotive oil-engine and electrical designers are engaged on the matter. The results obtained up to the present are encouraging though finality has not yet been reached. The whole matter is bound up with considerations of diverse operating conditions which, if known to Mr. Calderwood, would doubtless cause him to have a greater appreciation of the difficulties involved.

Reply to Mr. J. C. Arthur.—Mr. Arthur in his remarks appears to have overlooked some of the essential principles of locomotive design. The wind resistance alone at a speed of 65 miles per hour would, under most conditions, be in excess of the total figure put forward by Mr. Arthur whom the Author would refer to his reply to Mr. Smeddle. In connexion with hydro-electrification schemes, the high initial capital cost has to be taken into consideration and the Author would refer the contributor to the remarks of Sir Charles Parsons in the discussion of Sir Vincent Raven's paper. Steam-turbine electric-power stations show, according to the Electricity Commissioners' report for 1922, a higher overall thermal efficiency than the internal-combustion and gas-producer plants at present in operation. Mr. Arthur appears to suggest that the tractive effort should follow the same curve as train resistance. Surely he must know that if such were the case there would be no power available for acceleration or hill climbing. The matter referred to in connexion with the Manchester-Blackpool trials is interesting. The official report of these trials mention the fact as follows, " These figures represent very fairly the usual consumptions for these runs the excess consumption on the Manchester-Blackpool journey over the return trip being normal and to be explained by peculiarities of the road, particularly the difficult start from Manchester and the severe ascents to Pendlebury and Diccon-son Lane. Mr. Arthur will readily perceive from this that, unless the results were averaged, there would be a decided inaccuracy as an engine that departs in one direction must return in the other and the mean results is obviously the correct one.

MINUTES OF PROCEEDINGS.*

27th October, 1922.

Special General Meeting.

Mr. HAROLD THOMSON, Vice-President, in the Chair.

The Secretary read the notice convening the meeting.

The Chairman dealt briefly with the reasons for the proposed change in the Articles of Association.

On the motion of Mr. Leslie Skinner (Vice-President), seconded by Mr. R. H. Winstanley (Honorary Treasurer), the following resolution which was contained in the notice convening the Meeting, was carried unanimously :—

That the necessary consent of the Board of Trade having been obtained, the Articles of Association of the Institution shall be altered in manner following, and that the alterations to Articles 15, 27 and 29 be regarded as taking effect as from the 1st day of August, 1922 :—

The following Article to be inserted :—

Every member of the Institution shall, by virtue of his membership, be also a member of the Cleveland Scientific and Technical Institution, and every member undertakes to contribute to the assets of the Cleveland Scientific and Technical Institution in the event of the same being wound up during the time that he is a member, or within one year afterwards, for the payment of the debts and liabilities of the same contracted before the time at which he ceases to be a member of the said Institution, and of the costs, charges and expenses of winding up the same, and for the adjustment of the rights of the contributories amongst themselves, such amount as may be required not exceeding 10s.

The following Articles shall be substituted for Articles 15, 27 and 29, namely :—

15. Every Graduate under the age of 21 shall contribute annually the sum of half-a-guinea; every Graduate over the age of 21 shall contribute annually the sum of one guinea. All Graduates shall be exempt from paying any entrance fee.

27. Graduates may attend all General Meetings, they may read Papers, and take part in discussions, but shall not sign proposals for the election of new members of the Institution, nor vote on any resolution, nor be eligible for office, except in their own Section as provided in the Bye-laws hereinafter mentioned regulating the proceedings of the Graduate Section of the Institution. Graduates over 21 years of age shall also receive copies of the *Transactions* and Graduates under 21 years of age shall receive copies of the *Transactions* upon payment of an additional subscription of half-a-guinea.

29. Any member of the Institution not being a Graduate under the age of 21 years elected at any time between the Annual General Meetings shall be entitled to copies of all the *Transactions* issued during the session to which his first subscription applies.

The Chairman announced that the resolution would be submitted for confirmation as a special resolution at a further General Meeting of the Institution to be convened for the purpose, in accordance with the provisions of the Companies Acts relating to special resolutions.

The meeting then terminated. There were 80 members present.

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* For Minutes of Annual General Meeting, see *Transactions*, vol. xxxix., p. 1

27th October, 1922.

Second General Meeting.

Mr. HAROLD THOMSON, Vice-President, in the Chair.

The Minutes of the Annual General Meeting held on the 20th October, 1922, were, with the permission of the meeting, taken as read, and signed by the Chairman.

Mr. Dinwoodie read a Paper entitled "Wave Transmission of Power." This was illustrated by lantern slides.

The following gentlemen took part in the discussion :—

Mr. Harold Thomson, Chairman.
Mr. R. W. Wilson.
Mr. D. Myles.
Mr. H. G. Kimber.
Mr. J. W. Hobson.
Mr. J. C. Arthur.

The Author replied to the points raised in the discussion.

A vote of thanks, moved from the Chair, was accorded to Mr. Dinwoodie for his Paper.

The meeting terminated at 9.20 p.m. There were 97 members and visitors present.

3rd November, 1922.

Third General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

Minutes of the Special and General Meetings held on the 27th October, 1922, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The following candidates for membership were unanimously elected :—

Members :

ARCHIBALD DOUGALL BRUCE.
JOHN LEO CHALONER.
HYO HAMADA.
ERNEST EVANS OLDERSHAW.
JAMES RICHARDSON.

Associate Members :

TSOA-FUN CHEN.
WALTER ERNEST CROMPTON.
NORMAN HARDY JENNINGS
ERNEST MARKHAM.
ROY RUTHVEN RUTHVEN.

Transfers from Graduates to Associate Members :

GEORGE ATKINSON.
RICHARD ALFRED COOMBS
JOHN FERGUSON.
WALTER GEORGE GREEN.
THOMAS GRIME.
ROLAND LLEWELLYN HUNTER.
FRANK MCGUINNESS.
HENRY WILLIAM NICHOLLS.
JAMES BLACKBURN SHEARLAW.
ROBERT HENRY GEORGE SUTTON.
RICHARD BERWYN WILMSHURST.
WILLIAM JAMES WILSON.

Associates :

STEPHEN ADAM COPELAND.
CHARLES HENRY LUKE

Graduates :

HOWELL ARTHUR BANISTER.
FRANCIS GEORGE BELLERBY.
HENRY CAIRD.
JOHN CUTHBERT DIXON
FRANK HAMILTON.
ROBERT MIESEGAES.
ALEXANDER PURVES MILLER.
JOHN ERNEST NIXON.
HENRY GRAHAM PIERCY
WILLIAM EDWIN KEITH PIERCY.
GERALD SLATER RAMSDALE
RICHARD GRINSELL SHAND.

Mr. Chorlton read a Paper entitled "Working Practice in the Design of Large Double-Acting Two-Stroke Engines." This was illustrated by lantern slides.

In opening the discussion the President stated that apologies for inability to attend had been received from a number of gentlemen, including Eng. Vice-Admiral Sir George Goodwin, Eng. Vice-Admiral R. B. Dixon, Air Vice-Marshal Sir Geoffrey Salmond, and Major-General Sir Sefton Brancker.

He read a communication from Major-General Sir Sefton Brancker, Director of Civil Aviation.

The following gentlemen took part in the discussion :—

Eng.-Commander C. J. Hawkes, R.N. (ret.).
Mr. A. D. Bruce.
Mr. L. S. Swinnerton-Dyer.
Mr. Harry Hunter (of Messrs. Alfred Holt & Co.).

In view of the lateness of the hour, the President requested the other members of the audience who had come with the intention of taking part in the discussion, to send their contributions to the Secretary in writing.

Mr. Chorlton replied to some of the points mentioned in the discussion and promised to complete his reply in writing.

A vote of thanks to Mr. Chorlton was moved by Mr. C. Waldie Cairns, seconded by the President, and carried with great acclamation.

The meeting terminated at 9.40 p.m. There were 95 members and visitors present.

17th November, 1922.

Special General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Secretary read the notice convening the meeting.

On the motion of Mr. R. H. Winstanley, seconded by Mr. David Myles, it was unanimously resolved :—

That the resolution passed at the Special General Meeting held on the 27th October, 1922, and contained in the notice convening this Special General Meeting, be confirmed as a special resolution.

There were 80 members present at the meeting.

17th November, 1922.

Fourth General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the previous meeting held on the 27th October, 1922, were, with the permission of the meeting, taken as read, and signed by the Chairman.

Mr. A. T. Thorne, B.Sc. (Associate Member) read a Paper by himself and Mr. J. Calderwood, M.Sc. (Graduate), entitled "Some Notes on Torsional Oscillations with Special Reference to Marine Reduction Gearing."

The following gentlemen took part in the discussion :—

The President.
Dr. J. H. Smith (of Belfast).
Mr. S. S. Cook.
Mr. S. B. Freeman.

The Authors replied to the points raised in the discussion.

A vote of thanks, moved by Mr. A. F. Ainslie, and seconded by Mr. C. W. Reed, was accorded to Messrs. Thorne and Calderwood for their Paper.

The meeting terminated at 9.25 p.m. There were 143 members and visitors present.

1st December, 1922.

Fifth General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the Special and General Meetings held on the 17th November, 1922, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The following candidates for membership were unanimously elected :—

Members :

CLIFFORD HOWLE.
ALBERT OSWALD LEASON.
HUGH JOHN DOUGLAS THOMPSON.

Associate Members :

JOHN BATEY.
BORIS BRAILOVSKY.
JOSEPH JAMES.
RICHARD DEE PURVIS.
GEORGE ROBERT REED.

Transfers from Graduates to Associate Members :

ROBERT FINDLEY DAGLISH.
EDMUND VICTOR TELFER.

Graduates :

RALPH BEAL.
JACOB EDWARD BURNETT.
AUBREY NOEL FRANCOMBE.
GODFREY FORSTER.
WILLIAM GIBB.
WILFRED ROY HARVEY.
HAROLD HINDMARSH.
JETHRO HUNT.
KENNETH BENJAMIN JACOB.
ALFRED BLAKEY LAVERICK.
HERBERT JOHN MILLER.
VERNON DALZIEL RIDDELL PAKENHAM-GILBERT.
FREDERIC SHOTTON PARKER.
WILLIAM FRANK PIERCY.
WILLIAM SWAN ROONIE.
EDMUND PATTERSON WILSON.

Mr. Harold Moore read a Paper entitled "Some Factors affecting the Utilization of Heavy Oils in Internal-Combustion Engines."

The following gentlemen took part in the discussion :—

The President (Sir Archibald C. Ross).
Mr. J. L. Chaloner.
Mr. K. O. Keller.
Mr. Paul Belyavin.
Mr. A. D. Bruce.

The Author replied to the points raised in the discussion.

Mr. R. S. Dalgliesh (Vice-President) moved a vote of thanks, which was carried with acclamation.

The meeting terminated at 9.25 p.m. There were 98 members and visitors present.

8th December, 1922.

Sixth General Meeting.

The President (Sir Archibald C. Ross) invited Professor J. J. Welch to take the Chair and the remaining proceedings were conducted under the Chairmanship of Professor WELCH.

The Minutes of the previous meeting, held on the 1st December, 1922, were with the permission of the meeting, taken as read, and signed by the Chairman.

Mr. E. V. Telfer (Associate Member) read a Paper entitled "Note on the Presentation of Ship Model Experiment Data."

The following gentlemen took part in the discussion :—

Mr. H. G. Williams.
Mr. John Tutin.
Mr. Robert Hinchliffe.
Mr. W. R. G. Whiting.
Mr. Eric Liddell.
Mr. Alfred Fletcher.
Prof. J. J. Welch.

The Author replied to the points raised in the discussion.

On the motion of the President (Sir Archibald C. Ross) a vote of thanks was accorded to Mr. Telfer for his Paper.

The meeting terminated at 9.25 p.m. There were 53 members and visitors present.

15th December, 1922.

Seventh General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the previous meeting, held on the 8th December, 1922, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The President announced that Mr. A. P. M. Fleming, who had been announced as to deliver the lecture on "Wireless Broadcasting and its Possibilities," had been prevented from attending the meeting, but that his assistant, Captain H. G. Bell, was deputizing for him.

Captain H. G. Bell then delivered the above-mentioned lecture. The lecture was illustrated by lantern slides and practical demonstrations.

A number of questions were asked at the close of the lecture and answered by Captain Bell.

On the motion of the President a hearty vote of thanks was accorded to Captain Bell for his lecture.

The company numbered over 500 members and visitors. The meeting terminated at 9.15 p.m.

12th January, 1923.

Eighth General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the previous meeting, held on the 15th December, 1922, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The following candidates for membership were unanimously elected :—

Members :

DAVID WALKER DICKIE. | JACOB ANDREAS LAMBRECHTSEN.
ROBERT OLIVER PATTERSON.

Associate Member :

JOHN FRANCIS O'DONNELL.

Transfer from Graduate to Associate Member :

GEORGE HENRY PALLISER BOSOMWORTH.

Associate :

THOMAS EDWARD ZEALAND.

Graduates :

NORMAN COOKE. | WALTER KEMP.
EDWARD GEORGE NANCE.

Seven Papers constituting a "Review of Different Types of Marine Internal-Combustion Engines" were presented by the following companies, through the representatives indicated after the companies' names hereunder:—

Sir W. G. Armstrong, Whitworth & Co., Ltd. (Mr. T. H. Matthews).
William Doxford & Sons, Ltd. (Mr. K. O. Keller).
Palmers Shipbuilding & Iron Co., Ltd., in conjunction with Messrs. Cammell, Laird & Co., Ltd. (Mr. D. M. Shannon).
Swan, Hunter & Wigham Richardson, Ltd. (Mr. Frank Rowntree).
Scotts' Shipbuilding & Engineering Co., Ltd. (Mr. Charles R. Bruce).
Vickers, Ltd. (Mr. D. Meikle).
R. & W. Hawthorn, Leslie & Co., Ltd. (Mr. A. D. Bruce).

Prior to the reading of the Papers the President read the Specification to which contributors to the Review had been required by the Institution to conform.

Eng.-Commander C. J. Hawkes discussed the Papers and moved a vote of thanks to the Authors of the Papers, which was carried with acclamation.

The meeting terminated at 9 p.m. There were 250 members and visitors present.

26th January, 1923.

Ninth General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the previous meeting, held on the 12th January, 1923, were, with the permission of the meeting, taken as read and signed by the Chairman.

The Papers constituting the "Review of Different Types of Marine Internal-Combustion Engines" were discussed. The following gentlemen took part in the discussion:—

Mr. Archibald Rennie.
Mr. W. J. Still.
Mr. Theo Theunissen.
Mr. W. A. Souter.
Mr. A. F. Ainslie.
Mr. R. J. Eyres.
Mr. G. F. Tweedy.
Mr. James Fleming.
Mr. John Tindale.

The following Authors replied:—

Mr. D. M. Shannon, representing Cammell, Laird & Co., Ltd., and Palmers Shipbuilding & Iron Co., Ltd.
Mr. Harold Thomson, representing Sir W. G. Armstrong, Whitworth & Co., Ltd.
Mr. K. O. Keller, representing William Doxford & Sons, Ltd.
Mr. C. R. Bruce, representing Scotts' Shipbuilding & Engineering Co., Ltd.
Mr. W. F. Rabbidge, representing Vickers, Ltd.

Mr. F. Rowntree, representing Swan, Hunter & Wigham Richardson, Ltd., and Mr. A. D. Bruce, representing R. & W. Hawthorn, Leslie & Co., Ltd., agreed to communicate their replies in writing, as the evening was then far advanced.

The President thanked the Authors and Companies who had contributed Papers, on behalf of the Institution.

The meeting terminated at 9.30 p.m. There were 300 members and visitors present.

2nd February, 1923.

Tenth General Meeting.

Mr. HAROLD THOMSON, Vice-President, in the Chair.

The Minutes of the previous meeting, held on the 26th January, 1923, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The following candidates for membership were elected:—

Members:

FRANCIS WALTER BENSON. | ROBERT PRICE KERR.

Associate Members:

GEOFFREY, BARON DE BANFIELD. | JOHN ROSS HUNTER.

Graduate:

JOHN HENRY SOMERSET HOUSTON.

The Chairman announced that the following gentlemen were nominated as candidates for election to fill vacancies occurring in the offices indicated upon the conclusion of the current session:—

President:

SIR ARCHIBALD C. ROSS, K.B.E. (retiring President).

Vice-Presidents (Four vacancies).

R. S. DALGLIESH (retiring Vice-President eligible for re-election).
W. G. DOBSON (retiring Ordinary Member of Council).
GEORGE MOFFITT (former Member of Council).
R. M. SUTTON (retiring Ordinary Member of Council).
H. G. WILLIAMS (retiring Ordinary Member of Council).
R. H. WINSTANLEY, *Honorary Treasurer*.

Ordinary Members of Council (Six vacancies).

R. B. ARMSTRONG, *Member*.
A. B. BINNS, *Member*.
J. T. BATEY (retiring Vice-President).
A. Q. CARNEGIE, *Member*.
S. S. COOK, *Member*.
Principal V. A. MUNDELLA, *Associate Member*.
W. A. SOUTER, *Associate*.
W. R. G. WHITING, *Member*.

Honorary Treasurer.

R. H. WINSTANLEY (retiring Honorary Treasurer, eligible for re-election).

The Chairman invited further nominations from the meeting but none were received.

The Chairman drew the attention of the members to the notice of the Institution Dinner issued with Part 3 of the *Transactions*. He intimated that late trains to the coast and other districts would, if necessary, and if possible, be arranged for the convenience of members attending the Dance.

Mr. Paul Belyavin, *Member*, read a Paper entitled "Marine Oil Engine Installation and Auxiliaries."

In addition to the Chairman, the following took part in the discussion:—

Mr. A. D. Bruce.
Mr. J. Calderwood.
Mr. Harry Hunter.
Mr. H. G. Kimber.

The Author said that he would reply to the discussion in writing.

On the motion of the Chairman, a vote of thanks was accorded to the Author.

The meeting terminated at 9.20 p.m. There were 83 members and visitors present.

16th February, 1923.

Eleventh General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the previous meeting, held on the 2nd February, 1923, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The President reminded the members of the Annual Dinner to be held on the 23rd February.

Professor A. L. Mellanby, *Associate Member,* read a Paper entitled "A Review of Turbine Troubles and their Causes" (written by himself and Mr. William Kerr).

The following gentlemen took part in the discussion :—

Mr. R. J. Walker.
Mr. A. Q. Carnegie.
Dr. W. H. Hatfield.
Mr. H. L. Guy.
Mr. Charles Blow.

Professor Mellanby replied to certain points raised in the discussion.

A vote of thanks was accorded to Professor Mellanby and Mr. Kerr.

The meeting terminated at 9.20 p.m. There were 210 members and visitors present.

Institution Dinner.

The Institution Dinner was held on the 23rd February, in Messrs. Tilley's Assembly Rooms, Newcastle-upon-Tyne.

The party at dinner, which included both ladies and gentlemen, numbered 292.

The Dinner was followed by a Conversazione and the latter by a Dance.

2nd March, 1923.

Twelfth General Meeting.

Professor J. J. WELCH took the Chair and conveyed the apologies of Sir Archibald C. Ross (the President) for his inability to attend.

The Minutes of the previous meeting, held on the 16th February, 1923, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The following candidates for membership were declared elected :—

Member :

ARTHUR HOMANS TYACKE.

Transfer from Graduate to Associate Member :

GEORGE SWAN.

Associates :

ROSE ETHEL BOURNE.
THOMAS BILLIN CLARKE.

Graduates :

FLETCHER ALLISON.
VICTOR BULLOCK.
THOMAS BROOKE DAVISON.
JOHN DAWSON HEAVISIDE.
DAVID LOWES.
EDWARD WOOD.
DONALD ELLIS WOOLLAND.

Mr. G. S. Baker, *Member,* read a Paper entitled "Measured Mile Trials and other Ship Propulsion Data."

The following gentlemen took part in the discussion:—

The Chairman (Prof. J. J. Welch).
Mr. H. G. Williams.
Mr. F. H. Alexander.
Mr. R. Hinchliffe.
Mr. A. F. Ainslie.
Mr. E. V. Telfer.
Mr. J. Calderwood.
Mr. F. McAlister.

The Author replied briefly to certain points raised in the discussion and stated that he would complete his reply in writing.

A vote of thanks, moved from the Chair, was carried with acclamation.

The meeting terminated at 9.35 p.m. There were 97 members and visitors present.

16th March, 1923.

Thirteenth General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the previous meeting, held on the 2nd March, 1923, were, with the permission of the meeting, taken as read, and signed by the Chairman.

Mr. T. G. Potts read a Paper entitled "Notes on the Economical Performance of Turbine Installations in Merchant Ships."

The following gentlemen took part in the discussion:—

The President (Sir Archibald C. Ross).
Dr. John Morrow.
Mr. John Neill.
Mr. R. J. Walker.
Mr. A. J. Berry.

The Author replied briefly to the discussion.

A vote of thanks, moved by Mr. Harold Thomson, was carried with acclamation.

The meeting terminated at 8.35 p.m. There were 64 members and visitors present.

6th April, 1923.

Fourteenth General Meeting.

The Hon. Sir CHARLES A. PARSONS, K.C.B., F.R.S., Past-President, in the Chair.

The Minutes of the previous meeting, held on the 16th March, 1923, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The following candidates for membership were elected:—

Members:

George Frederick Carrington. | Percy Lewis Jones.
Arthur Samuel Crisp. | James Smith.

Transfer from Associate Member to Member:

Arthur Temple Thorne.

Associate Members:

Tom Beresford Best. | Norman Macleod Hunter.

Transfer from Graduate to Associate Member:

Charles William Robson.

Graduate:

Albert Edward Westlake.

Dr. J. T. Dunn, F.I.C., read a Paper entitled "Powdered Coal as Fuel."

The following gentlemen took part in the discussion:—

Prof. G. G. Stoney.
Dr. H. Peile.
Eng.-Commander C. J. Hawkes.
Mr. T. A. Crowe.
Mr. E. Angus Leybourne.

The Author replied to certain points raised in the discussion.

A vote of thanks, moved from the Chair, was carried with acclamation.

The meeting terminated at 9.20 p.m. There were 65 members and visitors present.

20th April, 1923.

Fifteenth General Meeting.

Sir ARCHIBALD C. ROSS, K.B.E., President, in the Chair.

The Minutes of the previous meeting, held on the 6th April, 1923, were, with the permission of the meeting, taken as read, and signed by the Chairman.

The President declared that the result of the Ballot for the election of President, Council and Honorary Treasurer was as follows:—

President:

SIR ARCHIBALD C. ROSS, K.B.E.

Vice-Presidents:

W. G. DOBSON.
GEORGE MOFFITT.
H. G. WILLIAMS.
R. H. WINSTANLEY.

Ordinary Members of Council:

R. B. ARMSTRONG.
J. T. BATEY.
A. B. BINNS
A. Q. CARNEGIE.
S. S. COOK.
PRIN. V. A. MUNDELLA.

Honorary Treasurer:

R. H. WINSTANLEY.

Mr. T. Grime, *Associate Member*, read a Paper entitled "Possibilities of Increased Efficiency in Railway Locomotives."

The following gentlemen took part in the discussion:—

The President (Sir Archibald C. Ross).
Mr. W. J. Still.
Mr. J. W. Hobson
Mr. H. Kelway-Bamber.
Mr. W. S. Burn.
Mr. Jessel Rosen.

The hour being late, the Author consented to communicate his reply to the discussion in writing.

A vote of thanks to the Author, moved by Mr. Harold Thomson, was carried with acclamation.

The Chairman announced the Papers already arranged for the forthcoming session.

The meeting terminated at 9.20 p.m. There were approximately, 70 members and visitors present.

OBITUARY.

FRANCIS JAMES DANIEL, Associate Member, died on the 19th January, 1923, aged 42 years.

Mr. Daniel entered Devonport Dockyard as a shipwright apprentice in 1894. He received the full educational course at the Dockyard School and upon completion of his apprenticeship served in the Drawing Office there until 1904. He was then transferred to the Admiralty, Whitehall, London, for service in the Department of the Director of Naval Construction. In 1906, having passed the necessary examination, he was appointed as a Ship Surveyor to the Marine Department of the Board of Trade and was stationed for the first two years of his surveyorship at the Board of Trade office, Mark Lane, London. Thereafter, he was transferred to the Board of Trade office, Sunderland, and held the appointment of Surveyor there up till the time of his demise. He leaves a widow and one son.

Mr. Daniel joined the Institution in 1909.

EDGAR SYDNEY GARDINER, Graduate, died on the 11th May, 1923, aged 22 years.

Mr. Gardiner received his general education at King James 1st Grammar School, Bishop Auckland, and served his apprenticeship as an engineer at the works of Messrs. Lingford, Gardiner & Co., Ltd., Bishop Auckland, of which firm his father is a principal. He was an Associate Member of the Institute of Mining and Mechanical Engineers; also, an Associate of the Institute of British Foundrymen. At the time he took ill he was studying for the examination for Associate Membership of the Institution of Mechanical Engineers.

He joined the Institution as a Graduate in January, 1921.

EDWARD HARDY, Associate Member, died on the 9th February, 1922

Mr. Hardy served his apprenticeship with Messrs. Swan & Hunter. For many years he held the position of foreman plater—at the Blyth Shipbuilding & Dry Docks Co., Ltd., Messrs. R. & W. Hawthorn Leslie's and Messrs. Stephenson's (now Messrs. Palmers) yards at Hebburn, and elsewhere. For three years he was surveyor to the Tank Storage & Carriage Co., Ltd., and for the eight years immediately preceding his demise was a superintendent on the staff of the Anglo-Saxon Petroleum Co., Ltd. In the last mentioned capacity his work was principally on the Tyne, Clyde and Mersey and he was well known in the shipping communities of these districts.

Mr. Hardy joined the Institution in 1907.

GEORGE JONES, Member, died on the 4th July, 1923, in his 77th year.

At the time of his death Mr. Jones was Managing Director of Messrs. William Gray & Co., Ltd., West Hartlepool. He served his apprenticeship with Messrs. Richardson, Duck & Co., of Stockton, of which company he was later Chief Draughtsman. He was connected for some time with the Northern Iron Works, from which company he proceeded to Messrs. William Gray & Co., as Shipyard Manager. He was appointed a Director of this company in 1889, and succeeded the present Sir William Gray in 1898 as Managing Director.

Mr. Jones was for many years President of the Shipbuilding Federation, and he had also served as Chairman of the Tees and Hartlepool Shipbuilders' Association.

Mr. Jones joined the Institution in 1888. From 1902 to 1905 he served as a Member of Council. At the time of his death he was a representative of the Institution on the Consultative Committee to the Marine Department of the Board of Trade.

RAGNVALD KROHN, Graduate, died abroad during the session.

Shortly prior to his death Mr. Krohn had been a student of mechanical engineering at Armstrong College, Newcastle-on-Tyne. He had previously served 16 months in the Machine Works at Nylands Verksted, Christiania, and also in the School of Automobile Engineering. He joined the Institution in December, 1919.

JOHN RAMSAY LUNN, Associate Member, died during 1922. He joined the Institution in 1911. At that time he was employed by Messrs. Havelock and Chaston as a marine surveyor.

ALEXANDER McRAE, Associate Member, died on the 4th May, 1923.

Mr. McRae was educated at Aberdeen Grammar School, after which he served his apprenticeship with Messrs. Hall, Russell, Shipbuilders, Aberdeen. He came to Swan, Hunter & Wigham Richardson's Neptune Works, in August, 1902, as a journeyman draughtsman. He graduated from there to take charge of the Designing Office and ultimately became Chief Draughtsman—which position he held at the time of his death.

During the war the company was fully employed on various types of vessels for the Admiralty, and for his services in connexion with this Mr. McRae was awarded the M.B.E. He had a wide knowledge of his business and will be greatly missed by all who were associated with him. He leaves a wife and one child.

Mr. McRae joined the Institution as a Graduate in 1903 and transferred three years later to Associate Membership.

LEONARD FRANK ORR, Associate, died suddenly while on holiday on the 27th August, 1922, aged 33 years.

Mr. Orr was well known on the North-East Coast as representative for Messrs. Thomas Firth & Sons, Ltd. From 1916-19 he served with the Royal Field Artillery in France. He is survived by Mrs. Orr and one son.

He joined the Institution in 1913.

EDWARD ROBSON, Member, died on the 25th Sepember, 1922, aged 58 years.

Mr. Robson was joint Managing Director of Messrs. Richardson, Duck & Co.'s South Stockton Shipyard, Thornaby-on-Tees. He joined the firm 40 years prior to his death as a junior clerk and reached the status of joint Managing Director entirely through his own abilities and the conscientious employment of these.

In the early stages of the formation of the Tees & Hartlepool Shipbuilders' Association, Mr. Robson undertook the secretaryship. This office he relinquished in 1913. At the time of his death, however, he was Vice-Chairman of the Association. He reached great prominence in the shipbuilding industry through holding the two before-mentioned appointments and was regarded as an authority on all those industrial questions connected with labour; in fact, his loss will be greatly regretted in this connexion both by the shipbuilding companies and the Trade Union leaders, as he was one of those employers responsible for the creation of the machinery which has resulted in the settling of many troublesome issues between shipbuilding employers and shipbuilding workmen by negotiation.

In private life, Mr. Robson took an active part in church work. He also took an interest in the Grange Estate Association, which had the tangible result of the provision of a playing field for the residents on the Grange Estate. Mr. Robson leaves a widow and grown-up son and daughter.

Mr. Robson joined the Institution in 1921 and was a member of the first Committee of the Institution's Middlesbrough Branch.

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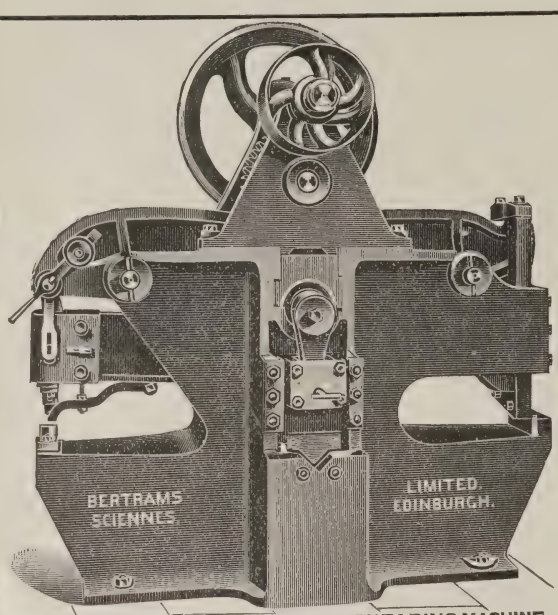

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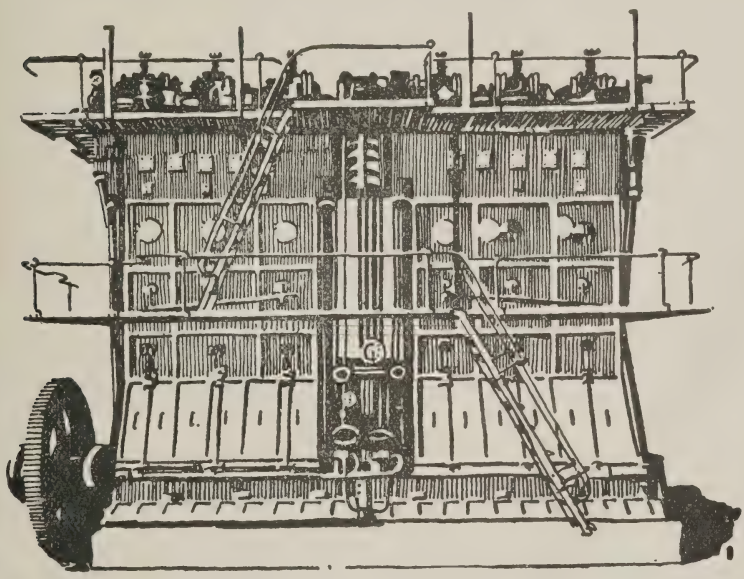

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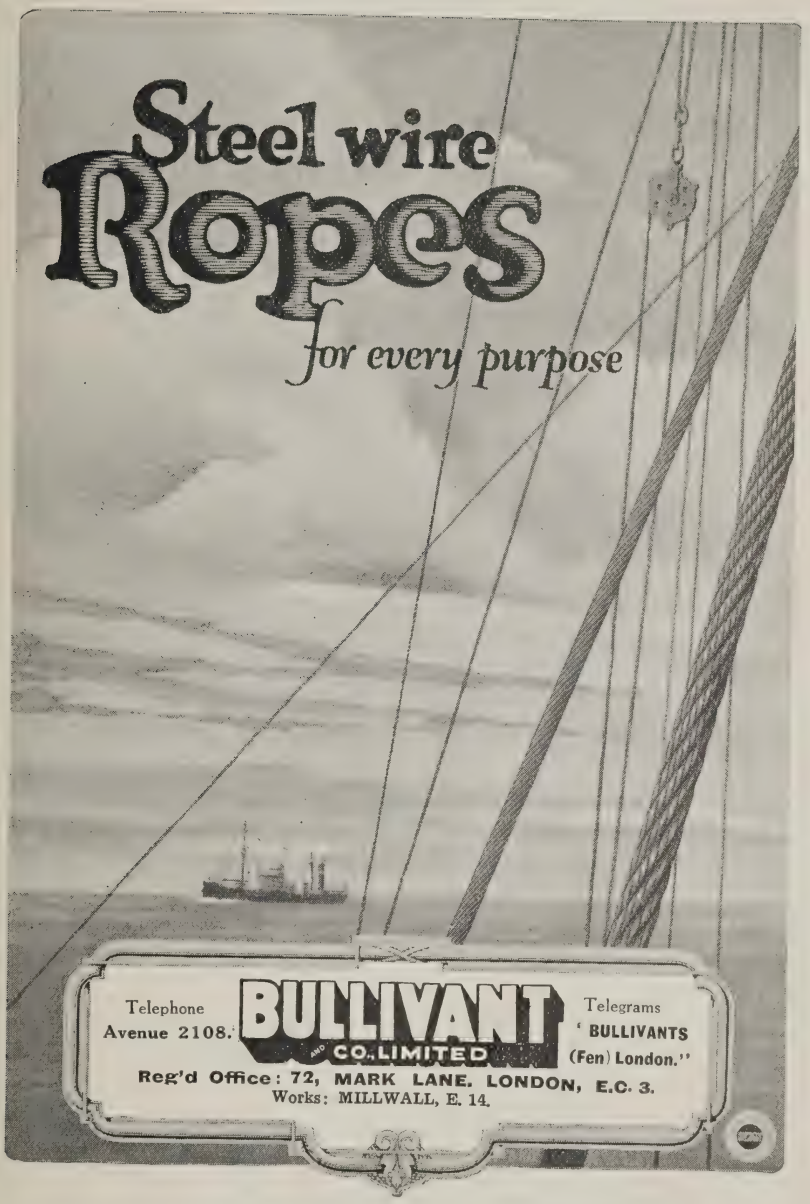
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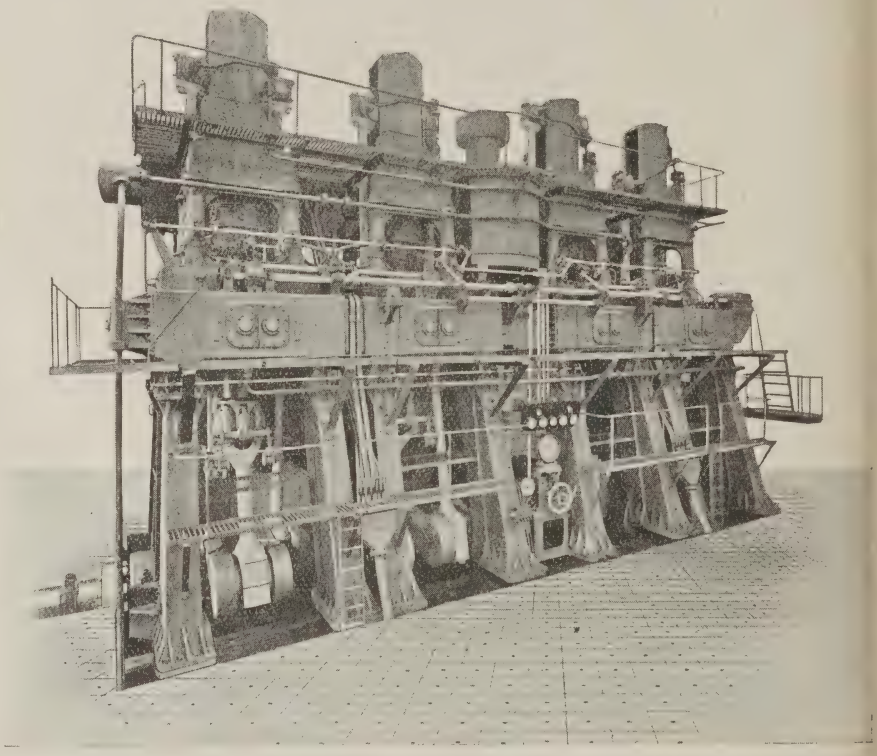

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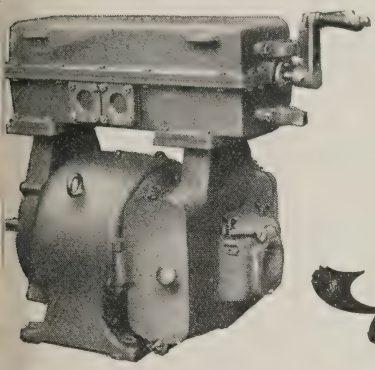

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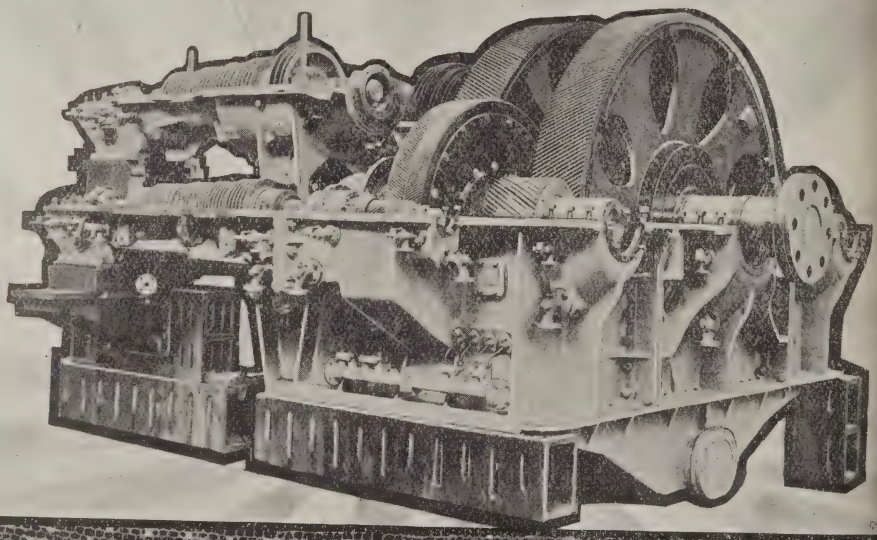

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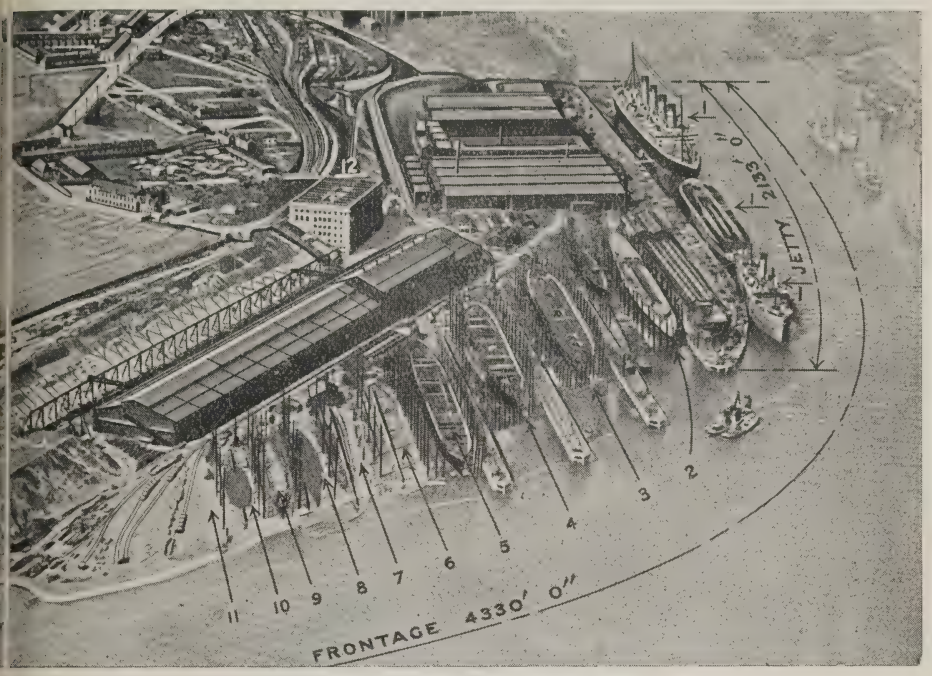

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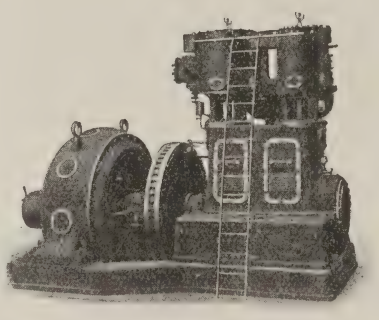

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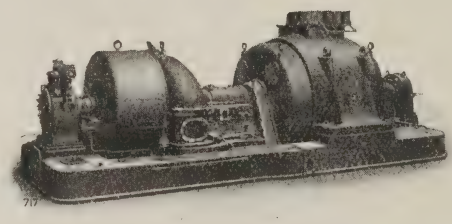

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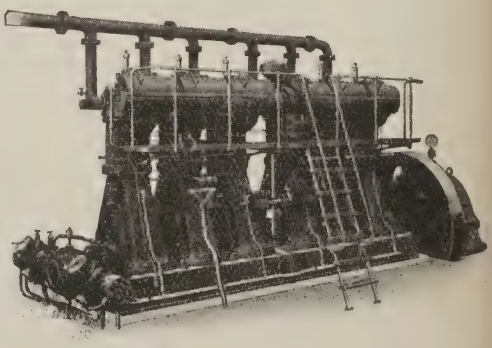

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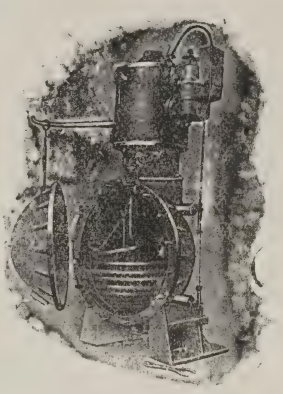

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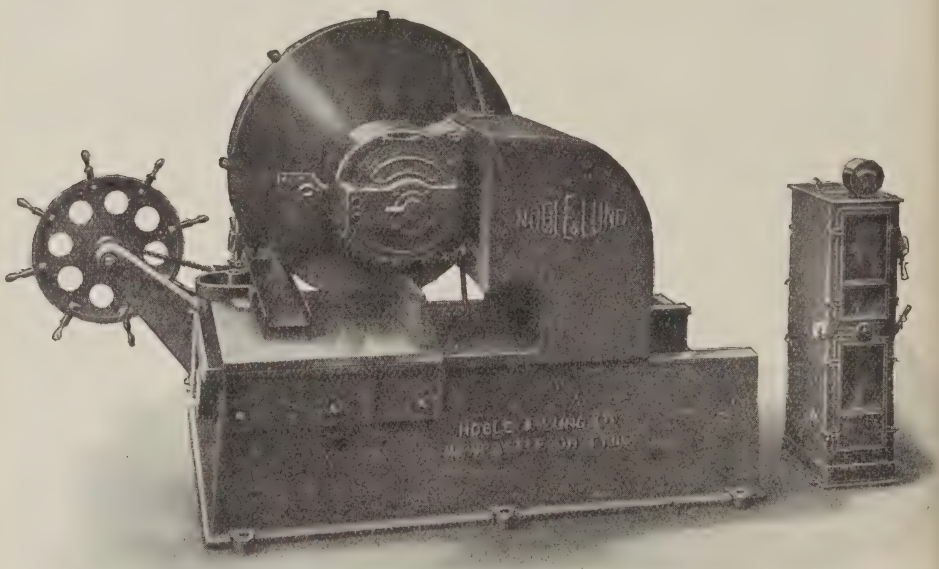

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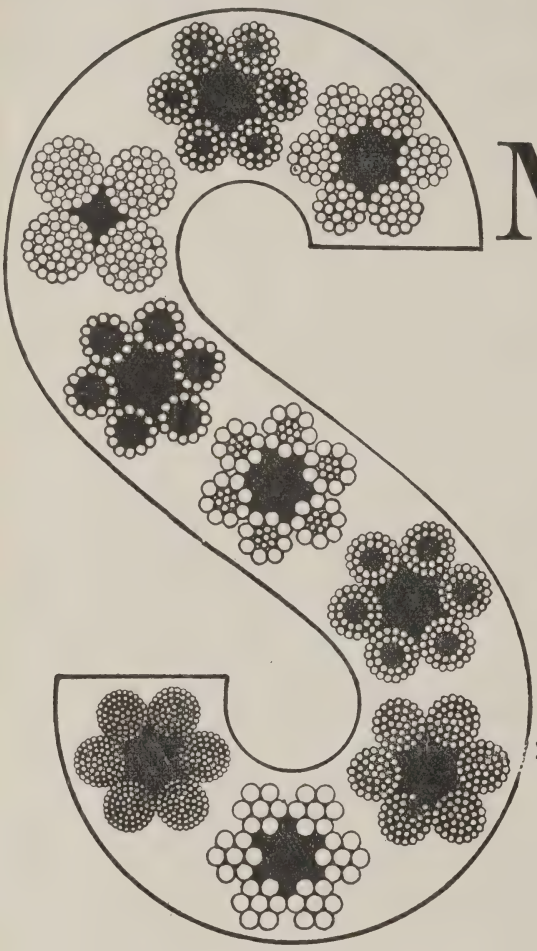

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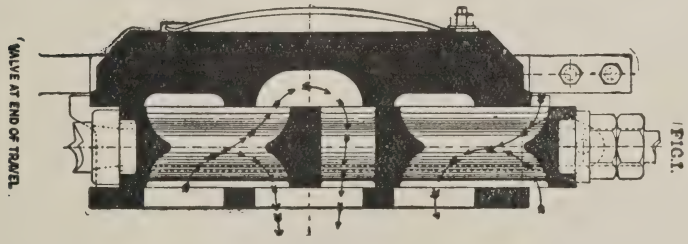

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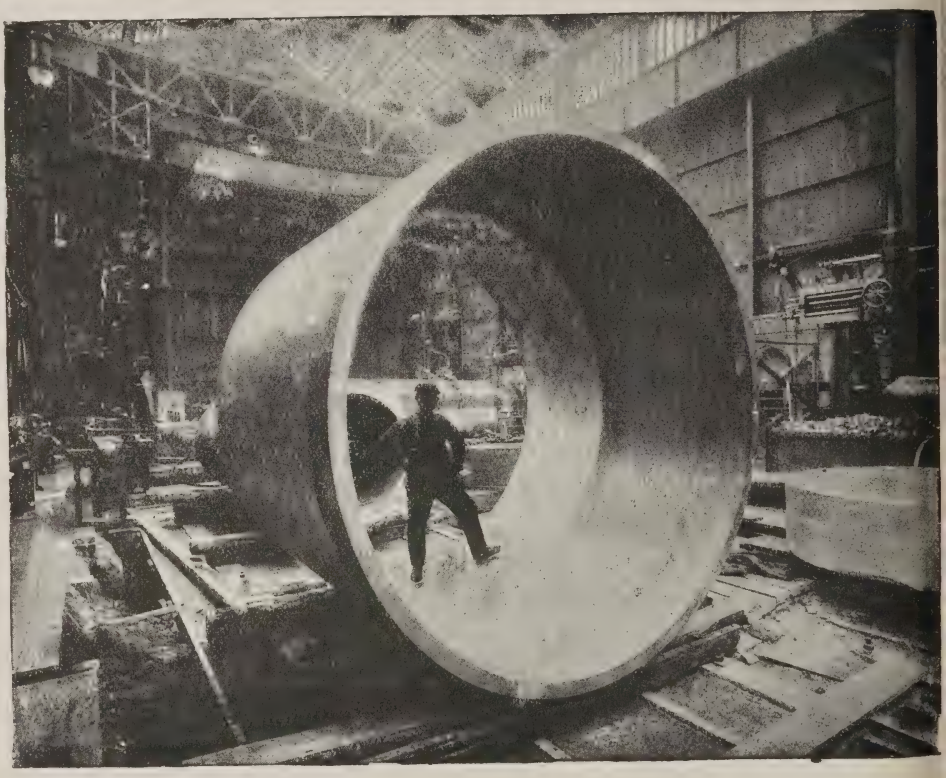

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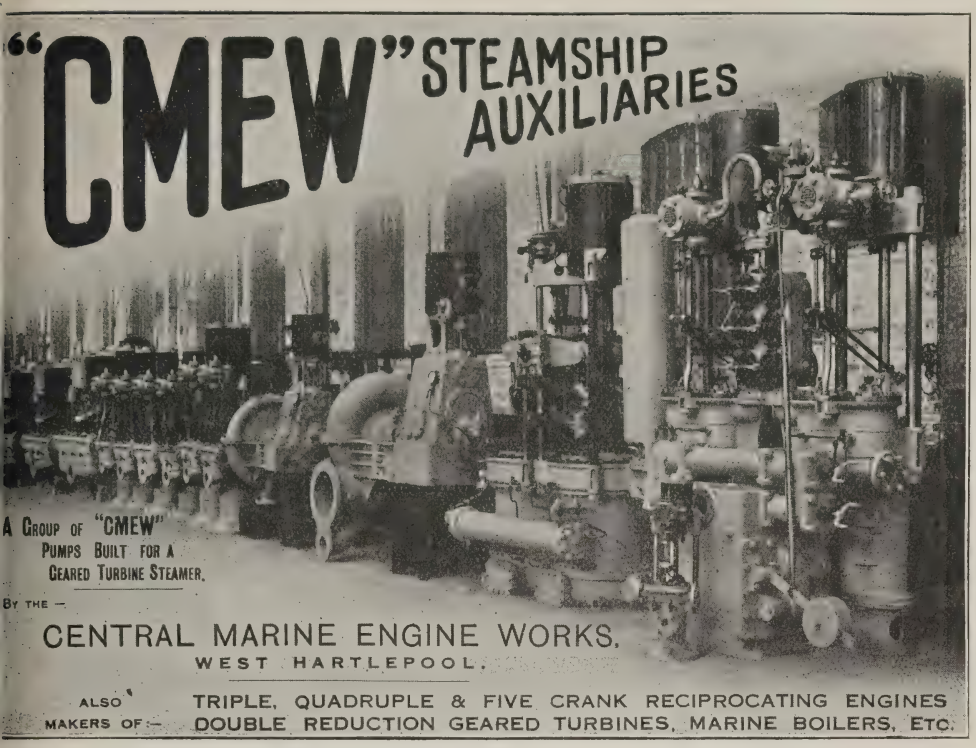

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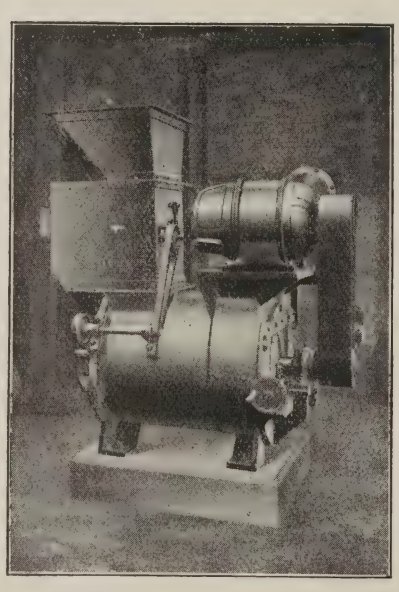

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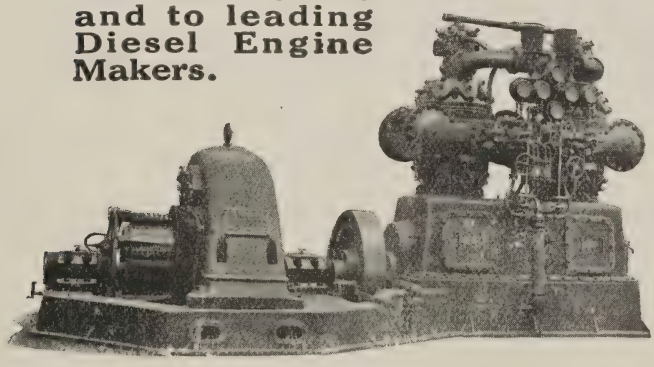

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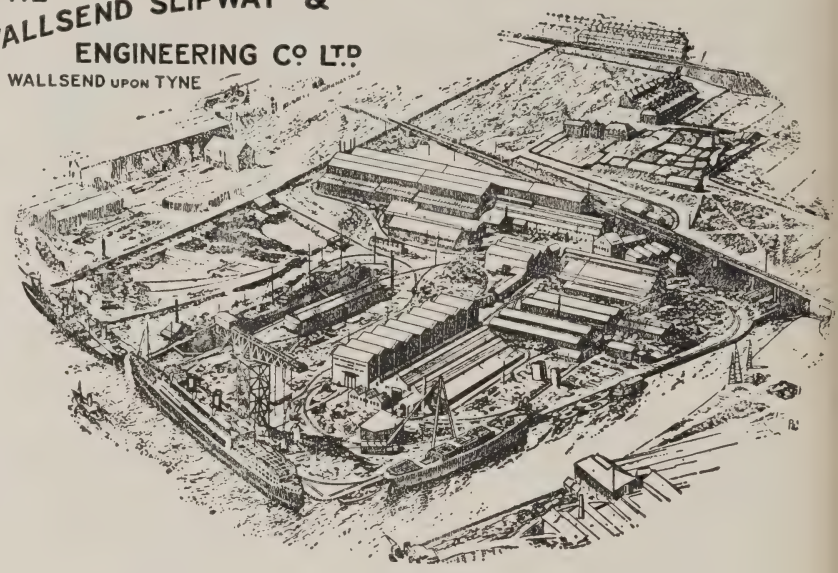

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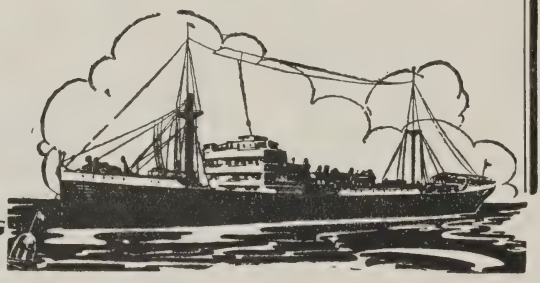

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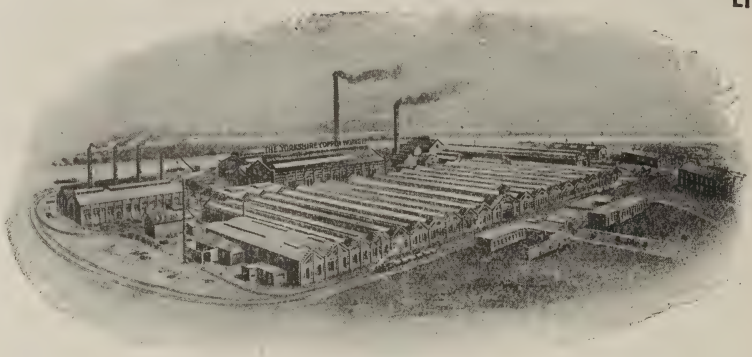

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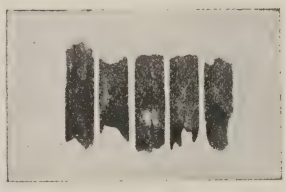

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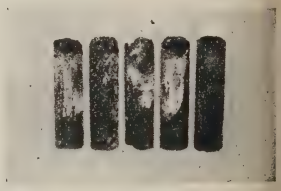

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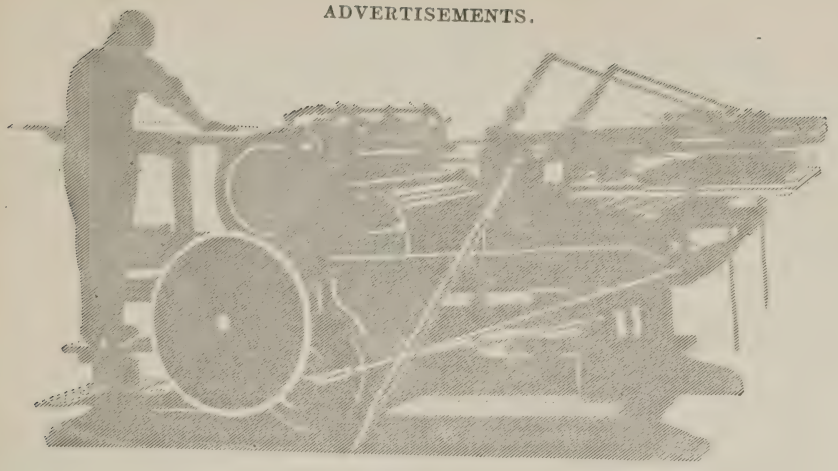

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